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THE
Philoſophical Works

Of the HONOURABLE

ROBERT BOYLE Eſq;

Abridg'd, methodiz'd, and diſpos'd under
the GENERAL HEADS

OF

PHYSICS,
STATICS,
PNEUMATICS,
NATURAL-HISTORY,
CHYMISTRY, and
MEDICINE.

The whole illuſtrated with NOTES, containing the *Improvements* made in the ſeveral *Parts* of natural and experimental *Knowledge*, ſince his Time.

By *PETER SHAW*, M. D.

VOL. II.

The SECOND EDITION, Corrected.

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TO

The RIGHT HONOURABLE

The Earl of *ORRERY*.

MY LORD,

YOUR known parts and learning juſtly entitle your Lordſhip patron to any deſerving production of genius or ſtudy: But the volume here inſcribed to your Lordſhip, has a farther right to your countenance and favour; as not only treating of ſubjects, for a thorough knowledge whereof your Lordſhip is become eminent; but being part of a work originally proceeding from one, whoſe immortal name adds to the dignity of your illuſtrious family.

Mr. *Boyle* ſurvives in his works; but if we would ſee him expreſs'd by real life, we muſt turn our eyes upon your Lordſhip, and your noble family, in whom reign, to perfection, the fine taſte, and the comprehenſive genius; the candid, generous, and communicative temper, ſo eminently predominant in Mr. *Boyle*.

I am,

May it pleaſe your Lordſhip,

Your Lordſhip's moſt humble,

moſt obedient,

and moſt devoted Servant,

Peter Shaw.

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EXPERIMENTS AND OBSERVATIONS UPON COLOURS.

SECT. I.

Diverſity of colours, what it ſignifies.

’TIS often thought that a diverſity of colours conſtantly argues an equal diverſity in the nature of the bodies wherein they reſide: but I cannot wholly give into this opinion. For not to mention changeable taffaties, the blue and golden feathers on the necks of pigeons, and ſeveral water-fowl, natural and artificial rainbows, &c. the colours whereof philoſophers call not real, but apparent; we ſee that the contiguous feathers in various birds are ſome of them red, others white, blue, yellow, &c. and that in ſeveral parts of the ſame feather there is frequently the greateſt diſparity of colours: ſo in tulips, july-flowers, and other vegetables, even ſeveral parts of the very ſame leaf are frequently found of different dyes; tho’ no difference is obſerved in their other properties. And ſuch a variety we have much more remarkably in the marvel of *Peru*; for of the great profuſion of fine flowers which that gaudy plant affords, I have ſcarce obſerved any two dyed perfectly alike. But tho’ ſuch particulars keep me from affirming, that a diverſity of colours always denotes ſome great difference in bodies, yet that it often ſignifies conſiderable alterations in the diſpoſition of their parts, appears from the extraction of tinctures, wherein the change of colour is the chief, and ſometimes the only thing by which the artiſt regulates his procedure in their preparation. Inſtances of this are alſo obvious in ſeveral ſorts of fruit, wherein according as the vegetable ſap is ripened, by paſſing from one degree of maturation to another, the external part of

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the fruit changes from one colour to another. A leſs obvious inſtance of this kind is afforded us by the method of tempering ſteel for gravers, drills, ſprings, *&c.* which is this. Firſt, the ſteel to be tempered is hardened, by heating it in glowing coals, and not quenched as ſoon as taken from the fire, but held over a baſon of water till it deſcend from a white heat to a red one; when 'tis immediately quenched in cold water. The ſteel thus hardened will, if it be good, look whitiſh; and being brighten'd at the end, and held in the flame of a candle, that the bright end may lie about half an inch diſtant from the flame, it will ſwiftly paſs from one colour to another, as from a bright yellow to a deeper and reddiſh yellow, from that to a fainter firſt, and then to a deeper blue; each of which ſucceeding colours argues ſuch a change made in the texture of the ſteel, that if it be taken from the flame, and immediately quenched in tallow, whilſt it is yellow, it will be of ſuch a hardneſs as fits it for drills; *&c.* but if kept for a few minutes longer in the flame, till it turns blue, it becomes much ſofter and proper to make ſprings for watches; which are therefore commonly of that colour; laſtly, if you keep the ſteel in the flame after the deep blue has appeared, it will grow too ſoft even for penknives. Any perſon may eaſily ſatisfy himſelf of the different hardneſs of ſteel of different colours, either by the file, or by breaking ſome ſlender wires thus tempered, and obſerving how they vary in point of brittleneſs.

Colour, what?

But before we deſcend to a more particular conſideration of our ſubject, 'tis proper to obſerve, that colours may be regarded either as a quality reſiding in bodies to modify light after a particular manner, or elſe as light itſelf ſo modified as to ſtrike upon the organ of ſight, and cauſe the ſenſation we call colour; and that this latter is the more proper acceptation of the word colour, will appear hereafter. And indeed it is the light itſelf, which after a certain manner, either mixed with ſhades, or otherwiſe, ſtrikes our eyes, and immediately produces that motion in the organ which gives us the colour of an object. * Yet becauſe there is in the coloured body a certain diſpoſition of the ſuperficial particles to ſend the light reflected or refracted to our eyes, colour may alſo, in ſome ſenſe, be ſaid to depend upon the viſible body; and therefore I ſhall not reject the popular form of ſpeaking of colours, provided we may have recourſe occaſionally to the diſtinction laid down. But ſtill colour is ſo far from being inherent in objects, that light itſelf produces the ſenſation only as it cauſes a determinate kind of local motion in ſome part of the brain; for if the like motion happen from any other cauſe, wherein the light is unconcerned, a man ſhall think he ſees the ſame colour*. Thus it is uſual in dreams to fancy we behold coloured images; and the ſtrange imaginations of diſtracted perſons, with the flaſhings of light, and other appearances, upon ſome diſorders of the brain, farther confirm this particular.

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* The words of Sir *Iſaac Newton* excellently clear and illuſtrate this matter. "The "homogeneal light and rays (ſays that great "philoſopher) which appear red, or rather "make objects appear ſo, I call rubrific, or "red-making; thoſe which make objects "appear yellow, green, blue, and violet, "I call yellow-making, green-making, blue-"making, violet-making, *&c.* And if at "any time I ſpeak of light and rays as co-"loured, or endued with colours, I would "be underſtood to ſpeak not philoſophically "and properly, but groſly, and according "to ſuch conceptions as vulgar people would "be apt to frame. For the rays, to ſpeak "properly, are not coloured. In them there "is nothing elſe than a certain power and "diſpoſition to ſtir up a ſenſation of this or "that colour; for as ſound in a bell, or mu-"ſical ſtring, or other ſounding body, is no-"thing but a trembling motion, and in the "air nothing but that motion propagated "from the object, and in the ſenſorium 'tis "a ſenſe of that motion under the form of "a ſound; ſo colours in the object are no-"thing but a diſpoſition to reflect this or "that ſort of rays more copiouſly than the "reſt: in the rays they are nothing but "their diſpoſitions to propagate this or that "motion into the ſenſorium, and in the "ſenſorium they are ſenſations of thoſe mo-"tions under the forms of colours. *Newton. Optic. p.* 108, 109.

The like effect may alſo be produced from internal cauſes which affect the optic nerve; for I remember in myſelf that, upon coughing with vehemence, ſudden flaſhes of vivid flame have appeared, as it were, before my eyes†. And a lady, to whom I am related, aſſured me, that all the objects of her ſight, once, of a ſudden, appeared of various unuſual colours, ſurprizingly bright and vivid; which ſymptom was, the next day, followed by a fit of the hyſteric diſeaſe. The like ſymptom a phyſician alſo informed me he had obſerved to be a certain prognoſtic of the plague, in a ſeaſon when that diſtemper raged; but that it generally went off after the exhibition of an emetic. As an appearance of colour may, therefore, be produced by internal motions, without the aſſiſtance of an external object; ſo the colour, which would otherwiſe be produced by an outward object, may, poſſibly, be ſometimes changed by a motion or new texture in the organ, as long as that motion or new diſpoſition continues.

Thus I have often obſerved, upon looking at the ſun thro' a teleſcope, darkened ſo as to make the ſplendor of that luminary ſupportable, the impreſſion upon the retina, would neverthelеſs be ſo vivid and permanent, that if afterwards I turned the eye, therein made uſe of, towards a flame, it would appear of a colour very different from its natural one.

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[*] 'Tis certain, that whenever the ſame eye is affected twice alike, viſion will be the ſame in both caſes; but different when the eye is differently affected. And this is a juſt, not an imaginary, foundation for diſtinguiſhing red light from blue, yellow from green, *&c.* for that is properly a red light which excites in us the idea of red, a blue which excites in us the idea of blue, *&c.* Theſe ſeveral colours, therefore differently affect the eye, and conſequently have different diſpoſitions.

[†] To this purpoſe Sir *Iſaac Newton* ſpeaks thus: "When a man in the dark, "ſays he, preſſes either corner of his eye "with his finger, and turns his eye away "from his finger, he will ſee a circle of "colours like thoſe in the feather of a peacock's tail. If the eye and the finger remain quiet, theſe colours vaniſh in a ſecond minute of time; but if the finger "be moved with a quavering motion, they "appear again. Do not theſe colours ariſe "from ſuch motions excited in the bottom "of the eye by the preſſure and motion of "the finger, as at other times are excited "there by light for cauſing viſion? And "do not the motions, once excited, continue about a ſecond of time before they "ceaſe? And when a man by a ſtroke upon "his eye ſees a flaſh of light, are not the "like motions excited in the retina by the "ſtroke?" *Newton. Opt. p.* 321, 322.

And if I ſeveral times ſucceſſively ſhut and opened the ſame eye, this new colour ſeem'd changed or impaired by degrees; till at length the flame appeared to me of the ſame colour it did to other ſpectators. The like effect I have alſo found by looking upon the moon, when near the full, thro' a teleſcope, without employing a coloured glaſs to defend the eye. And here 'tis remarkable, that tho' my right eye, with which I looked thro' the teleſcope, were thus affected by the light, yet if I ſhut that eye, and looked upon the ſame object with the other, it appeared of its uſual colour; but if I again opened and made uſe of the dazzled eye, the vivid adventitious colour would again return. 'Tis further obſervable, that a vehement blow upon the organ of viſion, eſpecially if it be naturally weak, may for a long time vitiate the action of vivid objects. I know a lady of unqueſtionable veracity, who having, by a deſperate fall, received ſeveral hurts, and particularly a conſiderable one near her eye, had her ſight ſo diſordered, that the next morning, when one of her ſervants came to her bed-ſide, his clothes appeared adorned with ſuch a variety of dazzling colours, as preſently obliged her to command him to withdraw; and even the images in her hangings did, for many days after, appear to her, if the room were not greatly darkened, embelliſhed with ſeveral offenſively vivid colours, which nobody elſe could ſee in them. She ſaid alſo, that ſhe ſometimes thought ſhe ſaw colours ſo new and glorious, that they were of a peculiar kind, and ſuch as ſhe could not deſcribe, by their likeneſs, to any ſhe had ſeen either before or ſince; and that white objects greatly diſordered her ſight; that for ſeveral days after her fall, if ſhe looked upon the inſide of a book, ſhe fancied ſhe there ſaw colours like thoſe of a rainbow. And even when ſhe ſeemed pretty well recovered, and left her chamber, happening to come into a place where the walls were white, they appeared to her of ſuch dazzling colours as much offended her ſight. She added, that this diſorder of her eyes laſted for five or ſix weeks; tho' ſince that time ſhe hath been able to read and write conſiderably, without finding the leaſt inconvenience. A man of great learning coming to adviſe with me about a diſtemper in his eyes, told me, that having once looked too attentively upon the ſun thro' a teleſcope, without a dark glaſs, the exceſs of light ſo ſtrongly affected his eye, that ever ſince, when he turns it towards a window, or any white object, he fancies he ſees a globe of light about the ſame bigneſs the ſun then appeared to him; tho' it were now ten years ſince he firſt obſerved it. 'Twere eaſy from ſome remarkable ſymptoms, obſerved by *Epiphanius Ferdinandus*, in perſons bitten by the tarantula, to ſhew, that without any change in the object, an alteration in the inſtrument of viſion may, for a great while, make ſome colours appear delightful, and others diſagreeable, and both to a high degree; tho' they had no ſuch effects before. But theſe already mentioned may ſuffice for our preſent purpoſe.

We before obſerv'd, that colour may, notwithſtanding all this, be conſider'd as a quality reſiding in the body ſaid to be colour'd; and indeed moſt of the following experiments refer to it principally under that notion.

For there is in colour'd bodies, and chiefly in their ſuperficial parts, a certain diſpoſition whereby they diſturb * the light that comes from them to our eye, ſo as to make that diſtinct impreſſion, upon whoſe account we ſay the viſible body is either white or black, red or yellow. But becauſe we ſhall hereafter more fully ſhew, that the changes, and conſequently the production and the appearance of colours often depend upon the continuation or alteration of the texture of the object, we ſhall here previouſly intimate two or three particulars relating to this matter. And firſt, it is not without reaſon, that I aſcribe colour chiefly to the ſuperficial parts of bodies; for, not to examine how plentifully opake corpuſcles may abound even in thoſe bodies we call diaphanous, it is plain, that we ſee little elſe than the ſuperficies of dark objects; for if we found the rays of light reflected from the object, pierc'd deep into the body, we ſhould not judge it opake, but either pellucid or ſemi-diaphanous †.

Whether colours depend upon the ſurfaces of bodies.

The ſchools ſeem to teach, that colour is a penetrating quality, reaching to the innermoſt parts of the object; as if a piece of ſealing-wax be broken into ever ſo many pieces, the internal fragments will be as red as the external ſurface. This is indeed a particular example; but it will not overthrow the foregoing doctrine; eſpecially ſince other examples of a contrary nature may be alledged. And two or three negative inſtances are ſufficient to overthrow the generality of a poſitive rule; at leaſt when that is built but upon one, or a few examples. Not then to mention cherries, plums, *&c.* wherein the ſkin is of one colour, and the inſide of another; I ſhall offer an inſtance or two of the colours of durable bodies, that are thought tolerably homogeneous, and whoſe parts are neither organical, nor of a nature approaching thereto. And firſt, I need only repeat, that blue, red, and yellow may be produced upon a piece of temper'd ſteel: for theſe colours, tho' very vivid, yet if you break the metal they adorn, will appear to be ſuperficial; not only the innermoſt parts of the ſteel, but thoſe within a hair's breadth of its ſuperficies, having none of theſe colours, but retaining that of the metal itſelf. Secondly, we melted a large quantity of pure lead with a ſtrong fire; then immediately pouring it into a clean iron veſſel, and carefully and nimbly taking off the ſcum, we perceiv'd the ſmooth gloſſy ſurface of the melted matter to be adorn'd with a very delightful colour, which almoſt immediately gave place to another vivid one, that was as immediately ſucceeded by a third, and this by a fourth. Thus theſe wonderfully vivid colours ſucceſſively appear'd and vaniſh'd, till the metal cooling, put a ſtop to this pleaſant ſight; tho' the colours which chanced to adorn the ſurface, when the lead began to cool, remain'd upon it, but were ſo ſuperficial, that how little ſoever we ſcraped off from the ſurface of the lead, all the colour came away, and diſcovered only that which is natural to the metal. But unleſs lead be brought to a very high degree of fuſion or fluidity, the phenomena will ſcarce appear. However, the ſame colours did neither always, nor regularly, ſucceed one another, as is uſual in ſteel; but in the following diverſify'd order, which paſſed ſo ſuddenly, that I was ſcarce able to commit it to paper; blue, yellow, purple, blue, green, purple, blue, yellow, red, purple blue, yellow and blue, yellow, blue, purple, green mix'd, yellow, red, blue, green, yellow, red, purple, green. To the ſame purpoſe I might add, that viewing a ſphere of rock cryſtal, which was ſawn aſunder by a lapidary, and expoſing the flat ſurfaces to the ſun beams, the little particles that, notwithſtanding their ſeeming ſmoothneſs in the ſhade, render'd their ſurfaces rough, ſo refracted and reflected the light, as to exceed the vivid colours of the rainbow; but in an interrupted order, ſometimes on one part of the ſurface, ſometimes on another, as it happen'd to be ſituated with regard to the ſun. And having cauſed a fine-grain'd touchſtone to be ſawn aſunder by the ſame artificer, I obſerved upon the new ſurfaces, tho' to the touch they ſeem'd ſmooth and poliſh'd, the vivid colours, as above-mention'd, according as the ſurfaces were put into various poſitions, in reſpect of the ſun and the eye; ſo that, notwithſtanding the great tranſparency of the cryſtal, and the great opacity of the touch-ſtone, their ſuperficial corpuſcles were found fit to exhibit the vivid colours we admire in the rain-bow.

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* It follows from Sir *Iſaac Newton*'s diſcoveries, relating to light and colours, that all the productions and appearances of them in the world are derived not from any phyſical change cauſed in light by refraction or reflection, but only from the various mixtures or ſeparations of rays, by virtue of their different refrangibility or reflexibility. And in this reſpect, ſays that great author, "the "ſcience of colours becomes a ſpeculation "as truly mathematical as any part of optics, ſo far as they depend on the nature "of light, and are not produced or altered "by the power of imagination, or by ſtriking or preſſing the eye." *Newton. Optic. p.* 219.

† Perhaps all opake bodies, when reduced ſufficiently thin or fine, will appear tranſparent, if viewed againſt the light. This evidently happens, when metals are diſſolved in proper menſtrua; when gold is beat thin; when ink aſcends in ſlender glaſs tubes; when an opake ſtone, or other object, is viewed thro' a hole made in the window ſhutter of a dark room, *&c.*

The atomiſts of old, and ſome learned men of late, have attempted to explain the variety of colours in opake bodies from the various figures of their ſuperficial parts. The attempt is indeed ingenious, and the doctrine ſeems partly true; but I confeſs other things appear to me neceſſary to be taken in, as contributing to thoſe different forms of aſperity whereon the colours of opake bodies depend. But in order to prove it, we muſt aſſume, that the ſurfaces of all ſuch bodies, how ſmooth ſoever they may appear to our ſight and touch, are only ſo in a popular, or a phyſical ſenſe. The truth hereof is evident from the uſe of microſcopes, which ſhew us in many bodies, that ſeem ſmooth to our naked eyes, little protuberancies riſing above that which may be conceiv'd to be the plain of the ſurface; as alſo numerous depreſſions beneath that level. And of this ſort of cavities we have, by the help of an excellent magnifier, on the ſurface of a thin piece of cork, that appeared ſmooth to the eye, obſerv'd ſixty in a row, within the compaſs of that glaſs, tho' not above $\frac{1}{32}$ of an inch; and theſe too, which made that little piece of cork look almoſt like an empty honey-comb, were not only very diſtinct, and of ſimilar figures, but conſiderably large, and prodigiouſly deep; ſo that their diſtinct ſhadows and ſides were very conſpicuous, and eaſily to be reckoned, and might have been well diſtinguiſhed, had they been ten times leſs than they were: whence we may make ſome eſtimate, what a ſtrange inequality, and what a multitude of little ſhades there may really be in a ſcarce ſenſible part of a phyſical ſuperficies, tho' they remain inviſible to the naked eye. There are alſo ſeveral experiments which confirm the ſame thing in other like ſubſtances: ſo that every ſenſible part of an opake body, may be conceiv'd to conſiſt of a multitude of corpuſcles ſingly inſenſible. But in giving theſe ſurfaces a diſpoſition to alter the light reflected thence to the eye, after the manner requiſite to make the object appear colour'd, the figures of theſe particles have a great, tho' not the only ſhare. 'Tis true indeed, that the protuberant particles may be of very various figures, ſpherical, elliptical, conical, *&c.* according to the nature whereof, and the ſituation of the lucid body, the light muſt be variouſly affected; as, after one manner from ſurfaces conſiſting of ſpherical, after another from thoſe made up of conical or cylindrical particles; ſome being fitted to reflect more of the incident rays * of light, others leſs; and ſome towards one part, others towards another. But beſides this difference of figure, many other things may greatly concur to vary the forms of aſperity, whereon colours have ſo great a dependance: for allowing the figure of the particles, the ſuperficial ones may be bigger in one body, and leſs in another; and conſequently fitted to allay the light falling on them with greater ſhades. The protuberant particles may alſo be ſet at a greater or a ſmaller diſtance from one another in different parts; and how theſe qualities may ſerve to produce colour, we may gueſs from what happens in the agitation of water; for if the bubbles that are thereby made, be large and few, the water will ſcarce acquire a ſenſible colour; but if it be reduced to a froth, which conſiſts of bubbles very minute, and contiguous to each other, it exhibits a very manifeſt whiteneſs. Beſides, it is not neceſſary that the ſuperficial particles which exhibit one colour, ſhould be all of the ſame ſhape; but different figures may be mixed upon the ſurface of the opake body: as when the corpuſcles that make a blue colour, and thoſe that make a yellow, come to be accurately and ſkilfully united, they afford a green; which tho' it ſeem one ſimple colour, yet in this caſe appears to be made by the union of corpuſcles of very different kinds. Moreover, the figure and magnitude of the little depreſſions, cavities, furrows, or pores, intercepted betwixt theſe protuberant corpuſcles, are as well to be conſider'd, as the ſizes and ſhapes of the corpuſcles themſelves.

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* Sir *Iſaac Newton* proves, that the cauſe of reflexion is not the impinging of light on the ſolid or impervious parts of bodies, as is commonly ſuppoſed; and makes it probable, that the reflexion of a ray is effected not by a ſingle point of the reflecting body, but by ſome power of the body which is evenly diffuſed all over its ſurface, and by which it acts upon the ray, without immediate contact. *Newton. Optic. p.* 237——241.

ſelves. For we may conceive the phyſical ſuperficies of a body to be horizontally cut by a mathematical plain; and then, as ſome parts of that phyſical ſuperficies will be protuberant, or ſwell above the plain, ſo others may be depreſſed beneath it. Thus in many places of the earth's ſurface, there are hills, trees, *&c.* rais'd above the horizontal level of the valley; and rivers, wells, pits, and other cavities, depreſſed beneath it. And that ſuch protuberant and concave parts of a ſurface may remit the lights ſo differently as greatly to vary a colour, we ſhall ſee hereafter. But at preſent it may ſuffice to ſay, that the different degrees or kinds of aſperity in the two flat ſides of the ſame piece of red marble, the one whereof is poliſhed, and the other left rough, will ſo diverſify the light reflected from the ſeveral different plains to the eye, that a painter would employ two different colours to repreſent them. The ſituation alſo of the ſuperficial particles is conſiderable. This I diſtinguiſh into the poſture of the ſingle corpuſcles, in reſpect of the light, and of the eye; and the order of them in regard to one another: for a body may reflect the light otherwiſe, when its ſuperficial particles are more erect upon the plain conceiv'd to paſs along their baſis; and when the extremities of ſuch particles are obverted to the eye, than when thoſe particles lie ſo inclined, that their ſides are in great meaſure diſcernible. Thus the colour of pluſh or velvet will appear various, if you ſtroak part of it one way, and part another; the poſture of the particular threads in regard to the light, or the eye, being thereby varied. And 'tis obſervable, that in a field of ripe corn, blown upon by the wind, there will appear waves of a colour different from that of the reſt of the corn; becauſe the wind, by depreſſing ſome of the ears more than others, cauſes one to reflect more light from the lateral and ſtrawy parts than another. Thus when dogs are ſo enraged, as to erect the hairs upon their necks, and other parts of their bodies, thoſe parts ſeem to acquire a colour different from that of the ſame hairs, when, in their uſual poſture, they lay more inclined. And that the order wherein the ſuperficial corpuſcles are ranged is conſiderable, we may gueſs by the turning of water into froth, the beating of glaſs, and the ſcraping of horn; in which caſes the corpuſcles that were before ſo marſhall'd, as to be tranſparent, become, by a diſturbance of that order, diſpoſed to ſtop and reflect more light, and thereby to appear whitiſh.

There are alſo other ways wherein the order of the protuberant parts, in regard to the eye, may greatly contribute to the appearance of a particular colour: for I have often obſerved, that when peaſe have been planted or ſet in parallel lines, and are ſhot up about half a foot above the ſurface of the ground, by looking on the field where they grew along the lines they ſtood in, much the greater part of the ground appeared of its own dirty colour; but if I view'd the field tranſverſly, it would appear very green; the upper parts of the peaſe concealing the intercepted parts of the ground from the eye. And perhaps even the motion of the ſmall parts of a viſible object may in ſome caſes contribute, tho' it be not eaſy to ſay how, to the production or variation of a colour: for I have ſeveral times made a liquor, which, when well ſettled in a cloſe vial, is tranſparent and colourleſs; but as ſoon as the glaſs is unſtopped, begins to fly away very plentifully in a white, opake fume: and there are other bodies, whoſe fumes, when they fill a receiver, would make one ſuſpect it contain'd milk; and yet theſe fumes become a liquor that is not white, but tranſparent. And ſuch white fumes I have ſeen afforded by unſtopping a liquor, which is itſelf diaphanous and red; nor are theſe the only inſtances of this kind that our experiments can ſupply us with. And if the ſuperficial corpuſcles be of the groſſer ſort, and ſo framed, that their differing ſides may exhibit differing colours, then the motion or reſt of thoſe corpuſcles may be conſiderable, as to the colour of the ſuperficies they compoſe; becauſe ſometimes more, ſometimes fewer of the ſides diſpoſed to exhibit ſuch a colour, may by this means become or continue more obverted to the eye than the reſt, and compoſe a phyſical ſurface more or leſs ſenſibly interrupted. Thus I remember, that in ſome ſorts of plants, whoſe leaves were thick ſet by one another, and the two ſides of each leaf of a ſomewhat different colour, there would appear a notable difference in their colour, if look'd upon, firſt when the leaves being at reſt, had their true upper ſides obverted to the eye; and again, when a breath of wind paſſing thorough them, made great numbers of the ſides of the leaves, that are uſually hidden, become conſpicuous. And tho' the little bodies above-mentioned, may ſingly and a-part ſeem almoſt colourleſs; yet when many of them are placed by one another ſo near, that the eye does not diſcern an interruption in a ſenſible ſpace, they may exhibit a colour: as we ſee, that tho' a ſlender thread of dy'd ſcarlet ſilk, whilſt look'd on ſingly, ſeems almoſt quite devoid of redneſs; yet when numbers of theſe threads are brought together into a skein, that colour then becomes glaring. And in changeable taffaties, where we ſee differing colours ariſe and vaniſh upon ruffling the ſame piece of ſilk; I have often, with pleaſure, obſerved, by means of a convenient microſcope, their component threads to paſs under and over each other, in almoſt innumerable points: and if I thus look'd upon any conſiderable portion of the ſtuff that, for example, appeared red to the naked eye, I could plainly ſee, that in ſuch a poſition the red threads were conſpicuous, and reflected a vivid light: and tho' I could alſo perceive that there were green ones, yet, by reaſon of their diſadvantageous poſition in the phyſical ſurface of the ſilk, they were in part hid by the more protuberant threads of the other colour; and, for the ſame reaſon, the reflection from as much of the green as was diſcover'd, appear'd but dim and faint. And if, on the contrary, I look'd thro' the microſcope upon any part that appear'd green, I could plainly ſee the red threads were leſs fully expoſed to the eye, and obſcured by the green ones, which, therefore, made the predominant colour. And by obſerving the texture of the ſilk, I could eaſily ſo expoſe the threads either of the one colour or the other to my eye, as at pleaſure to exhibit the appearance of red or green, or make thoſe colours ſucceed each other: ſo that, obſerving their ſucceſſion, I could mark how the predominant colour ſtarted up, when the threads that exhibited it, came to be advantageouſly placed; and by making little folds in the taffaty, the ſides that met, and terminated in them, would appear to the naked eye, the one red, and the other green. And when threads of more than two differing colours chance to be interwoven, the reſulting changeableneſs of the taffaty may alſo be ſomewhat different. And perhaps it were proper to try whether the microſcope would give us the reaſon of the variableneſs of colour, that is ſo conſpicuous in mother of pearl, opals, *&c.* 'Tis true indeed, that what I here propoſe concerning the different forms of aſperity in bodies, whence the incident light either comes to be reflected with more or leſs of ſhade, and with that ſhade more or leſs interrupted, or happens to be otherwiſe modify'd, is but conjectural; yet I am apt to ſuſpect, that if we had perfect microſcopes*, we might diſcern in the phyſical ſurfaces of bodies, both a great many latent inequalities, and the particular ſize, figure, and ſituation of the extremely little bodies that cauſe them; and perhaps might perceive, among other varieties, how thoſe little protuberances and cavities interrupt and dilate the light, by mingling with it a multitude of very ſmall ſhades, ſome of them more, and ſome leſs minute; ſome leſs, and ſome more numerous, according to the nature and degree of the particular colour we attribute to the viſible object. Thus with good teleſcopes we can diſcern many hills and vallies in the moon, whereof ſome are more, and others leſs ſtrongly illumin'd; whilſt theſe have a fainter, and thoſe a deeper ſhade: tho' the naked eye can diſcern no ſuch things in that planet. Thus alſo, where the naked eye could only ſee a green powder, a microſcope enabled us to diſcover particular granules, ſome of a blue, and ſome of a yellow colour; tho' we had before cauſed them to be exquiſitely mixed, to compound a green. And here, to confirm what we have delivered, as to the poſſibility of diſcerning the different forms of aſperity in the ſurfaces of bodies of ſeveral colours, I ſhall add a remarkable relation which I had from Dr. *Finch*, anatomiſt extraordinary to the great duke of *Tuſcany*. This gentleman told me, that he ſaw a blind man at *Maeſtricht*, in the *Low Countries*, who at certain times could diſtinguiſh colours by the touch with his fingers. This appear'd ſo extraordinary to me, that I freely propoſed many ſcruples concerning it to the doctor; and particularly inquired, whether care were taken to prevent the man from uſing his ſight in the caſe, if he had any: but, upon the whole, I found the doctor had been very cautious and circumſpect, to prevent being impoſed upon herein. And, that he might not, thro' any miſtake in point of memory, miſinform me, he preſented me the notes he took for his own uſe; the ſubſtance whereof is as follows. The name of the man was *John Vermaaſen*, at that time about thirty-three years of

A blind man, who diſtinguiſhed colours by the touch.

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* Sir *Iſaac Newton* ſeems to be of opinion, that microſcopes may be ſo far improved, if they are not, in ſome degree, already arrived at that perfection, as to diſcover all the particles of bodies on which their colours depend, except thoſe of black ſubſtances, which he ſhews to be the moſt minute. *Newton. Optic. p.* 236.

age, who, when he was two years old, had the ſmall pox, which render'd him abſolutely blind, tho' he is at preſent an organiſt in a public choir. The doctor diſcourſing with him over night, the blind man affirmed, that he could diſtinguiſh colours by feeling, but not unleſs he were faſting; for that any quantity of drink deprived him of that exquiſite touch which is requiſite to ſo nice a ſenſation. Upon this, the doctor provided againſt the next morning ſeven pieces of ribbon of theſe ſeven colours, black, white, red, blue, green, yellow, and grey; but as for mixed colours, this *Vermaaſen* would not undertake to diſcern them; tho', if offer'd, he could tell that they were mixed. To diſcern the colour of the ribbon, he places it betwixt his thumb and his fore-finger, but his moſt exquiſite perception is in his thumb, and much better in the right than in the left. After the man had four or five times told the doctor the ſeveral colours, whilſt a napkin was tied over his eyes, the doctor obſerved he twice miſtook, for he called the white black, and the red blue; but ſtill before his error, he would lay them by in pairs, ſaying, that tho' he could eaſily diſtinguiſh them from all others, yet thoſe two pair were not eaſily diſtinguiſhable from one another. Then the doctor deſired to know what kind of difference he found in colours by his touch. To which the blind man reply'd, that all the difference he obſerved, was a greater or leſs degree of aſperity*; for, ſays he, black feels like the points of needles, or ſome harſh ſand, whilſt red feels very ſmooth; that black and white are the moſt rough, or unequal, of all colours, and ſo like, that 'tis very hard to diſtinguiſh them; but black the rougheſt of the two: that green is next in aſperity; grey next to green; yellow the fifth in degree of aſperity; red and blue ſo alike, that 'tis as hard to diſtinguiſh between them as between black and white, tho' red be ſomewhat more rough than blue; ſo that red has the ſixth place, and blue the ſeventh in aſperity. To this information the doctor was pleaſed to add the preſent of three of thoſe very pieces of ribbon, whoſe colours, in his preſence, *Vermaaſen* had diſtinguiſhed; pronouncing the one grey, the other red, and the third green; which I keep by me as rarities. Before I ſaw the notes from whence this account is taken, I confeſs I ſuſpected this blind man might have thus diſtinguiſhed colours rather by the ſmell than by the touch; for ſome of the ingredients employ'd by dyers, have different and ſtrong ſcents, which a very nice noſe might diſtinguiſh; and this I the rather ſuſpected, becauſe he required that the ribbons he was to judge of, ſhould be offer'd him in the morning faſting: for I have obſerved in ſetting-dogs, that the feeding of them greatly impairs their ſcent. And, indeed, I could wiſh for the opportunity of examining this man myſelf, about many particulars which I do not find to have been yet thought upon. And tho' it be not incredible, that ſince the liquors uſed by dyers are qualified to give a colour, by multitudes of little corpuſcles of the pigment diſſolved and extracted by the fluid wherein they float, thoſe corpuſcles of colour inſinuating themſelves into, and filling up the pores of the body to be dyed, may give a greater or leſs roughneſs to its ſuperficies, according to the bigneſs and texture of the particles of the pigment; yet I can ſcarce believe that this blind man could diſtinguiſh all the colours he did, merely by the ribbons having more or leſs of aſperity: but the forms of it might alſo aſſiſt him herein, tho' theſe would perhaps have been very difficult for him to deſcribe; becauſe thoſe minute differences having not been taken notice of by men for want of a touch ſufficiently exquiſite, are things he could not have expreſſed intelligibly. Thus under the name of ſharp, ſweet, and ſour, there are abundance of peculiar intermediate reliſhes, or taſtes, in different ſorts of wine; which tho' critical and experienc'd palates eaſily diſcern, yet the judges cannot have their idea's convey'd to others; ſuch minute differences not having hitherto any diſtinct names aſſign'd them. And it ſeems that there was ſomething here in the forms of aſperity requiſite to the diſtinction of colours, beſides the degree of it; ſince this man found it ſo difficult to diſtinguiſh black and white from one another, tho' not from other colours. For I might urge, that he ſeems to contradict himſelf about the red, which in one place he repreſents as ſomewhat more rough than the blue, and in another very ſmooth; but becauſe he ſpeaks of this ſmoothneſs in that place where he mentions the roughneſs of black, we preſume that he meant but a comparative ſmoothneſs: however, he found it difficult not only to diſtinguiſh red and blue, but black and white from one another, tho' not from other colours. And indeed, tho' in the ribbons offer'd him, they might be almoſt equally rough, yet in ſuch minute corpuſcles as thoſe of colour, there may eaſily be conceiv'd not only a greater cloſeneſs of parts, a want of protuberant corpuſcles, and little extant particles otherwiſe figured and ranged in the white than in the black; but the cavities alſo may be much deeper in one than in the other. And perhaps, where the particles are ſo exceeding ſlender, we might allow the parts expoſed to the ſight and touch to be a little convex in compariſon of the erect particles of black bodies; as if they were wires many times ſlenderer than a hair, for whether you ſuppoſe them to be figured like needles, or cylindrically like the hairs of a bruſh, with hemiſpherical tops, they will be ſo very ſlender, and conſequently the points both of the one ſort and the other, ſo very ſharp, that an exquiſite touch will be able to obſerve no greater difference between them, than that which our blind man allowed, when, comparing black and white bodies, he ſaid, that the latter were the leſs rough of the two. Nor is every kind of roughneſs, tho' ſenſible, inconſiſtent with whiteneſs, for the phyſical ſuperficies of a body is ſometimes made, by the ſame operation, both rough and white; as when the level ſurface of clear water being agitated and made rough, with a multitude of unequal bubbles, thereby acquires a whiteneſs; and as a ſmooth piece of glaſs by being ſcratched with a diamond, diſcloſes the ſame colour. This minuteneſs and figure of the extant particles being ſuppoſed, the depth of the little cavities intercepted between the extant particles, without being ſo much greater in black bodies than in white ones, as to be very perceptible to the groſs organs of touch, may be vaſtly greater with regard to their diſpoſition of reflecting the ſubtile rays of light. For in black bodies thoſe ſmall intercepted cavities, and other depreſſions, may be ſo figured, ſo narrow and ſo deep, that the incident rays of light, which the more extant parts of the phyſical ſuperficies are diſpoſed to reflect inwards, may be detained there, and prove unable to emerge; whilſt in a white body the ſlender particles may not only, by their figure, be fitted to reflect the light copiouſly outwards, but the intercepted cavities being ſhallow, and perhaps ſomewhat wide, the bottoms of them may be ſo conſtituted, as to be fit to reflect outwards much of the light that falls upon them.

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* The different magnitudes of the parts of coloured bodies, whereon their different colours, according to Sir *Iſaac Newton*'s theory, depend, appear to be too minute for the ſenſe of feeling to take notice of. This ſenſe, indeed, may, by great degrees, be more exquiſite in one man than in another; and, if it could be ſuppoſed equal, or nearly equal to that of ſight, which perhaps is only a finer degree of touch, this different aſperity might, ſuppoſing the truth of the relation, become ſenſible to one, poſſeſſed of an exquiſite ſenſe of feeling. Sir *Iſaac Newton* well obſerves, that there is a harmony and diſcord of colours, which he ſeems to think ariſes from the proportions of the vibrations propagated thro' the fibres of the optic nerve into the brain, as the harmony and diſcord of ſounds ariſe from the proportions of the vibrations of the air. For ſome colours, if they be viewed together, are agreeable to one another, as thoſe of gold and indigo, and others diſagree. See *Newton. Optic. p.* 320.

There is no neceſſity of concluding, from the blind man's relation, that tho', according to his touch, black was the rougheſt, as it is the darkeſt of colours, that therefore white, which is the lighteſt, ſhould alſo be the ſmootheſt; for he makes yellow to be two degrees rougher than blue, and as much ſmoother than green; tho' yellow not only appears to the eye a lighter colour than blue, but we ſhall hereafter ſhew, that yellow reflects much more light than blue, and manifeſtly more than green; which ſeems to ſtrengthen our conjecture, that there was ſomething in the kinds of aſperity, as well as in the degrees of it, that enabled *Vermaaſen* to diſtinguiſh colours; and, at leaſt ſhews, that we cannot, in all caſes, from the bare difference in the degrees of aſperity in colours, ſafely conclude that the rougher reflects the leaſt light. Thus much, however, we gain from the teſtimony of this blind man, that ſince many colours may be felt with the circumſtances above related, the ſurfaces of ſuch coloured bodies muſt certainly have different degrees, and in all probability different forms, or kinds of aſperity belonging to them; which is all the uſe I here deſigned to make of the hiſtory of this blind man: for hence it ſufficiently appears, that colour greatly depends upon the diſpoſition of the ſuperficial parts of bodies, and, probably, wherein ſuch a diſpoſition principally conſiſts*.

But

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* The permanent colours of natural bodies ariſe from hence, that ſome ſuch bodies reflect ſome ſorts of rays, and others other ſorts more copiouſly than the reſt. " Minium, ſays Sir *Iſaac Newton*, reflects the leaſt refrangible, or " red-making rays moſt copiouſly, and " thence appears red. Violets reflect the " moſt refrangible, moſt copiouſly, and " thence have their colour, and ſo of " other

How changes are produced in colours, by liquors.

But to return; the cauſes of the ſeveral forms of aſperity, that may diverſify the ſurfaces of coloured bodies, will, perhaps, afford us ſome general conjectures as to ſeveral of the ways whereby 'tis poſſible to produce the ſudden changes of colours, we ſhall hereafter find in our experiments; for moſt of thoſe phenomena being produced by means of liquors, which generally abound with very minute, active, and variouſly figured ſaline corpuſcles, they may well very ſuddenly alter the texture of the body they are employed to work on; and ſo change their form of aſperity, and thereby make them reflect to the eye the light that falls on them, after another manner than they did before, and by that means vary the colour ſo far as it depends upon the texture or diſpoſition of

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" other bodies. Every body reflects the rays of its own colour more copiouſly than the reſt, and from their exceſs and predominance in the reflected light has its colour. ——— Every body appears moſt ſplendid and luminous when viewed in the light of its own colour. Cinnabar, in the homogeneal, red, priſmatic light is moſt reſplendent, in the green light it is manifeſtly leſs reſplendent, and in the blue light ſtill leſs. Indigo in the violet blue light, is moſt reſplendent, and its ſplendor is gradually diminiſhed, as it is removed thence by degrees thro' the green and yellow light to the red. By a leek the green light, and next that the blue and yellow, which compound green, are more ſtrongly reflected than the other colours, red and violet, *&c.* But to make theſe experiments the more manifeſt, ſuch bodies ought to be choſen as have the fulleſt and moſt vivid colours, and two of thoſe bodies are to be compared together. Thus, for inſtance, if cinnabar and ultramarine blue, or ſome other full blue be held together in the red homogeneal light, they will both appear red, but the cinnabar will appear of a ſtrongly luminous and reſplendent red, and the ultramarine blue, of a faint, obſcure, and dark red; and if they be held together in the blue homogeneal light, they will both appear blue, but the ultramarine will appear of a ſtrongly luminous and reſplendent blue, and the cinnabar of a faint and dark blue; which puts it paſt diſpute, that the cinnabar reflects the red light much more copiouſly than the ultramarine doth, and that the ultramarine reflects the blue light much more copiouſly than the cinnabar doth. The ſame experiment may be made ſucceſsfully with red lead and indigo, or with any other two coloured bodies, if due allowance be made for the different ſtrength or weakneſs of their colour and light. And ſince theſe reflected lights, which differ in colour, differ alſo in degree of refrangibility, it's certain that ſome bodies reflect the more refrangible, and others the leſs refrangible rays more copiouſly; and that this is the only reaſon of theſe colours, appears from hence, that the colour of homogeneal light cannot be changed by the reflexion of natural bodies. For if bodies, by reflexion, cannot in the leaſt change the colour of any ſort of rays, they cannot appear coloured by any other means, than by reflecting thoſe which are either of their own colour, or which, by mixture, muſt produce it. But in making experiments of this kind, care muſt be had that the light be ſufficiently homogeneal." *Newton. Optic. p.* 156,——158.

" In tranſparently coloured liquors, 'tis obſervable, that their colour uſes to vary with their thickneſs. Thus a red liquor, in a conical glaſs, held between the light and the eye, looks of a pale and dilute yellow at the bottom where 'tis thin, and a little higher where 'tis thicker, grows orange; and where 'tis ſtill thicker, becomes red; and where 'tis thickeſt, the red is deepeſt and darkeſt. For it is to be conceived, that ſuch a liquor ſtops the indigo-making and violet-making rays " moſt of the viſible parts of the object; for I do not abſolutely exclude all other ways of modifying the rays of light, between their parting from the lucid body, and their reception into the common organ of viſion. Now there ſeem to be ſeveral ways whereby liquors may ſuddenly alter the colour of one another, but I ſhall only touch upon a few. And, firſt, the minute corpuſcles that compoſe a liquor may eaſily inſinuate themſelves into thoſe pores of bodies whereto their ſize and figure adapt them; and theſe pores they may either exactly or inadequately fill; in which latter caſe they will, for the moſt part, alter the number and figure, but always the

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“ moſt eaſily, the blue-making rays more difficultly, and the red-making moſt difficultly; and that if the thickneſs of the liquor be only ſo much as ſuffices to ſtop a competent number of the violet-making and indigo-making rays, without diminiſhing much the number of the reſt, the reſt muſt compound a pale yellow. But if the liquor be ſo much thicker, as to ſtop alſo a great number of the blue-making rays, and ſome of the green-making, the reſt muſt compound an orange; and where it is ſo thick as to ſtop alſo a great number of the green-making, and a conſiderable number of the yellow-making, the reſt muſt begin to compound a red, and this red muſt grow deeper and darker, as the yellow-making and orange-making rays are more and more ſtopped by the increaſing thickneſs of the liquor; ſo that few rays beſides the red-making can get through.

“ Of this kind is an experiment lately related to me by Dr. *Halley*, who, diving deep into the ſea in a diving veſſel, found, in a clear ſunſhine day, that when he was ſunk many fathoms deep into the water, the upper part of his hand, on which the ſun ſhone directly through the water, and through a ſmall glaſs-window in the veſſel, like that of a damaſk roſe, and the water below and the under part of his hand illuminated by light reflected from the water below, looked green. From thence it may be gathered, that the ſea-water reflects back the violet and blue-making rays moſt eaſily, and lets the red-making paſs moſt freely and copiouſly to great depths; for thereby the ſun's direct light, at all great depths, by reaſon of the predominating red-making rays muſt appear red, and the greater the depth is, the fuller and intenſer muſt that red be. And at ſuch depths as the violet-making rays can ſcarce penetrate to, the blue-making, green-making and yellow-making rays being reflected from below more copiouſly than the red-making ones, muſt compound a green. Now if there be two liquors of full colours, ſuppoſe a red and a blue, and both of them ſo thick as ſuffices to make their colours ſufficiently full, though either liquor be ſufficiently tranſparent a-part, yet will you not be able to ſee through both together; for if only the red-making rays paſs through one liquor, and only the blue-making through the other, no rays can paſs through both. This Dr. *Hook* tried caſually with glaſs wedges, filled with red and blue liquors, and was ſurprized at the event.

“ Now whilſt bodies become coloured, by reflecting or tranſmitting this or that ſort of rays more copiouſly than the reſt, it is to be conceived that they ſtop and ſtifle in themſelves the rays which they do not reflect or tranſmit. For if gold be foliated, and held between the eye and the light, the light looks of a greeniſh blue; and therefore maſſy gold lets into its body the blue-making rays, to be reflected to and fro within it, till they be ſtopped and ſtifled, whilſt it reflects the yellow-making outwards, and thereby looks yellow. And much after the ſame manner that leaf-gold is yellow by reflected, and blue by tranſmitted light, and maſſy gold is yellow in all poſitions of the eye; there are ſome liquors, as the tincture of *Lignum Nephriticum*, and ſome ſorts of glaſs, which tranſmit one ſort of light moſt copiouſly,

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the magnitude of the former pores. And in what capacity ſoever theſe corpuſcles of a liquor come to be lodged or harboured in ſuch pores, the ſurface of the body will commonly have its aſperity altered, and the incident light, that meets with a groſſer liquor in the little cavities that before contained nothing but air, or ſome more ſubtile fluid, will have its rays either refracted, abſorbed, or reflected more or leſs irregularly than if the body had been dry. Thus we ſee that even fair water falling on white paper, linen, *&c.* will, for ſome ſuch reaſon as thoſe aſſigned, immediately alter the colour of them, and for the moſt part render it deeper than that of the unmoiſten'd parts of the ſame bodies. In like manner, when, during the ſummer, the high-ways are dry and duſty, if there fall much rain, they quickly appear of a darker colour than before. Thus alſo if a drop of oil be let fall upon white paper, that part of the paper which by imbibing the liquor acquires a greater continuity, and ſome tranſparency, will appear much darker than the reſt; many of the incident rays of light being now tranſmitted, that would, otherwiſe, be reflected to the eye.

Secondly, a liquor may alter the colour of a body, by freeing it from thoſe things that prevent its appearing of its genuine colour; and tho' this may be ſaid to be rather a reſtoration of a body, or a reſtitution of

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" copiouſly, and reflect another ſort, " and thereby look of ſeveral colours, " according to the poſition of the eye to " the light. But if theſe liquors or " glaſſes were ſo thick and maſſy, that " no light could get through them, I que-" ſtion not but they would, like all other " opake bodies, appear of one and the " ſame colour in all poſitions of the eye. " For all coloured bodies, ſo far as my " obſervation reaches, may be ſeen through, " if made ſufficiently thin, and therefore " are in ſome meaſure tranſparent, and dif-" fer only in degree of tranſparency from " tinged and tranſparent liquors; theſe li-" quors, as well as thoſe bodies, by a ſuffi-" cient thickneſs becoming opake. A tranſ-" parent body, that looks of any colour " by tranſmitted light, may alſo look of " the ſame colour by reflected light, the " light of that colour being reflected by " the farther ſurface of the body, or by " the air beyond it. And then the re-" flected colour will be diminiſhed, and " perhaps ceaſe, by making the body ve-" ry thick, and pitching it on the back-" ſide, to diminiſh the reflexion of its " farther ſurface, ſo that the light re-" flected from the tinging particles may " predominate. In ſuch caſes the colour " of the reflected light will be apt to va-" ry from that of the light tranſmitted." *Newton. Optic. p.* 159—163.

This doctrine is farther confirmed, by mixing coloured light, ſo as to compound a beam of light of the ſame colour and nature with a beam of the ſun's direct light, and making experiments therein. Thus Sir *Iſaac Newton* found, that ſuch light is endued with all the properties of a beam of the ſun's light; natural bodies appearing in it of the ſame Colours they have in day-light. Thus cinnabar appeared of the ſame red colour in it as it does by day, and if the green-making and blue-making rays were ſtopped, its redneſs became more full and lively; but if the red-making rays were intercepted, it became yellow or green, or of ſome other colour, according to the ſorts of rays which were not intercepted. So gold in this light appears of the ſame yellow colour as in day-light; but by ſtopping a due quantity of the yellow-making rays, it appears white like ſilver; which ſhews, that its yellowneſs ariſes from the exceſs of the intercepted rays tinging that whiteneſs with their colours when they are let paſs. See *Newton. Optic. p.* 163—167.

its native colour, than a change; yet there here happens a change of the colour which the body afforded before this operation. And ſuch a change a liquor may work, either by diſſolving or corroding, or by ſome ſuch means carrying off the matter, which either veiled or diſguiſed the colour that afterwards appears. Thus we reſtore old pieces of dirty gold to a clear ſhining yellow, by putting them into the fire, and into *Aqua fortis*, which takes off the acquired filth from the metal; and there is alſo an eaſy way to reſtore ſilver coins to their due luſtre, by fetching off that which diſcoloured them. I have likewiſe a chymical liquor, which I employ to reſtore pieces of cloth ſpotted with greaſe, to their proper colour, by imbuing the ſpotted part therewith; which incorporating with the greaſe, and yet being of a very volatile nature, eaſily carries it away with itſelf. And I have ſometimes try'd, that by rubbing upon a good touchſtone a certain metalline mixture ſo compounded, that the impreſſion it left upon the ſtone appeared of a very different colour from that of gold; yet a little *Aqua fortis* would immediately make the golden colour diſcloſe itſelf, by diſſolving only the other metalline corpuſcles that concealed thoſe of the gold.

Thirdly, a liquor may alter the colour of a body, by comminuting its parts; and principally after two ways: Firſt, by disjoining and diſſipating thoſe cluſters of particles which ſtick more looſely together, only by means of ſome cement that is eaſily diſſoluble; and this ſeems to be the caſe in ſome of the following experiments, where the colour of many corpuſcles brought to cohere, by being precipitated together, is deſtroyed by the affuſion of very ſharp and piercing liquors. Secondly, By dividing the groſſer and more ſolid particles into minute ones, which will be, for the moſt part, otherwiſe ſhaped than the entire corpuſcles ſo divided; as happens in a piece of wood reduced to ſplinters or chips, or when a piece of cryſtal heated red-hot, and quenched in cold water, cracks into a multitude of little fragments, which, tho' they do not fall aſunder, alter the diſpoſition of the body of it, as to its manner of reflecting the light.

There is a fourth way oppoſite to the third, whereby a liquor may change the colour of another body, eſpecially a fluid, by procuring the coalition of ſeveral particles that before lay ſcattered, and too remote from each other to exhibit the colour that afterwards appears. Thus, when a ſolution of gold has been ſo diluted, that I queſtioned whether the liquor really imbibed any of the metal, I have been quickly ſatisfied it did, by pouring a little mercury into it; for the gold therein would ſoon clothe the ſurface of the quickſilver with a thin film of its own livery. And by this way of bringing the minute parts of bodies together, in ſuch numbers as to make them a fit object for the eye, many of thoſe colours ſeem to be generated which are produced by precipitation; eſpecially if made with fair water; as when reſinous gums, diſſolved in ſpirit of wine, are let fall again, if the ſpirit be greatly diluted with that weakening liquor. And thus out of the rectified and tranſparent butter of antimony, there will, by the bare mixture of fair water, be plentifully precipitated that milk-white ſubſtance, which, by having its looſer ſalts well waſhed off, is turned into *Mercurius vitæ*.

A fifth way wherein a liquor may change the colour of a body, is by diſlocating its parts, and changing their order; at the ſame time, perhaps, alſo altering the poſition of the ſingle corpuſcles, as well as their ſituation in reſpect of one another. What ſhare particular commotions or diſlocations of the parts of a body may have in changing its colour, is evident from the mutations of that quality obſervable in quickſilver, and ſome other concretes, long kept by chymiſts in a convenient heat, tho' in cloſe veſſels; and alſo in the obvious degenerations thereof in bruiſed cherries, and other fruit: and that ſuch liquors as we have been ſpeaking of, may greatly diſcompoſe the textures of many bodies, and thereby alter the diſpoſition of their ſuperficial parts, the great commotion made in metals and other ſubſtances by *Aqua fortis*, oil of vitriol, *&c.* eaſily perſuade us. And what ſuch a varied ſituation of parts may do towards diverſifying the manner wherein they reflect the light, may be gueſſed, in ſome meaſure, by the pulverization of tranſparent glaſs, which thereby becomes a white powder; but ſtill better, by the experiments we ſhall hereafter ſet down, with relation to the production and deſtruction of colours by ſubtile, ſaline liquors. And in ſome chymical oils, as particularly in that of lemon-peel, the tranſpoſition of the parts, conſequent upon bare concuſſion, will repreſent on the ſurfaces of the bubbles thereby occaſioned, exceeding lively colours, which, when the bubbles relapſe into the reſt of the oil, immediately vaniſh. I know not whether I ſhould mention, as a diſtinct method, that power whereby a liquor may alter the colour of another body, by putting the parts of it into motion; for tho', poſſibly, the motion ſo produced, ſeldom, of a ſudden, changes the colour of the body whoſe parts are agitated; yet this ſeems to be one of the moſt general, tho' not immediate cauſes of the quick change of colours in bodies; for the parts being put into motion by the adventitious liquor, many of them, that were before united, may be thereby disjoined; and when that motion ceaſes or decreaſes, others of them may ſtick together in a new order: by which means the motion may ſometimes produce permanent changes of colours; as in the experiment we ſhall hereafter mention, where a ſnow-white body is immediately turned into a yellow, by the bare affuſion of fair water. And tho' when you rub good blue vitriol upon the clean blade of a knife, it will not impart its latent colour thereto; yet if you moiſten that ſubſtance with common water, the particles of the liquor disjoining thoſe of the ſtone, and thereby giving them the various agitation requiſite to fluid bodies, the metalline corpuſcles of the vitriol thus diſſolved will lodge themſelves in throngs in the ſmall pores of the iron, and give the ſurface of it the genuine colour of copper.

Laſtly, The moſt important way, by which a liquor may alter the colour of another body, is by aſſociating the ſaline, or other more rigid corpuſcles of the liquor, with the particles of the body whereon it is employ'd to work. For theſe adventitious corpuſcles uniting with the protuberant particles of the ſurface of a coloured ſubſtance, muſt neceſſarily alter their bigneſs, and moſt commonly their ſhape. And that the colours of bodies greatly depend upon the bulk and figure of their ſuperficial particles, ſeems probable, ſince many ancient and modern philoſophers have thought, that all colours might, in the general, be accounted for from them alone. The diverſification hereof will, in our caſe, be attended with theſe two circumſtances; the one, that the protuberant particles being increaſed in bulk, muſt often be varied as to the cloſeneſs or laxity of their order; fewer of them being contained within the ſame ſenſible ſpace than before; or elſe, by approaching one another, they muſt ſtreighten the pores; and perhaps too by the manner of their aſſociating themſelves with the protuberant particles, intercept new pores. And thus, alſo, theſe adventitious corpuſcles may produce a great change, as well in the little cavities, as in the protuberances of a coloured body; for they may likewiſe, by lodging themſelves in thoſe little cavities, fill them up; and it may eaſily happen, that they ſhall not only fill the pores they inſinuate themſelves into, but have their upper parts extant above them: and partly by ſuch new protuberances, partly by increaſing the bulk of the former, theſe extraneous corpuſcles may much alter the number and bigneſs of the ſuperficial pores, changing the old ones, and intercepting new; and very probably the order of the little extancies, and conſequently that of the little depreſſions, in point of ſituation will be altered likewiſe. Thus if you diſſolve quickſilver in ſome kinds of *Aqua fortis*, the ſaline particles of the menſtruum aſſociating themſelves with the mercurial globules, will make a green ſolution, which afterwards eaſily degenerates; and red lead, diſſolved in ſpirit of vinegar, yields a clear ſolution, the redneſs of the lead being deſtroyed by the liquor. But a better inſtance is afforded from copper; for I have try'd, that if upon a copper plate you let ſome drops of weak *Aqua fortis* reſt for a while, the corpuſcles of the menſtruum joining with thoſe of the metal, will produce a very ſenſible aſperity upon the ſurface of the plate, and thence coagulate into very minute grains of a pale blue vitriol; tho', if upon another part of the ſame plate you ſuffer a little ſtrong ſpirit of urine to remain for a competent time, the rough ſurface will be adorned with a deeper and richer blue. And the ſame *Aqua fortis*, that ſuddenly changes the redneſs of minium into a darker colour, will, being put upon crude lead, produce a whitiſh ſubſtance, as with copper it did a bluiſh, and as with iron it gives a reddiſh, and on white quills a yellowiſh colour: ſo much may the coalition of the parts of the ſame liquor, with the differently figured particles of ſolid bodies ſeveral ways turn the differingly diſpoſed ſurfaces rough, and diverſify the colour of thoſe bodies. And 'twill be eaſily believed, that in many changes of colour, which happen upon the diſſolutions of metals, and precipitations made with oil of tartar, *&c.* a coalition of ſaline corpuſcles may be made with the particles of the body diſſolved or precipitated; if we conſider how much the vitriol of a metal may be heavier than the pure metalline part thereof, upon account of the ſaline parts coagulated therewith; and that in ſeveral precipitations the weight of the calx, for the ſame reaſon, much exceeds that of the metal when it was firſt expoſed to be diſſolved.

But I am not aiming to eſtabliſh a particular theory of colours, only deſign to offer ſome experiments and obſervations on the ſubject of them. I ſhall, therefore, add but two particulars more, with relation to the forms of ſuperficial aſperity in coloured bodies. And firſt, I ſay, that there are many other means whereby true and permanent colours may be produced in bodies, beſides thoſe practicable by the help of liquors; for proof whereof ſeveral examples might be alledged; but I need only refer to what we have already obſerved of the change of colours ſuddenly made on tempered ſteel and lead, by the operation of heat, without the interpoſition of a liquor. Secondly, I obſerve, what is of more importance to our preſent ſubject, that though nature and art may, in ſome caſes, ſo change the aſperity of the ſuperficial parts of a body, as to alter its colour by any of the mentioned ways a-part, yet 'tis generally by two or three, or perhaps by more of them together, that the effect is produced; and if it be conſidered how variouſly thoſe ſeveral ways, with others of the like nature, may be compounded and applied, it will not appear ſurprizing, that ſuch fruitful principles ſhould be fitted to change or generate many differing colours.

Whether all bodies are tranſparent?

Hitherto we have conſidered the little protuberances, or other ſuperficial particles which conſtitute the roughneſs of bodies, as if we took it for granted that they muſt be perfectly opake, and impenetrable to the rays of light, and ſo contribute to the variety of colours, as they ſtop more or leſs thereof, and reflect them to the eye particularly mixed with more or leſs of ſhade. I have often thought it worth a ſerious enquiry, whether or no particles of matter, each of them ſingly inſenſible, may not yet ſeparately conſiſt of many minuter particles, betwixt which we may conceive little junctures, where they adhere to one another, in ſome degree pervious to the prodigiouſly ſubtile corpuſcles of the rays of light; whence conſequently thoſe particles would, in ſuch a degree, be diaphanous. For as perfectly opake bodies can only reflect the incident rays of light, thoſe that are diaphanous refract * them too; and that refraction has a great hand in the production of colours, appears from light paſſing thro' drops of water, which exhibits a rain-bow; thro' priſmatic glaſſes, and many other tranſparent bodies. And I find, that in a darkned room, where the light is permitted to enter but at one hole, the little floating particles of duſt, commonly called motes, being viewed by an eye placed on one ſide of the beam of light, appear in certain poſitions adorned with very vivid colours, like thoſe of the rain-bow, or rather like thoſe of very minute ſparkling fragments of diamonds; and that as ſoon as their motion has brought them to an inconvenient poſition in regard to the light and the eye, they are only viſible, without affording any lively colours: which ſeems to argue, that this duſt, or minute fragments of ſeveral ſorts of bodies reputed opake, and only crumbled into duſt, do not barely reflect the rays that fall upon them, but reflect them to the eye refracted.

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* Refrangibility of the rays of light is their diſpoſition to be turned out of their way, in paſſing out of one tranſparent body into another; reflexibility, their diſpoſition to be turned back into the ſame medium from the ſurface of any other. Sir *Iſaac Newton* firſt ſhewed the preciſe difference between them, in the *Philoſophical Tranſactions*, N°. 80. *A. D.* 1671. and in ſeveral ſubſequent pieces which occur in the ſame *Tranſactions*, ſo far vindicated his diſcovery from ſeveral objections made againſt it, that his opponents, and particularly F. *Pardies*, ingenuouſly acknowledged themſelves ſatisfied with it. At length Sir *Iſaac* publiſhed his doctrine more fully, in his excellent treatiſe of the *reflections, refractions, inflections, and colours of light*; wherein he confirms it by numerous experiments, made with the utmoſt ſimplicity and plainneſs. Herein he ſhews that the colours of light produced by the priſm, and alſo thoſe reflected from opake bodies,

We may alſo obſerve, that ſeveral bodies, which paſs for opake, appear in great meaſure tranſparent, when reduced into thin parts, and held againſt a ſtrong light. This I have not only taken notice of in pieces of ivory reduced to thickiſh leaves, in many conſiderable thick ſhells of fiſh, and in ſhavings of wood; but have alſo found, that a piece of very thick deal purpoſely interpos'd betwixt my eye and the clear day-light, appear'd quite thorough of a lovely red. And in the darkned room above-mentioned, bodies held againſt the hole at which the light enter'd, appear'd far leſs opake than they would elſewhere have done: ſo that I could eaſily and plainly ſee, thro' the whole thickneſs of my hand, the motions of a body placed at a very ſmall diſtance beyond it. And even in minerals, the opacity is not always ſo exceeding great, if the body be made thin; for white marble, though of a pretty thickneſs, being within a due diſtance placed betwixt the eye and a convenient light, will ſuffer the motions of one's finger to be well diſcern'd through it; and ſo will thick pieces of many common flints. But, above all, that inſtance afforded us by *Muſcovy* glaſs is remarkable; for tho' plates of this mineral of a moderate thickneſs often appear opake, yet if one of theſe be dextrouſly ſplit into the thinneſt leaves 'tis made up of, it will yield ſuch a number of them, as ſcarce any thing but experience could have induced me to believe. Theſe leaves afford the moſt tranſparent ſort of ſolid bodies that, for ought I have obſerved, are yet known: and a ſingle plate of it will be ſo

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bodies, have different degrees both of reflexibility and refrangibility; and that the mixture of all the coloured rays, into which the ſun's light is thrown by the priſm, makes white light; and therefore, that all homogenial light, that is, light whoſe rays are all alike refrangible, have a peculiar colour anſwering to their degree of refrangibility; that this is unalterable by any farther reflections or refractions whatever; that the ſun's light is compoſed of all the primary colours together; and that compound colours proceed from a mixture of the primary. Hence it follows, that no colours can ariſe from any new modifications of light. But Sir *Iſaac* is of opinion, that the permanent colours of bodies are owing to the diſpoſition of them to reflect ſome one ſort of rays, as red, blue, &c. more plentifully than others. Hence red bodies appear moſt beautiful in red light, blue bodies in blue light, &c.

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far from being opake, that 'tis ſcarce ſo much as viſible. And multitudes of bodies there are, whoſe fragments ſeem opake to the naked eye, which yet, when included in good microſcopes, appear tranſparent. On the other hand, there may, perhaps, be ſome bodies, whoſe minute particles, even in an excellent microſcope, will not appear diaphanous. For upon viewing, by the help of a good glaſs, mercury precipitated *per ſe*, the little granules that made up the powder, appeared like little fragments of coral beheld at a diſtance, with the naked eye. Filings of ſteel likewiſe, and copper, tho' viewed in an excellent microſcope, on a fair day, ſhow'd like pretty large fragments of thoſe metals, and appear'd conſiderably bright on ſome of their ſurfaces; yet I was not ſatisfy'd that I perceived reflections from the inner parts of any of them. Nay, looking thro' my beſt microſcope upon the red calx of lead, neither I, nor any to whom I ſhew'd it, could diſcern it to be other than opake, tho' the day was clear, and the object ſtrongly enlighten'd. And even calcined vitriol, tho' deeply red, appeared in the ſame microſcope like coarſe brick-duſt. Nor would I be forward to determine how far, or in what caſes the tranſparency or ſemi-diaphaneity of the ſuperficial corpuſcles of larger bodies may have an intereſt in the production of their colours; eſpecially ſince even in ſeveral white ſubſtances, as beaten glaſs, ſnow, and froth, where it ſeems manifeſt that the ſuperficial parts are ſingly diaphanous, we ſee no ſuch variety of colours as is uſual from the refraction of light in thoſe bodies, when by their bigneſs, ſhape, *&c.* they are properly qualified to exhibit the colours of the rain-bow and priſmatic glaſs †.

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† For a clear account of opacity and tranſparency, let us hear the excellent Sir *Iſ. Newton.* The opacity of bodies proceeds (according to that great Philoſopher) from the multitude of reflections cauſed in their internal parts, by a diſcontinuity of them; for "between the parts of opake "and coloured bodies are many ſpaces, "either empty, or repleniſhed with me"diums of other denſities.—And that this "diſcontinuity of parts is the principal "cauſe of the opacity of bodies, appears "from conſidering, that opake ſubſtances "become tranſparent by filling their pores "with any ſubſtance of nearly equal den"ſity with their parts. Thus paper dipt in "water or oil, the *Oculus mundi* ſtone "ſteeped in water, linen cloth oil'd or "varniſh'd, and many other ſubſtances "ſoaked in ſuch liquors as will inti"mately pervade their little pores, become, "by that means, more tranſparent than "otherwiſe: ſo, on the contrary, the "moſt tranſparent ſubſtances may, by "evacuating their pores, or ſeparating their "parts, be render'd ſufficiently opake; as "ſalts or wet paper, or the *Oculus* "*mundi* ſtone, by being dried; horn, "by being ſcraped; glaſs, by being re"duced to powder, or otherwiſe, flaw'd; "turpentine, by being ſtirred about with "water, till they mix imperfectly; and "water, by being form'd into many ſmall "bubbles, either alone, in the form of "froth, or by ſhaking it together with "oil of turpentine, or oil olive, or "with ſome other convenient liquor, "wherewith it will not perfectly incor"porate. And to the increaſe of the "opacity of theſe bodies, it conduces "ſomething, that the reflexions of very "thin, tranſparent ſubſtances are conſi"derably ſtronger than thoſe made "by the ſame ſubſtances of a greater "thickneſs." *Newton. Optic.* p. 223, 224.

Sir *Iſaac Newton* farther obſerves, that the parts of bodies, and their interſtices, muſt not be leſs than of ſome definite big-

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Whether objects are coloured in the dark?

By what has hitherto been delivered we may be aſſiſted to judge of that famous controverſy anciently held, betwixt the atomiſts on one ſide, and moſt other philoſophers on the other ſide; the former denying bodies to be coloured in the dark, and the latter maintaining colour to be an inherent quality, as well as figure, hardneſs, weight, &c. For, if colour be only light modify'd, how can it ſubſiſt in the dark? But if colour be conſidered as a certain conſtant diſpoſition of the ſuperficial parts of the object, to diſorder the light they reflect after a determinate manner; as this modifying diſpoſition perſeveres in the object, whether it be enlighten'd or not, there ſeems no juſt reaſon to deny, that, in this ſenſe, bodies retain their colour as well in the night as in the day. Or it may, upon this ſuppoſition, be otherwiſe ſaid, that bodies are potentially coloured in the dark, and actually coloured in the light.

Whether emphatical colours be real or imaginary?

But 'tis of greater moment in the inquiry into the nature of colours, to decide that controverſy, whether thoſe of the rain-bow; thoſe often ſeen in clouds before the riſing, or after the ſetting of the ſun; and, in a word, whether thoſe other colours, uſually called emphatical, ought to be accounted true: for it being generally granted, and may eaſily be proved, that emphatical colours are light modify'd chiefly by refractions, with

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bigneſs to render them opake and coloured; "for the opakeſt bodies, if their parts be ſubtily divided, become perfectly tranſparent. Thus water, ſalt, glaſs, ſtones, and ſuch like ſubſtances, are tranſparent; for, upon divers conſiderations, they ſeem to be as full of pores or interſtices between their parts as other bodies are; but yet their parts and interſtices ſeem to be too ſmall to cauſe reflexions in their common ſurfaces." *Newton. Optic. p.* 225.

"The tranſparent part of bodies, ſays Sir *Iſaac Newton*, according to their ſeveral ſizes, reflect rays of one colour, and tranſmit thoſe of another, on the ſame grounds that thin plates or bubbles do reflect or tranſmit thoſe rays;" and this Sir *Iſaac* takes to be the ground of all their colours: "for if, ſays he, a thinned or plated body, which being of an even thickneſs, appears all over of one uniform colour, ſhould be ſlit into threads, or broken into fragments, I ſee no reaſon why every thread or fragment ſhould not keep its colour; and, by conſequence, why a heap of thoſe threads or fragments, ſhould not conſtitute a maſs or powder of the ſame colour which the plate exhibited before it was broken. And the parts of all natural bodies being like ſo many fragments of a plate, muſt, on the ſame grounds, exhibit the ſame colour. Now that they do ſo, will appear by the affinity of their properties. The finely coloured feathers of ſome birds, and particularly thoſe of peacocks tails, do, in the very ſame part of the feather, appear of ſeveral colours in ſeveral poſitions of the eye, after the very ſame manner that thin plates do. And therefore their colours ariſe from the thinneſs of the tranſparent parts of the feathers; that is, from the ſlenderneſs of the very fine hairs, or *Capillamenta*, which grow out of the ſides of the groſſer lateral branches or fibres of thoſe feathers. And to the ſame purpoſe it is, that the webs of ſome ſpiders, by being ſpun very fine, have appeared coloured; and that the coloured fibres of ſome ſilks, by varying the poſition of the eye, do vary their colour. Alſo the colours of ſilks, cloths, and other ſubſtances which water or oil can intimately penetrate, become more faint and obſcure, by being immerſed in thoſe liquors, and recover their vigor again, by being dried after the manner of thin bodies. Leaf-gold,

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with the concurrence ſometimes of reflections, and perhaps other accidents depending on them both; if theſe emphatical colours be reſolved genuine, it will ſeem to follow, that colours, or at leaſt many of them, are but diverſify'd light, and not ſuch real and inherent qualities as men commonly ſuppoſe.

Now ſince we allow echoes, and other ſounds of bodies, to be true ſounds, all their odours to be true odours, and in ſhort, other ſenſible qualities to be true, becauſe they are the proper objects of ſome of our ſenſes; I ſee not why emphatical colours, being the proper and peculiar objects of the organ of ſight, and capable to affect it as truly and as powerfully as other colours, ſhould be reputed imaginary. And if we have proved, that colour, when taken in its more proper ſenſe, is only modify'd light, there will be little reaſon to deny, that theſe are true colours, which more manifeſtly than others ſhew themſelves to be produced by the diverſifications of light. There is indeed a difference taken notice of betwixt theſe apparent colours, and thoſe that are uſually eſteemed genuine, as to their duration; which has induced ſome learned men to call the former rather evanid than fantaſtical. But, as *Gaſſendus* obſerves, if this way of arguing were true, the greenneſs of a leaf ought to paſs for apparent, becauſe ſoon fading into a yellow, it cannot

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" ſome ſorts of painted glaſs, the infuſion " of *Lignum Nephriticum*, and ſome other " ſubſtances, reflect one colour, and tranſmit another, like thin bodies alſo. And " ſome of thoſe coloured powders which " painters uſe, may have their colours a " little changed, by being very elaborately and finely ground. Where I ſee not " what can be juſtly pretended for thoſe " changes, beſides the breaking of their " parts into leſs parts, by that contrition, " after the ſame manner that the colour of " a thin plate is changed, by varying its " thickneſs: for which reaſon alſo it is, " that the coloured flowers of plants and " vegetables, by being bruiſed, uſually become more tranſparent than before; or " at leaſt in ſome degree or other change " their colours. And thus, by mixing various liquors, very odd and remarkable " productions and changes of colours may " be effected, of which no cauſe can be " more obvious and rational, than that the " ſaline corpuſcles of one liquor do variouſly act upon, or unite with, the tinging corpuſcles of another, ſo as to make " them ſwell or ſhrink, (whereby not only " their bulk, but their denſity alſo may " be changed) or to divide them into ſmaller corpuſcles, (whereby a coloured liquor may become tranſparent) or to " make many of them aſſociate into one " cluſter, whereby two tranſparent liquors " may compoſe a coloured one. For we " ſee how apt thoſe ſaline menſtrua are to " penetrate and diſſolve ſubſtances to which " they are applied; and ſome of them to " precipitate what others diſſolve. In " like manner, if we conſider the various " phenomena of the atmoſphere, we may " obſerve, that when vapours are firſt raiſed, they hinder not the tranſparency of " the air; being divided into parts too " ſmall to cauſe any reflexion in their ſuperficies. But when, in order to compoſe drops of rain, they begin to coaleſce, and conſtitute globules of all intermediate ſizes, thoſe globules, when they " become of a convenient ſize to reflect " ſome colours, and tranſmit others, may " conſtitute clouds of various colours, according to their ſizes. And I ſee not " what can be rationally conceived in ſo " tranſparent a ſubſtance as water, for the " production of theſe colours, beſides the " various ſizes of its fluid and globular parcels." *Newton. Opt. p.* 226,——228.

be compared, in point of duration, with the greenneſs of an emerald. But if the ſun-beams paſs in a convenient manner thro' a glaſs priſm, ſo as to be thrown upon ſome well ſhaded object, within a room; the rainbow thereby painted on the ſurface of a body that terminates the rays, may often laſt longer than ſome colours I have produced in certain bodies: which colours would juſtly, and without ſcruple, be accounted genuine, tho' they ſuddenly degenerate, and loſe their nature.

A greater diſparity betwixt emphatical colours, and others, may, perhaps, be urged; becauſe genuine colours ſeem to be produced in opake bodies, by reflection; but apparent ones, in diaphanous bodies, principally, by refraction: for, in ſome caſes, reflection, alſo, may concur. But ſtill this ſeems not to prove, that theſe latter colours are falſe ones; nor muſt what we lately ſaid of the differences of true and ap[illegible]rent colours, be underſtood in too unlimited a ſenſe: for if water be agitated into froth, it exhibits a white colour, which it ſoon after loſes upon the reſolution of the bubbles into air and water; in which caſe, the whiteneſs of the froth is either a true colour, or not. If it be true, then true colours, ſuppoſing the water pure, may prove as ſhort-lived as thoſe of the rainbow; and the matter wherein the whiteneſs reſided, may, in a few moments, be perfectly deprived of it. Beſides, even diaphanous bodies may be capable of exhibiting true colours, by reflection; for that whiteneſs is ſo produced, will ſhortly appear. But if it be ſaid, that the whiteneſs of froth is an emphatical colour; then it muſt no longer be ſaid, that fantaſtical colours require a certain poſition of the illuminating body and the eye; and muſt be varied or deſtroyed by the change thereof: for froth appears white, whether the ſun be riſing or ſetting; in the meridian, or any where between that and the horizon; and in what part ſoever the ſpectator's eye is placed. And ſince, by making a liquor tenacious, without deſtroying its tranſparency, or ſtaining it with any colour, we can give the little films whereof the bubbles conſiſt, ſuch a texture as will make the froth laſt for many hours, or days, together; it ſeems improper to aſſign duration for the diſtinguiſhing character of genuine from fantaſtic colours: for ſuch froth may long out-laſt the true colours of ſome natural productions. Thus in that gaudy plant, the marvel of *Peru*, the flowers often fade on the very ſame day they are blown. And I have often ſeen a *Virginia* flower, which uſually withers within the compaſs of a day. I am alſo credibly inform'd, that a curious neighbouring botaniſt has a plant whoſe flowers periſh in about an hour's time.

But if the whiteneſs of water, turn'd into froth, muſt therefore be reputed emphatical, becauſe it appears not that the nature of the body, but only that the diſpoſition of its parts, with regard to the incident light, is changed; why may not that whiteneſs be accounted emphatical too, which I ſhall ſhortly ſhew to be producible, barely by ſuch another change in black horn? And yet this whiteneſs, which is ſo eaſily acquir'd, ſeems to be as truly its colour as blackneſs was before; and is, at leaſt, more permanent than the greenneſs of leaves, the redneſs of roſes, or the genuine colours of moſt natural productions. It may, indeed, be further objected, that, according as the ſun or other luminous body changes place, theſe emphatical colours alter, or vaniſh. If a piece of cloth, in a draper's ſhop, where the light is ſeldom primary, be variouſly folded, it will appear of different colours, as the parts happen to be more illuminated or ſhaded; and if you ſtretch it flat, it will commonly exhibit ſome one uniform colour; yet theſe are not reputed emphatical: ſo that the difference ſeems to be chiefly this, that in the caſe of the rain-bow, and the like, the poſition of the illuminating body varies the colour; and, in cloth, the poſition of the object does it. Yet I am not forward to allow, that in all caſes the apparition of emphatical colours requires a determinate poſition of the eye; for if men will have the whiteneſs of froth emphatical, we have already ſeen what may be inferred from thence. Beſides, the ſun-beams tranſmitted thro' a triangular priſm of glaſs, after the manner lately mentioned, will, upon the body that terminates them, paint a rain-bow, viſible to the eye, whether placed on the right hand of it, or on the left; above it, or beneath it; before it, or behind it: and tho' there may appear ſome little variation in the colours of the rain-bow, beheld from different parts of the room; yet the like may be obſerved by an attentive eye, in real colours, view'd under the like circumſtances. Nor will it follow, that, becauſe there remain no footſteps of the colour upon the object, when the priſm is removed, that therefore the colour was not real; ſince the light was truly modify'd by the refraction * and reflection it ſuffer'd in paſſing thro' the priſm: and the object, in our caſe, ſerved as a ſpeculum to reflect that colour to the eye. For a rough and coloured object may ſerve for a ſpeculum to reflect the artificial rain-bow we ſpeak of; ſince, in darkned rooms, a wall, conveniently ſituated within, will ſo reflect the colours of bodies external to the room, that they may very clearly be diſcern'd and diſtinguiſhed: and yet it is taken for granted, that the colours ſeen in a darkned room, tho' they leave no traces upon the wall or body that receives them, are the true colours of the external objects; together with which the colours of the images are moved, or reſt. And the error lies not in the eye, whoſe office is only to perceive the appearance of things, and truly does ſo; but in the judgment that, by miſtake, concludes the colour belongs to the wall, which really belongs to the object; becauſe the wall is that from whence the rays of light which carry the viſible ſpecies, come in ſtrait lines directly to the eye. And thus, for the ſame reaſon, we ſuppoſe at a certain diſtance from concave ſpherical glaſſes,

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* We muſt again repeat, that Sir *Iſaac Newton* demonſtrates, all the colours in the univerſe, which are made of light, and depend not on the power of imagination, are either the colours of homogeneal lights, or compounded of them in a certain proportion. "For the changes of colour, made by refraction, do not ariſe from any new modification of the rays impreſſed by thoſe refractions; and, by the various terminations of light and ſhadow; as has been the conſtant and general opinion of philoſophers." *Newton. Optic.* p. 99, & p. 138.

that we ſee the image ſtand out to meet us, and hang in the air betwixt the glaſs and us; becauſe the reflected rays, that compoſe the image, croſs one another in that place where the image appears to be; and thence in direct lines take their courſe to the eye. I might here add what will more fully appear hereafter, that colours, called emphatical, becauſe not inherent in the bodies where they appear, may be compounded with one another, as thoſe that are confeſſedly genuine.

SECT. II.

The nature of whiteneſs.

I Come now to inquire into the nature of whiteneſs and blackneſs. Whiteneſs, conſidered as a quality in the object, ſeems, in the general, chiefly to depend upon the roughneſs of the ſurface of the body, call'd white; which gives it innumerable ſmall ſuperficies, that acting like ſo many little ſpecula in various poſitions, they reflect the rays of light that fall on them, not towards one another, but externally towards the ſpectator's eye. The ſun, and other very lucid bodies, not only offend or dazle our eyes; but if any colour is aſcribed to them, it ſhould be whiteneſs: * for the ſun at noon-day, and in clear weather, when his face is ſerene,

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* White, in ſtrictneſs, we have already obſerved to be no ſimple colour; but a compound of all colours in a due proportion. "When the ſeveral ſorts of rays are mixed, "ſays Sir *Iſaac Newton*, and, in croſſing, "paſs thro' the ſame ſpace, they act not "upon one another, ſo as to change their "colour-making qualities; but, by mixing "their actions in the ſenſorium, beget a "ſenſation, differing from what either "would do a-part: that is, a ſenſation of "a mean colour between their proper co-"lours. And particularly, when, by the "concourſe and mixtures of all ſorts of "rays, a white colour is produced; the "white is a mixture of the colours which "the rays would have a-part: the rays in "that mixture do not alter their ſeveral co-"lour-making qualities; but, by all their "various kinds of actions, mixed in the "ſenſorium, beget a ſenſation of a mid-"dling colour, between all their colours, "which is whiteneſs. For whiteneſs is a "mean between all colours; having itſelf "indifferently to them all; ſo as with equal "facility to be tinged with any of them. "A red powder mixed with a little blue, "or a blue with a little red, doth not pre-"ſently loſe its colour; but a white pow-"der, mixed with any colour, is preſently "tinged with that colour, and is equally "capable of being tinged with any colour "whatever." See *Newton. Optic. p.* 117, 118, 138, 139.

We farther learn from Sir *Iſaac Newton*, that as a mixture of all the priſmatic colours makes perfect white light; ſo if ſoapy water be agitated into a froth, various colours will appear therein, when viewed near; and, at a diſtance, when thoſe colours can no longer be diſtinguiſhed, the froth will ſeem perfectly white. But, by mixing coloured powders, he tells us, we are not to expect a ſtrong and full white, but ſome dusky obſcure one; becauſe they ſuppreſs and ſtop in them a very conſiderable part of the light by which they are illuminated. Thus he produced a dark white, or a dun colour, by mixing one part of red lead with five parts of verdigreaſe; for theſe two colours were ſeverally ſo compounded of others, that, in both together, there was a mixture of all colours.

Again, one part of red lead, and four parts of blue biſe, compoſed a dun colour, varying a little to purple; and, by adding to it a certain mixture of orpiment

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ſerene, and his rays paſs thro' a much leſs part of the atmoſphere to our eyes, appears of a colour more approaching to white than when he is nearer the horizon; in which caſe the interpoſition of certain fumes and vapours make him oftentimes appear either red or yellow. And when the ſun ſhines upon ſmooth water, that part of it which appears moſt illumined, ſeems far whiter than the reſt. And I have ſometimes found, that when the ſun was veil'd with a thin white cloud, tho' ſtill too bright to be look'd upon directly; that by caſting my eyes upon ſtill water, his body, being not far from the meridian, appeared to me exceedingly white. And tho' we vulgarly ſay, in *Engliſh*, a thing is red-hot, to expreſs a ſuperlative degree of heat; yet, at the forges and furnaces of artificers, by a white heat they underſtand a further degree of ignition, than by a red one.

2. Common experience informs us, that as too much light over-powers the eye; ſo when the ground is covered with ſnow, thoſe who have a weak ſight, complain that this proſpect is offenſive to them. And even thoſe who have good eyes, are from hence generally ſenſible of an extraordinary light in the air; and, if obliged to look very long upon the ſnow, find their ſight injured by it. Thus *Xenophon* relates, that *Cyrus* marching his army, for many days, over mountains covered with ſnow, the dazling ſplendor of its whiteneſs prejudiced the ſight of a great number of his ſoldiers, and blinded ſome of them; and other accounts of the ſame nature may be met with in writers of good note. The like has alſo been affirmed to me by credible perſons of my own acquaintance, and eſpecially by one, who, during

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ment and verdigreaſe, in due proportion, it loſt its purple tincture, and became perfectly dun. But the experiment ſucceeded beſt without red lead, thus: "To orpiment, ſays Sir *Iſaac*, I "added, by little and little, a certain "full bright purple, which painters "uſe, till the orpiment ceaſed to be "yellow, and became of a pale red: "then I diluted that red, by adding a "little verdigreaſe and a little more blue "biſe than verdigreaſe, till it became "of ſuch a grey or pale white, as va- "ried to no one of the colours more than "to another. For thus it became of a "colour equal in whiteneſs to that of "aſhes, or of wood newly cut, or of a "man's skin. The orpiment reflected "more light than did any other of the "powders; and therefore conduced more "to the whiteneſs of the compounded "colour than they. To aſſign the pro- "portions accurately, may be difficult, "by reaſon of the different goodneſs of "powders of the ſame kind. As the co- "lour of any powder is more or leſs full "and luminous, it ought to be uſed in a "leſs or greater proportion. Now con- "ſidering that theſe grey and dun co- "lours may be alſo produced, by mixing "whites and blacks; and, by conſe- "quence, differ from perfect whites, not "in ſpecies of colours, but only in de- "gree of luminouſneſs; it is manifeſt "that there is nothing more requiſite to "make them perfectly white, than to "increaſe their light ſufficiently; and, "on the contrary, if by increaſing their "light, they can be brought to perfect "whiteneſs; it will thence alſo follow, "that they are of the ſame ſpecies of "colour with the beſt whites; and differ "from them only in the quantity of "light." And accordingly, by placing ſome powder, compoſed of orpiment, purple, biſe, and verdigreaſe, in the ſun's rays; and, viewing it at a diſtance, it appeared intenſely white. *Newton. Optic.* p. 129—134.

his ſtay in *Muſcovy*, found his eyes much impaired, by being frequently obliged to travel in the ſnow: and this weakneſs of ſight did not leave him when he left that country, but ſtill continues, tho' he be a young man. I myſelf alſo, as well as others, have obſerved, that upon travelling by night, when the ground was all covered with ſnow, tho' it would otherwiſe have been dark, yet we could very well ſee to chuſe our way. But much more to my preſent purpoſe is that account given us by *Olaus Magnus* of a way of travelling in the *Northern* regions during the winter, where the days of that ſeaſon are very ſhort. "In the day-time, ſays he, " they travel twelve *Italian* miles, but twice or thrice as far in the night, " and that with eaſe; for the light of the moon, reflected by the ſnow, " renders both hills and vales conſpicuous; ſo that then they can ſee not " only precipices afar off, but the wild beaſts they would avoid."

This teſtimony I the leſs ſcruple to alledge, becauſe it agrees very well with what has been affirmed to me by a phyſician of *Moſcow*; who informed me, that he could ſee things at a far greater diſtance, and with more clearneſs, when he travelled by night on the ſnow there, tho' without the aſſiſtance of the moon-ſhine, than we, in theſe parts, would eaſily believe. 'Tis true, indeed, the intenſeneſs of the cold might contribute ſomething to the conſiderableneſs of the effect, by clearing the air of dark ſteams, which, in theſe more temperate climates, are uſually thick in ſnowy weather: for this phyſician, and the ingenious navigator Captain *James* agree, that in dark froſty nights, in frozen climates, they could diſcover more ſtars, and have a clearer proſpect of the heavens, than we in *England*. I know, indeed, many learned men ſuppoſe ſnow thus ſtrongly affects our eyes, not by a borrowed, but a native light. I venture, however, to give it as a proof, that white bodies reflect more light than others, becauſe having once placed a parcel of ſnow in a room, carefully darkened, that no celeſtial light might fall upon it, neither I, nor an ingenious perſon ſkilled in optics, could find it had any light beſides what it received; and 'tis uſual, among ſuch as travel in dark nights, to make their guides wear ſomething of white to be diſcern'd by; for there is ſcarce any night ſo dark, but that, in the free air, ſome light remains, tho' broken and debilitated, perhaps, by a thouſand reflections from the opake corpuſcles, that ſwim in the atmoſphere, and ſend it to one another before it arrives at the eye.

3. And the better to ſhew that white bodies reflect much more light than others, I held in the darkened room, formerly mentioned, not far from the hole at which the light entered, a ſheet of white paper; from whence caſting the ſun-beams upon a white wall, it manifeſtly appeared both to me, and to a perſon I took to be witneſs of the experiment, to reflect a far greater light than any of the other colours; for the wall itſelf was not only thus notably enlightened, but alſo a conſiderable part of the room. And, further, to ſhew that white bodies reflect the rays outwards, let me add, that ordinary burning-glaſſes will not, in a great while, burn or diſcolour white paper; ſo that when I was a boy, and delighted to

make trials with ſuch glaſſes, I could not but wonder at this odd phenomenon, which ſet me very early upon gueſſing at the nature of whiteneſs; and the more, becauſe I took notice that the image of the ſun, upon white paper, was not ſo well defined as upon black; and becauſe that, upon inking over the paper, the moiſture would be quickly dried up, and the paper, that I could not burn before, would now preſently take fire. I have alſo try'd, that by expoſing my hand, with a thin black glove on it, to the warm ſun, it would thereby very ſuddenly be more conſiderably heated, than if I took off the glove, and held my naked hand to its rays, or put on another glove of thin white leather.

4. And to ſhew that white bodies are apt, like ſpecula, to reflect the light that falls on them, we have found, in a darken'd room, that the ſun-beams being caſt from a coloured body upon a white wall, the determinate colour of the body was from the wall reflected to the eye; whilſt we could, in many caſes, manifeſtly alter the colour arriving at the eye, by ſubſtituting, at a convenient diſtance, a colour'd gloſſy body inſtead of the white wall; thus by throwing the rays from a yellow body upon a blue, there would be exhibited a kind of green. I might alſo here take notice, that when looking upon the calm and ſmooth ſurface of a river, lying betwixt my eye and the ſun, it appeared to be a natural ſpeculum, wherein that part, which reflected to my eye the entire and defined image of the ſun and rays adjacent thereto, appeared of a great whitiſh brightneſs, but the reſt comparatively dark; when, if afterwards the ſuperficies chanced to be a little ruffled by a gentle breath of wind, and thereby reduced into a multitude of ſmall, ſmooth ſpecula, the ſurface of the river would, at a diſtance, appear almoſt white; tho' it loſt that appearance upon a return of the ſurface to a calm and uniform level. And I have ſometimes, as an experiment, brought in a lenticular glaſs, the image of a river illumined by the ſun, into a darkened room, at about the diſtance of a quarter of a mile; by which means the numerous declining ſurfaces of the water were ſo contracted, that, upon the body which received the image, the whole river appeared a very white object at two or three paces diſtance. But if we drew near it, this whiteneſs ſeemed to proceed from innumerable lucid reflections, made by the ſuperficies of the water gently waved, which looked, when cloſely viewed, like a multitude of very little ſhining ſcales of fiſh; many whereof every moment diſappeared, while as many were by the ſun, wind, and river, generated anew. But tho' this obſervation ſeemed ſufficiently to diſcover how the apparent whiteneſs, in that caſe, was produced; yet in ſome other caſes, water may have the ſame, tho' not ſo vivid a colour, upon other accounts; for it often happens that the ſmooth ſurface of the water appears bright or whitiſh, by reaſon of the reflection, not immediately of the image of the ſun, but of the brightneſs of the ſky; and in ſuch caſes a convenient wind may, where it paſſes along, make the ſurface look black, by cauſing many ſuch furrows and cavities, as make the inflected ſuperficies of the water reflect the brightneſs of the ſky, rather internally

ly than externally. And again, if the wind increaſe into a ſtorm, the water may appear white, eſpecially near the ſhore, becauſe the rude agitation breaks it into foam or froth; ſo much do whiteneſs and blackneſs depend upon the diſpoſition of the ſuperficial parts of a body, to reflect the rays of light inward or outward. But that as white bodies reflect the moſt light of any; ſo their ſuperficial particles are of a ſpecular nature, I ſhall further endeavour to ſhew, by making ſpecular bodies white, and a white body ſpecular.

5. Upon diſtilling quickſilver in a cucurbit, fitted with a capacious glaſs-head, I have obſerved, that when the operation was performed by proper degrees of fire, there would ſtick to the inſide of the alembic a multitude of little round drops of mercury. And as mercury is a ſpecular body, each of theſe little drops was a ſmall ſpherical looking-glaſs; and a number of them lying near one another, made the glaſs they were faſtened to, manifeſtly appear a white body. And as many parts of the sky, eſpecially the milky way, appear white to the naked eye; yet the galaxy, viewed thro' a teleſcope, does not appear white, but to be made up of a vaſt multitude of little ſtars; ſo, many lucid bodies, if too ſmall to be ſingly diſcerned by the eye, and ſet ſufficiently thick by one another, may, by their united rays, appear to the eye as one white body: and why may not the like happen, when a multitude of bright, little corpuſcles, crowded together, are made jointly to reflect vivid rays to the eye, tho' they ſhine by a borrowed light?

But to return to our experiments; we may take notice, that the white of an egg, which tho' in part tranſparent, yet, by its power of reflecting ſome incident rays of light, is, in ſome meaſure, a natural ſpeculum, being long agitated with a whisk or ſpoon loſes its tranſparency, and becomes a very white froth; that is, an aggregate of numerous ſmall bubbles, whoſe convex ſuperficies fit them to reflect the light every way outwards. And 'tis worth obſerving that when water, for inſtance, is agitated into froth, if the bubbles be great and few, the whiteneſs will be but faint, becauſe the number of ſpecula within a narrow compaſs is but ſmall; and they are not thick enough ſet to reflect ſo many little images or rays of the lucid body, as go to produce a vigorous ſenſation of whiteneſs. And leſt it ſhould be ſaid, that the whiteneſs of ſuch globular particles proceeds from the air included in the froth, (tho' who can prove that the air itſelf is white?) and at the ſame time to illuſtrate our doctrine of whiteneſs, I ſhall add this experiment. I put to ſome fair water, contained in a glaſs vial, a convenient quantity of ſpirit of turpentine, which will not incorporate with water, yet is almoſt as clear and colourleſs as that; and theſe being well ſhaken together, I found the agitation broke the oil into a multitude of little globes, which each of them reflecting outwards a lucid image, made the imperfect mixture of the two liquors, appear whitiſh; but if by vehemently ſhaking the glaſs for a competent time, a further comminution of the oil be made into far more numerous and ſmaller globules, whilſt it is alſo thereby more thoroughly

PHYSICS. confounded with the water, the mixture will appear of a much greater whiteneſs, and almoſt like milk; tho' if the glaſs be let alone a while, the colour will gradually fade as the oily globules grow fewer and larger, and at length quite vaniſh; leaving both the liquors diſtinct and diaphanous as before. And ſuch a trial hath ſucceeded, when inſtead of oil, or ſpirit of turpentine, I took a yellow mixture, made with a large proportion of crude turpentine, diſſolved in that liquor; and it alſo ſucceeded better than one would expect, when I employ'd an oil brought to a deep green, by infuſing copper filings therein. Thus aromatics, diſtilled with water, often yield a white liquor, which may long continue of that colour; becauſe if the fire be made too ſtrong, the ſubtile oil is thereby much agitated, broken, and blended with the water, in ſuch numerous and minute globules, as cannot eaſily, in a ſhort time, emerge to the top of the water, and, whilſt they remain therein, make it look whitiſh. And hence, perhaps, it is, that we find hot water uſually more opake and whitiſh than the ſame when cold; the agitation turning the more volatile particles of the water into vapours, and thereby producing, in the body of the liquor, a multitude of ſmall bubbles, which interrupt the free paſſage of the rays of light, and from the innermoſt parts of the water, reflect many of them outwards. Theſe, and the like examples, have induced me to ſuſpect, that the ſuperficial particles of white bodies, may, for the moſt part, be as well convex as ſmooth; tho' it ſeems not eaſy to prove, that when diaphanous bodies are reduced into white powders, each corpuſcle muſt needs be of a convex ſuperficies; and, perhaps, it may here ſuffice that the ſpecular ſurfaces look ſeveral ways. We have ſeen that when a diaphanous body is reduced to very minute parts, it thereby acquires a multitude of little ſurfaces within a narrow compaſs; and tho' each of thoſe ſhould not be of a convenient figure to reflect a round image of the ſun, they may reflect ſome phyſical line of light, wherein ſome refraction of that which falls upon the body, whereon it depends, may often contribute to its whiteneſs. Thus if a ſlender wire, or ſolid cylinder of glaſs, be expoſed to luminous rays, you ſhall ſee, in ſome part thereof, a vivid line of light; and if we were able to draw out and lay together a multitude of theſe little wires or threads of glaſs, ſo ſlender, that the eye could not diſcern a diſtance between the luminous lines, there is no doubt, as far as I can gueſs by a trial of this kind purpoſely made, that the whole phyſical ſuperficies, compoſed of them, would appear white to the eye: and if ſo, it is not always neceſſary that the figure of thoſe corpuſcles, that make a body appear white, ſhould be ſpherical: and ſnow itſelf commonly appears both to the naked eye, and when viewed thro' a microſcope, to conſiſt principally of little ſlender icicles of ſeveral ſhapes, which afford ſuch numerous lines of light as we ſpeak of.

6. If you take a diaphanous body, as, for inſtance, a piece of glaſs, and reduce it to powder, the ſame body which, when entire, freely tranſmitted the rays of light, acquiring, by contuſion, a multitude of minute ſurfaces, each of which is, as it were, a little ſpeculum, becomes thereby

by qualified to reflect, in a confuſed manner, ſo many rays, or little and ſingly unobſervable images of the lucid body, that from a diaphanous, it degenerates into a white ſubſtance. And heating lumps of cryſtal red-hot, in a crucible, I have found, that, upon quenching 'em in fair water, even thoſe which remained ſeemingly entire, exchanged their tranſparency for whiteneſs; the ignition and extinction having cracked each lump into a multitude of minute bodies, and thereby given it a multitude of new ſurfaces. And even with coloured diaphanous bodies, there may, by this way, be a great degree of whiteneſs produced. I have, by contuſion, obtained whitiſh powders from granats, glaſs of antimony, and emeralds; but the experiment is more eaſily made, by comparing deep blue pulverized vitriol of copper, with ſome of the entire cryſtals of the ſame, for this will, comparatively, exhibit a conſiderable degree of whiteneſs.

7. And as by a change of poſition in the parts of differently coloured bodies, they may be rendered white; ſo by a ſlight change, in the texture of its ſurface, a white body may be deprived of that property. A piece of ſilver newly boiled, with ſalt and tartar, after the goldſmith's faſhion, is of a lovely white; but if, with a piece of ſmooth ſteel, a part of it be burniſh'd, that part preſently loſes its whiteneſs, and turns to a ſpeculum, almoſt every where dark, like other mirrors; which adds a great confirmation to our doctrine. For hence we ſee what it chiefly is that made the body white before; ſince all that was done to deprive it of that whiteneſs, was only to depreſs the little irregular protuberancies, that ſtood out on the ſurface of the ſilver, into one continued regular plain.

The nature of blackneſs.

8. What we have ſaid of whiteneſs may aſſiſt us to form a notion of blackneſs; thoſe two qualities being ſufficiently oppoſite to illuſtrate each other. And as that which makes a body white, is chiefly ſuch a diſpoſition of its parts, as diſpoſes it to reflect more of the light that falls on it, than bodies of different colours; ſo that which renders a body black, is principally a peculiar kind of texture of its ſuperficial particles; whereby it damps the light that falls on it, ſo that very little is reflected to the eye.

9. This texture is explicable two ſeveral ways; and firſt, by ſuppoſing, in the ſuperficies of the black body, a particular kind of aſperity; whence the ſuperficial particles reflect few of the incident rays outwards, and the reſt inwards, upon the body itſelf: as if, for inſtance, the ſurface of a black body ſhould riſe up in numberleſs little cylinders, pyramids, cones, *&c.* which, by being thick ſet and erect, throw the rays of light from one to another inwards, ſo often, that, at length, they are loſt before they can come out again to the eye. The other way ſuppoſes the textures of black bodies either to yield to the rays of light, or, upon ſome other account, to ſtifle and keep them from being reflected in any number, or with any conſiderable vigour outwards. According to this notion it may be ſaid, that the corpuſcles, which compoſe the rays of light, thruſting one another from the lucid body, and falling on black ſubſtances, meet with ſuch a texture, that they receive into themſelves, and retain almoſt all the

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motion communicated to them by thoſe corpuſcles, and conſequently reflect but few of them, or thoſe but languidly, towards the eye; as when a ball, thrown againſt a floor, rebounds a great way upwards, but very little or not at all when thrown againſt mud or water; becauſe the parts yield, and receive into themſelves the motion which ſhould reflect the ball back. But this laſt manner of accounting for blackneſs I barely propoſe, without either adopting, or abſolutely rejecting it; for the hardneſs of touchſtones, black marble, and of other bodies that are black, and ſolid, ſeems to render it ſomewhat improbable that they ſhould be of ſo yielding a nature; unleſs we ſay that ſome bodies may be more diſpoſed to yield to the impulſes of the corpuſcles of light, by reaſon of a peculiar texture, than others, which, by particular trials, appear to be ſofter than they. Both the ſolutions, however, agree in this, that black bodies reflect but little of the light which falls on them. And it is not impoſſible that, in ſome caſes, both the diſpoſition of the ſuperficial particles as to figure and poſition, and the yielding of the body, or ſome of its parts, may jointly contribute to render a body black. The conſiderations which induced me to propoſe this notion of blackneſs, are principally theſe:

(1.) Whiteneſs and blackneſs being generally reputed contrary qualities, whiteneſs depending, as I ſaid, upon the diſpoſition of the parts of the body, to reflect light plentifully, it ſeem'd probable, that blackneſs might depend upon a contrary diſpoſition of ſurface; but upon this I ſhall not inſiſt. However, if a body, of an uniform colour, be placed, part in the ſun-beams, and part in the ſhade, that part, which is not illumined, will appear nearer allied to blackneſs than the other, from which more light is reflected to the eye; dark colours alſo ſeem the blacker, the leſs is the light they are viewed in: and all things ſeem black in the dark, when they yield no rays to make impreſſions upon our organs of ſight; ſo that ſhadow and darkneſs are near a-kin: and ſhadow, we know, is but a privation of light; blackneſs, accordingly, ſeems to proceed from the want of rays reflected from the black body to the eye; tho' the bodies we call black, as marble, jet, &c. are not perfectly ſo, for if they were, we ſhould not ſee them at all. But notwithſtanding the rays which fall on the ſides of thoſe erect particles we mentioned, do few of them return outwards; yet ſuch as fall upon the points of thoſe cylinders, cones, or pyramids, may be thence reflected to the eye, tho' they make but a faint impreſſion there; becauſe they are mixed with a great proportion of little ſhades. Thus, having procured a large piece of black marble to be well poliſhed, and brought to the form of a large ſpherical concave ſpeculum, the inſide thereof was a kind of dark looking-glaſs, wherein I could plainly ſee a little image of the ſun, when it ſhone thereon. But this image was very far from offending or dazling my eyes, as it would have done from another ſpeculum; and tho' this were large, I could not, in a long time, ſet a piece of wood on fire with it; tho' a far leſs ſpeculum of the ſame form, and of a more reflecting material, would preſently have made it flame. And having expoſed to the ſun a pretty large mortar of

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of white marble, poliſh'd on the inſide, we found that it reflected a great quantity of glaring light, but ſo ſcatter'd, that we could not make the reflected rays meet in any ſuch conſpicuous focus as that we obſerv'd in the black marble; tho' by holding a candle, in the night-time, at a convenient diſtance, we were able to procure a concourſe of a few reflected rays at about two inches diſtance from the bottom of the mortar. But we found the heat of the ſun-beams ſo diſperſedly reflected, to be very languid, even compared to the focus of the black marble: and the little picture of the ſun that appear'd upon the white marble as a ſpeculum, was very faint, and exceedingly ill defined.

(2.) Taking two pieces, the one of black, and the other of white marble, whoſe ſurfaces were plain and poliſh'd; and caſting on them ſucceſſively the rays of the ſame candle, in ſuch a manner that the adjacent ſuperficies being ſhaded by an opake and perforated body, the incident rays paſs'd thro' a round hole of about half an inch in diameter, the circle of light that appear'd on the white marble was, in compariſon, very bright, but very ill defined; whilſt that on the black marble was far leſs luminous, but much better defined.

(3.) When we look upon a piece of linen that has ſmall holes in it, they appear very black; ſo that men are often deceiv'd in taking holes for ſpots of ink: and painters, to repreſent holes, make uſe of black; the reaſon whereof ſeems to be, that the rays which fall on thoſe holes, penetrate ſo deep, that none are reflected back to the eye. And in a narrow well, part of the mouth ſeems black, becauſe the incident rays are reflected downwards, from one ſide to another, till they can no longer rebound to the eye. We may conſider too, that if different parts of the ſame piece of black velvet be ſtroak'd oppoſite ways, there will appear two diſtinct kinds of blackneſs, the one far darker than the other; probably becauſe in the leſs obſcure part of the velvet, the little ſilken piles, whereof 'tis compos'd, being inclined, there is a greater part of each of them turn'd to the eye; whilſt in the other part the piles of ſilk being more erect, there are by far fewer rays ſent outwards from the lateral parts of each pile: ſo that moſt of thoſe reflected to the eye, come from the tops of the piles, which make but a ſmall part of the whole ſuperficies of the velvet. This explanation I propoſe, not that I think the blackneſs of the velvet proceeds from the cauſe aſſign'd; ſince each ſingle pile of ſilk is black by reaſon of its texture, in what poſition ſoever it be view'd; but becauſe the greater blackneſs of a ſingle tuft ſeems to proceed from the greater defect of rays reflected thence, and from the want of thoſe parts of a ſurface that reflect rays, and the multitude of thoſe ſhaded parts that reflect none. And I have often obſerv'd, that the poſition of particular bodies, far greater than piles of ſilk, may, notwithſtanding each of them hath a colour of its own, make one part of their aggregate appear far darker than another. Thus a heap of carrots appear of a much darker colour when view'd with their points, than with their ſides obverted to the eye.

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(4.) I have obſerv'd in a darken'd room, that if the ſun-beams which came in at the hole were receiv'd upon white, or any other colour, and directed to a convenient part of the room, they would manifeſtly increaſe the light of that part; but if we ſubſtituted either a piece of black cloth or black velvet, it would ſo damp the incident rays, that the ſaid place would be leſs illumin'd than before, when it receiv'd its light only from the weak and oblique reflections of the floor and walls of a pretty large room; over which the beams that came in at the hole, were confuſedly and in a broken manner diſpers'd.

(5.) And to ſhew that the rays which fall on black bodies, as they do not rebound outwards to the eye, ſo they are reflected towards the body itſelf, as the nature of thoſe erect particles to which we have imputed blackneſs requires; we ſhall add an experiment, that will at the ſame time confirm our doctrine of whiteneſs. We took, then, a broad and large tile, and having whited over one half of its ſuperficies, and black'd the other, we expos'd it to the ſummer ſun. And having let it lie there a convenient time, we found that whilſt the whited part of the tile remain'd cool, the black'd part of it was grown very hot. And for further ſatisfaction, we have ſometimes left upon the ſurface of the tile a part that retain'd its native red; and expoſing all to the ſun, we obſerv'd the latter to have contracted a heat in compariſon of the white part, but inferior to that of the black. 'Tis alſo remarkable, that rooms hung with black are not only darker than they would be otherwiſe, but warmer too. I have known a great lady, of a tender conſtitution, complain that ſhe commonly took cold upon going into the air, after having made any long viſit to perſons whoſe rooms were hung with black. And this is not the only lady I have heard complain of the warmth of ſuch rooms; which, tho' perhaps it may partly be imputed to the effluvia of thoſe materials wherewith the hangings were dyed, yet probably the warmth in this caſe depends chiefly upon the ſame cauſe with darkneſs; for upon expoſing two pieces of ſilk, the one white, the other black, in the ſame window to the ſun, I have often found the former conſiderably heated, when the latter has remain'd cool.

(6.) A virtuoſo of unſuſpected credit acquainted me, that in a hot climate he had, by carefully blacking the ſhells of eggs, and expoſing them to the ſun, ſeen them thereby well roaſted, in no long time. But in *England*, the ſun's rays ſeem not to be ſufficiently ſtrong to produce ſuch an effect; for having expos'd eggs in the ſummer ſeaſon thereto, they acquired indeed a conſiderable degree of heat, but not enough to roaſt them.

(7.) Laſtly, our conjectures about the nature of blackneſs, may be ſomewhat confirmed by the obſervation of the blind man, formerly mention'd, who diſcerns colours with his fingers; for he ſays, that he feels a greater roughneſs upon the ſurfaces of black bodies, than upon thoſe of red, yellow, or green. And *Bartholine* tells us, that a blind earl of *Mansfield* could diſtinguiſh white from black only by the touch; which might ſufficiently argue a great difference in the aſperities, or ſuperficial textures of the bodies of thoſe two colours; if the learned relator had affirm'd the matter upon his own knowledge. Let us next take in the aſſiſtance of our experiments, purpoſely made to bring us farther acquainted with the nature of white and black.

The nature of whiteneſs and blackneſs ſhewn by experiments.

1. Take any quantity of fair water, heat it, and add thereto as much good common ſublimate as it will diſſolve, or till ſome of it lie untouched at the bottom of the liquor; then filter this ſolution thro' cap-caper, and, to a ſpoonful or two of the clear, add four or five drops of good limpid ſpirit of urine: ſhake them together, and immediately the whole mixture will appear white, like milk. After this, if you preſently add a convenient proportion of rectify'd *Aqua fortis*, the whiteneſs will immediately diſappear, and the whole mixture become tranſparent; which you may, if you pleaſe, again reduce to a conſiderable degree of whiteneſs, by pouring thereinto more freſh ſpirit of urine. It is not neceſſary to employ either *Aqua fortis*, or ſpirit of urine, about this experiment; for we have made it with other liquors.

2. Make a ſtrong infuſion of bruis'd galls in fair water; and having filtered it into a clean vial, add more of the ſame fluid to it, till you have made it ſomewhat tranſparent, and ſufficiently diluted the colour for the credit of the experiment. In this infuſion, ſhake a convenient quantity of a clear, but very ſtrong ſolution of vitriol; and you ſhall immediately ſee the mixture turn black, almoſt like ink: and if, preſently after, you drop into this mixture a ſmall quantity of good oil of vitriol, and, by ſhaking the vial, ſuddenly diſperſe it thro' the two other liquors; you will ſee the dark colour of the whole preſently begin to diſſipate, grow clear, tranſparent, and loſe its inky blackneſs; which may be again reſtored by the affuſion of a ſmall quantity of a ſtrong ſolution of ſalt of tartar. And tho' both theſe atramentous liquors will ſeem very pale, if you write with a clean pen dipt in them; yet that is common to them, with ſome ſorts of ink, which prove very good when dry; as I have found, that when theſe were carefully made, what I wrote with either, eſpecially with the former, would, after a while, turn ſufficiently black. This experiment of deſtroying and reſtoring blackneſs, we have likewiſe try'd in common ink; tho' with this it ſucceeds not ſo well, and but very ſlowly; becauſe the gum uſually employ'd in making it, oppoſes the operations of the ſaline liquors. And tho' it be taken for granted, that bodies will not precipitate with alkalizate ſalts, which have not been firſt diſſolved in ſome acid menſtruum; yet I have found, upon trial, that many vegetables, barely infuſed, or but ſlightly boiled in common water, afford, upon the bare affuſion of a ſtrong and clear lixivium of pot-aſhes, a large quantity of coagulated matter; ſuch as I have had in the precipitations of vegetable ſubſtances, by means of acids; and that this matter was eaſily ſeparable from the reſt of the liquor; being left behind by it in the filtre. And, from the firſt ink mentioned in this experiment, I could, by filtration, ſeparate a conſiderable quantity of a very black pulverable ſubſtance. And when the ink was made clear again, by the oil of vitriol, the affuſion

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of diſſolved ſalt of tartar ſeem'd but to precipitate, and thereby unite, and render conſpicuous, the corpuſcles of the black mixture, that had been before diſperſed, into very minute and ſingly inviſible particles, by the reſolving power of the highly corroſive oil of vitriol. And that galls are not abſolutely neceſſary to make atramentous liquors, appears from the following experiment. We boil'd dry'd roſe-leaves for a while in fair water, and into two or three ſpoons-ful of the decoction, ſhook a few drops of a ſtrong and well filtred ſolution of vitriol; whereupon the mixture immediately turn'd black: and preſently ſhaking herein a juſt proportion of *Aqua fortis*, we changed it from a black to a deep red ink; which, by the affuſion of a little ſpirit of urine, may be reduced immediately to an opake blackiſh colour.

3. In theſe experiments, the infuſion of galls, the decoction of roſes, and the ſolution of vitriol, have each their own colour; but we may ſuddenly produce a blackneſs, by mixing an infuſion of orpiment, and a ſolution of minium, both whereof ſhall be limpid and colourleſs. And with theſe liquors may be exhibited a curious and ſurprizing phenomenon, if made and applied in the following manner: 1. Take of the ſtrongeſt unſlaked lime about two parts, of yellow orpiment one part, of fair water fifteen or ſixteen parts; beat the lime groſly, and powder the orpiment, with care to avoid the noxious duſt: and having put theſe two ingredients into the water, let them remain there for two or three hours; ſtirring the mixture from time to time. Thus you'll obtain a fetid liquor; the clear part whereof muſt be poured off from the reſt, or gain'd by the filtre. 2. In the mean time burn a piece of cork, and quench it, whilſt fired, for ſeveral times ſucceſſively in fair water; and having, by this means, reduced it to a coal, you may eaſily, by grinding it with a ſolution of gum-arabic in water, bring it to the colour and conſiſtence of a good black ink. 3. Take any quantity of red lead, and two or three times its weight of vinegar, or rather the weak ſpirit of it; and, putting the powder and that into a glaſs vial, let them infuſe in ſome conſiderably warm place for two or three hours, till the liquor has acquired a ſweet taſte. Matters being thus prepared, write what you pleaſe with a clean pen dipt in the ſolution of the red lead; which, if filtred, will prove ſo clear, as to be inviſible upon the paper. Over what is thus written, you may draw any characters or letters you pleaſe, with a pen dipt in the black ink made with cork. And, laſtly, to ſhew the experiment, dip a linen rag in the fetid ſolution of the lime and orpiment, which is alſo limpid, and draw it over the written paper; and this will at once both wipe out the ſtrokes of the black ink, and render all that was wrote with the inviſible ink conſpicuouſly black.

4. If pieces of white hart's-horn be, with a moderate degree of fire, diſtilled in a glaſs retort, they will, after the ſeparation of the phlegm, ſpirit, volatile ſalt, and the looſer and lighter parts of the oleaginous ſubſtance, remain one behind of a coal-black colour. And even ivory itſelf, when ſkilfully burnt, affords painters one of the beſt and deepeſt blacks

they have. Yet, in the inſtance of diſtill'd hart's-horn, the operation being made in glaſs veſſels, carefully cloſed, no extraneous black ſubſtance inſinuates itſelf into the white horn; but the whiteneſs is deſtroyed, and the blackneſs generated only by a change of texture made in the burnt body by the receſs of ſome parts, and the tranſpoſition of others. And tho' I remember not to have ever found the *Caput mortuum* of diſtilled hart's-horn to paſs from a black to a true whiteneſs, whilſt it continued in cloſe veſſels; yet, having taken out the coal-black fragments, and calcined them in open veſſels, I could, in few hours, quite deſtroy that blackneſs, and, without ſenſibly changing their bulk or figure, reduce them to a degree of whiteneſs: ſo much do theſe two colours depend upon the diſpoſition of the little parts that the bodies wherein they are to be met with, conſiſt of. And we find, that if white-wine tartar, or the white cryſtals of ſuch tartar, are burnt, without being truly calcined, the *Caput mortuum* will be black. But if the calcination be continued till the tartar is perfectly reduced to aſhes, and kept long enough in a ſtrong fire, the remaining calx will be white. And ſo we ſee, that not only other vegetable ſubſtances, but even white woods, as the hazel, will yield a black charcoal, and afterwards whitiſh aſhes. Thus alſo animal ſubſtances, naturally white, as bones and egg-ſhells, grow black, upon being burnt, and white again, when perfectly calcined.

5. Yet I much queſtion whether the rule, *aduſta nigra, peruſta alba*, will hold as univerſally as is preſumed; for I have ſeveral examples to alledge againſt it. By burning alabaſter ſo as to make it appear to boil almoſt like milk, and to reduce it to a very fine powder, it would not grow black at all, but retain its pure and native whiteneſs: and tho', by keeping it longer than uſual in the fire, I produced a faint yellow in that part of the powder which lay neareſt the top of the crucible; yet a curious and experienced ſtone-cutter told me he had found, that if alabaſter, or plaiſter of *Paris*, be very long kept in a ſtrong fire, the whole heap of burnt powder would exchange its whiteneſs for a much deeper colour than the yellow I obſerved. Lead calcined with a ſtrong fire, turns at length, to minium, whoſe colour we know is a deep red; and if this minium be again urged with a ſtrong fire, you will ſoon find a glaſſy brittle body, darker than minium, than any white calx or glaſs. 'Tis known among chymiſts, that the white calx of antimony, by a more vehement operation of the fire, may be melted into a glaſs; which we have obtain'd of a red colour far deeper than that of the calx of burnt antimony: and tho' common glaſs of antimony, being uſually adulterated with borax, have its colour thereby diluted, often to a very pale yellow: yet not only ours, made more genuinely, was, as we ſaid, of a colour leſs remote from black than the calx; but, by melting it once or twice more, we found the colour heighten'd. And if you burn blue unſophiſticated vitriol very ſlowly, and with a gentle degree of heat, you may obſerve, that when 'tis burnt only ſo far as to rub to powder betwixt your fingers, it will be of a white, or whitiſh colour: but if you proſe-

cute the calcination, this body will paſs thro' other colours, as a grey, a yellowiſh, and a red: and if you continue it in a long and vehement fire, by that time it comes to be thoroughly calcined, it will be of a dark purple, nearer to black, not only than the firſt calx, but than the vitriol, before it at all felt the fire. I might add, that *Crocus Martis*, made by the laſting violence of the reverberated flames, is not ſo near to white as the iron or ſteel that afforded it, before its calcination.

Theſe inſtances may ſuffice to ſhew, that minerals are to be excepted from the foremention'd rule; which, tho' it ſeldom fails in ſubſtances belonging to the animal or vegetable kingdom, may yet be ſuſpected even in ſome of theſe, if *Belonius* ſay true, that charcoal, made of the wood of oxycedar is white. And I could not find, tho' hart's-horn, and other white bodies, will turn black in retorts, by heat, that camphire would at all loſe its whiteneſs; tho' I have purpoſely kept it in ſuch a heat as made it melt and boil.

6. And tho' I could not, in cloſed glaſſes, blacken camphire by heat, but it would ſublime to the ſides and top of the veſſel in its natural form; yet being ſet on fire in the free air, it ſends out a great ſmoke: and having, purpoſely, upon ſome of it, whilſt flaming, clapt a large glaſs, almoſt in the form of a hive, with a hole at the top, it continued burning, ſo as to line all the inſide of the glaſs with a ſoot as black as ink; and in ſo great a quantity, that the cloſeneſs of the veſſel conſider'd, almoſt all that part of the camphire which took fire, ſeem'd to have been changed into that deep black ſubſtance.

7. I took rectified oil of vitriol, and, by degrees, mixed with it a convenient proportion of the eſſential oil of wormwood; and, warily diſtilling the mixture in a retort, there remain'd a ſcarce credible quantity of dry matter, black as a coal. And becauſe the oil of wormwood, tho' a chymical oil, and drawn by a virtuoſo, ſeem'd to have ſomewhat in it of the colour of the plant, I ſubſtituted in its room, the pure and ſubtile oil of winter-ſavory; and gradually mixing it with an equal weight of the ſame oil of vitriol, and diſtilling them, as before, in a retort, even theſe two clear liquors left me a conſiderable proportion of a ſubſtance black as pitch; which I keep by me as a rarity.

8. Take a little yellow wax, ſcraped, or thinly ſliced, and putting it into a convenient glaſs, pour to it a conſiderable quantity of ſpirit of wine; and, placing the veſſel in warm ſand, increaſe the heat by degrees, till the ſpirit of wine juſt begins to boil; and by continuing that degree of heat, you will quickly find the wax diſſolv'd: then taking it off, you may either ſuffer it to cool as haſtily as with ſafety to the glaſs it can, or pour it, whilſt yet hot, into a filtre of paper; and either in the glaſs where it cools, or in the filtre, you will ſoon find the wax and menſtruum together reduced into a white ſubſtance almoſt like butter; which, by letting the ſpirit exhale, will ſhrink into a much leſs bulk, but ſtill retain its whiteneſs. 'Tis a pretty phenomenon in working of this magiſtery of wax, that the yellowneſs vaniſhes, and neither appears in the

ſpirit of wine that paſſes limpid thro' the filtre, nor in the butter, if I may ſo call it, which is wax well and ſuddenly blanched.

9. We took two parts of common ſublimate, and one of tin-glaſs, both finely powdered, and exactly mixed; theſe we ſublimed together, by proper degrees of fire, and there aſcended a matter of a very peculiar texture, for the moſt part made up of very thin, ſmooth, ſoft, ſlippery plates, almoſt like the fineſt ſort of fiſh-ſcales; but of ſo lovely a white, inclining to pearl-colour, and of ſo curious and ſhining a gloſs, that they appeared, in ſome reſpects, little inferior to orient pearls, and, in others ſeem'd to ſurpaſs them. They were applauded for a ſort of the prettieſt trifles we had ever prepared to amuſe the eye. I will not undertake, that tho' a man can hardly miſs changing the colour of the tin-glaſs, that he ſhall at the firſt, or perhaps the ſecond trial, hit upon the right way of making this glittering ſublimate.

10. When, in *Aqua fortis*, we diſſolve a mixture of gold and ſilver, melted into one maſs, it uſually happens, that the powder of gold which falls to the bottom, as not being diſſoluble by that menſtruum, will not retain its own yellow, but appear of a black colour; tho' neither the gold, the ſilver, nor the *Aqua fortis* did before manifeſt any blackneſs. And many chymiſts, when they make ſolutions of minerals, are very glad to ſee a black powder precipitated to the bottom; taking it for a hopeful ſign, that thoſe particles are of a golden nature, which appear in a colour ſo common to gold, when ſeparated from other metals by *Aqua fortis*, tho' ſo obſtinate, that it is hard for the refiner to reduce the precipitated calx to its native colour. For tho' that may quickly be done by fire, which will make this gold look very charmingly, and is indeed one of the beſt ways in practice for the refining of gold; yet it requires a watchful eye, and competent skill, to give it ſuch a degree of fire, as will ſerve to reſtore its luſtre, without bringing it to fuſion. I remember, that having taken a flat bright piece of gold refined by a curious and skilful perſon, on purpoſe to try to what height of purity that metal could be brought by art; I found this very piece, as glorious as it look'd, being rubb'd a little upon fine clean linen, ſullied it with a kind of black. And the like I have obſerved in refined ſilver; which I therefore mention, becauſe I formerly ſuſpected, that the impurity of the metal might have been the only cauſe of what I have frequently obſerved in wearing ſilver-hilted ſwords, that where they rubb'd upon my clothes, when made of light-coloured cloth, they would quickly black them. And thus alſo I have found pens blacked nearly all over, upon carrying them about me in a ſilver ink-caſe. And as in theſe ſeveral inſtances of acquired blackneſs, the metals are worn off, or otherwiſe reduced to very minute parts, 'tis a circumſtance that deſerves to be remark'd.

11. That a ſolution of ſilver will dye hair of a black colour, is a known experiment, wherewith ſome perſons of greater curioſity than skill have unluckily made their friends very merry. And I lately diverted myſelf by an improvement of this obſervation; for having diſſolved ſome

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pure ſilver in *Aqua fortis*, and wholly evaporated the menſtruum, I caus'd a quantity of fair water to be poured upon the calx, two or three ſeveral times; and to be each time evaporated, till the calx was very dry, and all the greeniſh blue, that uſually appears in common cryſtals of ſilver, quite vaniſhed: after this, I made thoſe I had a deſign upon, to moiſten ſome part of their skin with their own ſpittle, and ſlightly rub the moiſtened parts with a little of the ſilver thus prepared; upon which they were ſurprized, to ſee that a ſnow-white body ſhould preſently produce a deep blackneſs there, as if the ſtains had been made with ink. This blackneſs could not, like that produced by ink, be readily waſhed away; but required a long time to be got off. And with the ſame calx, and a little fair water, we likewiſe ſtain'd the white hafts of knives with a laſting black, in thoſe parts where the calx was plentifully laid on; but where it was very thinly ſpread, the ſtain was not of quite ſo deep a colour.

An inquiry into the cauſe of blackneſs in the Negroes.

12. The cauſe of blackneſs, in whole nations of *Negroes*, has been long diſputed by learned men; who poſſibly had done well to have conſidered why ſome whole races of other animals, as foxes and hares, are diſtinguiſhed by a blackneſs unuſual to the generality of the ſame ſpecies. However, I ſhall freely acknowledge, that this inquiry ſeems more abſtruſe to me than it does to many others; becauſe, conſulting many authors, the accounts of voyages, and travellers, to ſatisfy myſelf in matters of fact relating to it; I have met with ſome things among them, which ſeem not to agree with the notion of the moſt claſſic writers upon this head. As it is, therefore, my preſent buſineſs to deliver rather matters hiſtorical, than theoretical, I ſhall annex ſome few of my collections, inſtead of a formal diſputation.

It is commonly preſumed, that the heat of the climates, inhabited by *Negroes*, is the cauſe of their colour; and this, principally, becauſe we plainly ſee, that mowers, reapers, and other country people, who ſpend the moſt part of the ſummer-days in the heat of the ſun, have the skin of their hands and faces, which are the parts immediately expoſed to his rays, of a dark colour, and tending to blackneſs. On the contrary, we obſerve, that the *Danes*, and other inhabitants of cold climates, and even the *Engliſh*, who feel not ſo ſevere a degree of cold, have uſually whiter faces than the *Spaniards*, *Portugueze*, and other *Europeans* of hotter countries. But this argument ſeems far more ſpecious than convincing; for tho' the heat of the ſun may darken the colour of the skin, yet experience doth not ſhew, that heat alone is ſufficient to produce a diſcolouration, which ſhall amount to a true blackneſs, like that of the *Negroes*. Beſides, in many parts of *Aſia*, under the ſame parallel, or in the ſame degree of latitude with the *African* regions, inhabited by blacks, the people are but tawny. And in *Africa* itſelf, many nations in the empire of *Ethiopia* are not *Negroes*, tho' ſituate in the torrid zone, and as near the equinoctial, as other nations, that are black. Again, I find not, by the beſt accounts, I could any where obtain of the *Weſt-Indies*, that, excepting, perhaps, one place, or two, of ſmall extent, there are any blacks, originally natives of any part of *America*; for the blacks, now there, were, by the *Europeans*, long ago tranſplanted thither: tho' it contains ſo great a variety of climates, and particularly reaches quite croſs the torrid zone, from one tropic to the other. 'Tis true, the *Danes* are a whiter people than the *Spaniards*; yet this may proceed rather from other cauſes, than the coldneſs of the climate; ſince the *Swedes*, and other inhabitants of thoſe cold countries, are not uſually ſo white as the *Danes*, nor whiter than other nations, in proportion to their diſtance from the pole. And, in the numerous train of an ambaſſador extraordinary from the *Ruſſian* emperor, I obſerved, tho' it were then winter, the colour of their hair and skins was far leſs whitiſh than that of the *Danes*, who inhabit a milder region; and rather, for the moſt part, of a darkiſh brown. And the phyſician to this ambaſſador told me, that, in *Muſcovy* itſelf, the generality of the people were more inclined to have dark-coloured hair, than flaxen; but ſeem'd to ſuſpect, that the true and ancient *Ruſſians*, ſome whereof he had met with in one of the provinces of that vaſt empire, were rather white, like the *Danes*, than any thing near ſo brown, as the preſent *Muſcovites*, whom he gueſſes to be deſcended from the *Tartars*, and to have inherited their colour from them.

But further, eminent authors inform us, that there are *Negroes* in *Africa*, not far from the cape of *Good-Hope*, and conſequently beyond the ſouthern tropic; and out of the torrid zone, about the ſame latitude to the north, there are many *American* nations, that are not *Negroes*; and wherein the inhabitants of *Candia*, ſome parts of *Sicily*, and even of *Spain*, are not ſo much as tawny. Nay, I find, by our lateſt accounts of *Greenland*, that the inhabitants there are olive-colour'd, or rather of a darker hue. But if the caſe were the ſame with men, and thoſe other kinds of animals, before mentioned; I ſhould offer a remark, as a conſiderable proof, that cold may do much towards making men white, or black. However it be, I ſhall ſet down the obſervation, as deſerving a place in the hiſtory of whiteneſs and blackneſs. It is affirmed by *Olaus Magnus*, and others, that in ſome parts of *Ruſſia* and *Livonia*, hares, foxes, and partridges, which were black before, or red, or grey, become white in the depth of winter, by reaſon of the great coldneſs thereof. And a virtuoſo, who lately travelled thro' *Livonia* to *Moſcow*, confirms this relation; adding, that himſelf had ſeen ſuch animals there, whilſt they were white, which the inhabitants aſſured him had been black, or of other colours, before the winter began, and that they would be ſo again, when that ſeaſon was over. But, for further ſatisfaction, I alſo conſulted one, who had, for ſome years, been an eminent phyſician in *Ruſſia*, and who, tho' he rejected ſome other traditions, that are generally believed concerning that country, told me, he ſaw no cauſe to doubt of this relation, as to foxes and hares; not only becauſe 'tis the common and unconteſted aſſertion of the natives, but alſo, becauſe he himſelf, in the winter, could never, that he remember'd, ſee foxes and hares of any other colour than white. And I myſelf, having ſeen a ſmall white fox brought out of

Ruſſia into *England* towards the latter end of winter, foretold thoſe who ſhew'd him me, that he would change colour in the ſummer; and accordingly, coming to look upon him in *July*, I found that his back and ſides, with the upper part of his head and tail, were already grown of a dark colour; the lower part of the head and belly ſtill continuing white.

Let me add, that were it not for ſome ſcruple, I ſhould think more than what *Olaus* relates confirm'd by *Olearius*, who, in his Account of *Muſcovy*, has this paſſage: "The hares are there grey, but in ſome pro-"vinces they grow white in the winter." And ſoon after—— "It is not "very difficult to find the cauſe of this change, which certainly proceeds "from the outward cold; ſince I know that even in ſummer, hares will "change their colour, if they be kept for a competent time in a cellar." But in the ſame page, this author affirms the like change of colour that happens to hares in ſome provinces of *Muſcovy*, happens to them alſo in *Livonia*; and yet immediately ſubjoins, that in *Curland* the hares vary not their colour in winter, tho' the two latter countries be divided only by the river *Dugna:* for it is ſcarce conceivable, how cold alone ſhould have, in countries ſo near, ſo very differing an effect; tho' not more ſtrange, than what is believed by thoſe who aſcribe the complexion of *Negroes* to the heat of the ſun, when they would have the river *Cenega* ſo to affect the *Moors*, that tho' on the north ſide they are but tawny, on the other ſide they are black. There is another opinion as to the complexion of the *Negroes*, not only embrac'd by many of the more vulgar writers, but by men of eminence and learning; who would have their blackneſs an effect of *Noah*'s curſe upon *Cham*. But tho' a naturaliſt may ſafely believe all the miracles atteſted by the holy ſcriptures, yet in this caſe to fly to a ſupernatural cauſe, will, I fear, look like ſhifting off the difficulty, inſtead of ſolving it; for we here enquire not into the firſt and univerſal, but the proper, immediate, and phyſical cauſe of the blackneſs in *Negroes*. Beſides, 'tis not expreſs'd in ſcripture, that the curſe meant by *Noah* to *Cham*, was the blackneſs of his poſterity; but 'tis plain this curſe was quite another thing, clearly expreſſing that he ſhould be a ſervant of ſervants, that is, a very abject ſervant to his brethren: which accordingly, in ſome meaſure, came to paſs, when the *Iſraelites*, the poſterity of *Sem*, ſubdued the *Canaanites* that deſcended from *Cham*, and kept them in great ſubjection. But how is blackneſs a curſe? for navigators tell us of black nations, who think ſo differently of their own condition, that they paint the devil white.

Blackneſs is not inconſiſtent with beauty, which depends not ſo much upon colour, as an advantageous ſtature, a comely ſymmetry of the parts of the body, and juſt features in the face; ſo that I ſee not why it ſhould be thought ſuch a curſe to the *Negroes*, unleſs, perhaps, they going naked in thoſe hot climates, the colour of their skin probably makes the ſunbeams more ſcorching to them, than they would prove to people of a white complexion. 'Tis very probable that the principal cauſe of blackneſs in *Negroes*, is ſome peculiar and ſeminal impreſſion; for black children,

dren, brought over into theſe colder climates, loſe not their colour. And credible authors inform us, that the offspring of negroes, tranſplanted out of *Africa* above a hundred years ago, ſtill retain the complexion of their progenitors; tho' poſſibly, in tract of time, it will decay: on the other hand, white people removing into very hot climates, have their skins, by the heat of the ſun, ſcorch'd into dark colours; tho' neither they nor their children are obſerv'd, even in the countries of negroes, to deſcend to a true black. Yet *Piſo* tells us, that betwixt the *Americans* and negroes, are generated a diſtinct ſort of men, which they call *Cabocles*; and that betwixt *Portugueze* men and *Ethiopian* women, he has ſometimes ſeen twins, one whereof had a white skin, and the other a black one, with other the like effects of ſeminal impreſſions, from whence they ſeem to proceed; ſince even organical parts may receive great differences from ſuch peculiar impreſſions, upon what account ſoever they came to be ſettled in the firſt individuals, from whom they are propagated to poſterity; as we ſee in the thick lips and flat noſes of moſt nations of negroes. And if we may credit what learned men deliver concerning the little feet of the *Chineſe*, the *Macrocephali*, taken notice of by *Hippocrates*, will not be the only inſtance we might apply to our preſent purpoſe. And on this occaſion it may not be impertinent to add what I have obſerved in other animals. Thus there is a ſort of hens which want rumps; and I have ſeen a perfectly white raven, both in bill and feathers; which I attentively conſider'd, for fear of being impos'd upon. A very ingenious phyſician has often told me of a young lady, a patient of his, who greatly complain'd of want of health, tho' there appear'd ſo little cauſe thereof in any reſpect, that he concluded it wholly imaginary, and adviſ'd only little journeys of pleaſure; when going once to viſit St. *Winifrid*'s well, the lady, who was a catholic, and devout in her religion, having continued a pretty while in the water, to perform ſome devotions, fix'd her eyes very attentively upon the red pebble ſtones, which, in a ſcatter'd order, made a large part of thoſe that appear'd thro' the water. A while after this, ſhe grew big, went her time, and was deliver'd of a child, whoſe skin was plentifully ſpeckled with ſpots of the colour and magnitude of thoſe ſtones: and tho' this child is many years old, yet ſhe ſtill retains them.

I have but two things here to add concerning the blackneſs of negroes; the one is, that the ſeat of that colour, ſeems to be only the thin outward skin; for I knew a young black, who, having been lightly ſick of the ſmall-pox or meaſles, had, in the places of the puſtules, whitiſh ſpecks left behind. And *Piſo* aſſures us, that upon diſſecting many negroes in *Brazil*, he found their blackneſs went no deeper than the cuticle, which being removed, the *cutis* appear'd as white as that of *European* bodies. And the like has been affirm'd to me by a phyſician, who diſſected a negroe in *England*. The other thing I ſhall here take notice of concerning negroes, is, that an intelligent acquaintance of mine, who keeps about three hundred, women and men, to work in his plantations in the *Indies*, told me their children came into the world almoſt of the like reddiſh colour

lour with our *European* children; and that in a few days after they appear'd black. Agreeable hereto is the account lately publiſh'd by a Jeſuit, one of the miſſionaries into *South-America*, who there baptiz'd ſeveral infants; which, when newly born, he ſays, were much of the ſame colour with *European* children, but within about a week begin to put on the hue of their parents. More full is the teſtimony of our countryman, *Andrew Battel*; who being ſent priſoner to *Angola*, lived there, and in the adjoining regions, near eighteen years; for he mentioning the *African* kingdom of *Longo* peopled with blacks, ſays, according to *Purchas*, "The children "in this country are born white, and change their colour in two days to "a perfect black. The *Portugueze*, in the kingdom of *Longo*, have ſome-"times children by the negro women; and the fathers are often deceiv'd, "thinking that when the child is born, it is theirs, and within two days "it proves itſelf the child of a negroe." And the ſame perſon has elſewhere a relation, which, if we may credit it, is very well worth our notice; ſince this, together with what we have formerly mention'd of ſeminal impreſſions, ſhews it poſſible that a race of blacks might be begun, tho' none of the ſons of *Adam* were for many precedent generations of that complexion. 'Tis ſurely as poſſible, that white parents may ſometimes have black children, as that *African* negroes ſhould ſometimes have laſtingly white ones; eſpecially ſince concurrent cauſes may more eaſily favour the production of the former, than, in the ſcorching heat of *Africa*, that of the latter. And I remember, what may a little countenance this aſſertion, that the poſſeſſor of the white raven I formerly mention'd, affirm'd to me, that in the neſt, out of which he was taken white, they found with him another young one, of as perfect a black as any common raven. But to come to the paſſage itſelf: "There are, ſays our author, ſpeaking of the "regions formerly mention'd, born in this country white children, which "is very rare among them, for their parents are negroes; and when any "of them are born, they are preſented to the king, and are call'd *Dondos*. "Theſe, who are as white as any white men, are the king's witches, "and are brought up in witchcraft, and always wait on the king. There "is no man that dares meddle with theſe *Dondos*; if they go to the mar-"ket, they may take what they liſt; for all men ſtand in awe of them. "The king of *Longo* hath four of them." Yet this country in our globes is placed almoſt in the midſt of the torrid zone; and our author elſewhere tells us, that the inhabitants here are ſo fond of their blackneſs, that they will not ſuffer the whites to be buried in their land; of which he annexes a particular example, that may be ſeen in his voyage, preſerv'd by the induſtrious *Purchas*. But it is high time to return to our experiments.

Whiteneſs produced in chymical preparations.

13. The way of producing whiteneſs by chymical precipitations, is well worth our obſerving; for thereby bodies of very different colours and natures, tho' diſſolved in various liquors, are all reduced to white powders. Thus we find, that not only crabs eyes, and pearls, but red coral, and minium, being diſſolved in ſpirit of vinegar, may be uni-

formly precipitated by oil of tartar into a white calx. Thus ſilver, and tin, ſeparately diſſolved in *Aqua fortis*, will the one precipitate itſelf, and the other be precipitated, by common ſalt-water into white powders; and ſo will crude lead, tin-glaſs, and quick-ſilver, diſſolved in *Aqua fortis*, and precipitated; and many of theſe powders may be made, at leaſt, as fair and white, if, inſtead of oil of tartar, they were precipitated with oil of vitriol. Nay, that black mineral antimony being reduced, by the ſalts that concur to compoſe common ſublimate, into the clear unctuous liquor, called rectify'd butter of antimony, will, as was before obſerved, by the bare plentiful affuſion of fair water, be ſtruck down into a ſnow-white powder; which, when well waſhed from its ſaltneſs, is term'd *Mercurius vitæ*; tho' the like powder may be made of antimony, without the addition of any mercury at all. And this whiteneſs commonly enſues, when ſpirit of wine, impregnated with thoſe parts of gums, or other vegetable concretions, ſuppoſed to abound with ſulphureous corpuſcles, is ſuddenly diluted with fair water. And on a tincture of benjamin, drawn with ſpirit of wine, and brought to be as red as blood, having procured ſome fair water, it preſently united with the tincture, and turn'd the whole mixture white, But if ſuch milky fluids be ſuffered to ſtand at reſt for a convenient time, they uſually let fall to the bottom a reſinous ſubſtance; which the ſpirit of wine, diluted, and weakned by the water, was unable any longer to ſuſtain. And ſomething of kin to this change of colour in vegetables, is that which chymiſts obſerve, in pouring acid ſpirits to the red ſolution of ſulphur, made with an infuſion of pot-aſhes, or ſome ſharp lixivium; when the precipitated ſulphur, before it ſubſides, immediately turns the red liquor white. But though moſt precipitated bodies are white, yet ſome are not. Gold, diſſolved in *Aqua regis*, whether precipitated with oil of tartar, or with ſpirit of ſal-armoniac, will not afford a white, but a yellow calx. Mercury alſo, though reduced to ſublimate, and precipitated with liquors, abounding in volatile ſalts, as the ſpirits drawn from urine, hart's-horn, and other animal ſubſtances; yet affords, as we formerly noted, a white precipitate: but, with ſome ſolutions, hereafter to be mentioned, it will let fall an orange-tawny powder. And ſo will crude antimony, if, being diſſolved in a ſtrong lixivium, you pour any acid liquor upon the ſolution, newly filtred, whilſt it is yet warm. And if, upon the filtred ſolution of vitriol, you pour a ſolution of lixiviate ſalts, there will ſubſide a large ſubſtance, very far from white, which the chymiſts call ſulphur of vitriol; ſo that the greateſt part of diſſolved bodies being, by precipitation, brought to white powders, and yet ſome affording precipitates of other colours, the reaſon of both the phenomena may deſerve to be inquired into.

Whether black receives no other colour, and white all colours?

14. Some learned moderns are of opinion, that the reaſon why whiteneſs and blackneſs ought to be called the two extreme colours, is, that black receives no other colour, but white very eaſily receives them all. And, not to diſpute about words, or expreſſions, the thing itſelf, that is

affirmed as matter of fact, ſeems to be true, in moſt caſes, but not in all. For tho' it be a common obſervation among dyers, that cloth which has once been thoroughly imbued with black, cannot well afterwards be dyed of a lighter colour; yet the experiments, lately delivered, may ſhew, that, were the change of colour in black bodies attempted, not by mixing thoſe of lighter colours with them, but by adding ſuch things, as are proper to alter the texture of thoſe corpuſcles, which contain the black; 'tis no difficult matter to effect. For inks of ſeveral kinds, may immediately be deprived of all their blackneſs; and thoſe made with log-wood, and red-roſes, be changed, the one into a red, the other into a reddiſh liquor. With oil of vitriol, I have ſometimes turn'd black pieces of ſilk into a kind of yellow; and though the taffaty were thereby made rotten, yet that no way prejudices the experiment; the change of black ſilk, into yellow, being never the leſs true, becauſe the yellow ſilk is not good. And as for whiteneſs, I think the general affirmation of its being ſo eaſily deſtroyed, or tranſmuted, by any other colour, ought not to be received, without ſome cautions, and reſtrictions. For tho', according to what I have formerly noted, lead is, by calcination, turn'd into that red powder, called minium; and tin, by the ſame means, reduced to a white calx; yet the common putty, inſtead of being, as it is pretended, and ought to be, only the calx of tin, is, by the artificers who make it, to ſave the charge of that metal, compoſed but of half tin, and half lead, if not far more lead than tin; and yet the putty, in ſpight of ſo much lead, is a very white powder, without diſcloſing any mixture of minium. And ſo, if you take two parts of copper, which is a high-coloured metal, and but one of tin, you may, by fuſion, bring them into a maſs, wherein the whiteneſs of the tin is much more conſpicuous, and predominant, than the reddiſhneſs of the copper. And a very honeſt man aſſures me upon his own experience, that if arſenic and copper be melted together in a due proportion, the arſenic will blanch the copper both within and without; and that this white mixture, skilfully kept upon the cupel, would preſently let go its arſenic, which made whiteneſs its predominant colour, and return to the reddiſhneſs of copper: ſo that ſome white mineral bodies may be very capable of eclipſing others, and of making their colour predominant in mixtures. And, as a further confirmation of this, I remember, that I had a lump of ſilver and gold, melted together, wherein, by the eſtimate of a very experienced refiner, there might be about a third part of gold; yet the yellow colour of that metal was ſo hid, that the whole maſs appeared to be ſilver; and, when rubbed upon the touch-ſtone, 'twas not eaſily diſtinguiſhed from the touch of common ſilver: tho' if I put a little *Aqua fortis*, upon any part of the white ſurface it had given the touch-ſtone, the ſilver, in the moiſtened part, being immediately taken up, and conceal'd by the liquor, the golden particles would preſently diſcloſe themſelves, and look as if pure gold alone had been rubbed upon the ſtone.

Colours depend not on the ſubſtantial forms of bodies.

15. Having ſcraped a piece of black horn, with a piece of glaſs, into many thin curled ſhavings, and laid a quantity of them together; I found, that the heap they compoſed, was white: and tho' if I laid it on a clean piece of white paper, its colour ſeem'd ſomewhat eclipſed by the greater whiteneſs of that body; yet if I laid it upon any thing very black, it appeared to be of a good white. This eaſy experiment ſeems very oppoſite to their doctrine, who would have colours flow from the ſubſtantial forms of bodies; and that of the chymiſts alſo, who aſcribe them to one, or other, of the three hypoſtatical principles: for tho', in our caſe, there was ſo great a change made, that the ſame body, without being, ſubſtantially, either increas'd, or leſſen'd, paſſes immediately from one extreme colour to another, even from black to white; yet this ſo great and ſudden change, is effected by a ſlight mechanical tranſpoſition of parts; there being no ſalt, ſulphur, or mercury, added, or taken away; nor any ſubſtantial form generated, and deſtroyed; the effect proceeding only from a local motion of the parts: which ſo vary'd their poſition, as to multiply their diſtinct ſurfaces, and qualify them to reflect far more light to the eye, than they could before they were ſcraped off from the entire body of the black horn.

The chymical doctrine of blackneſs confuted.

16. The chymiſts, we know, uſually aſſign, for the cauſe of blackneſs, the fuliginous ſteams of aduſt ſulphur; but many of the preceding experiments will abundantly confute this doctrine. If they here mean the fumes of common ſulphur, 'tis proper to remind them, that fuſion, or ſublimation, does not turn that ſubſtance black; but it thereby becomes rather more or leſs white; and, when fired, it affords ſo little ſoot, as ſcarce, in any degree, to blacken white paper; but the ſmoke of it rather blanches linen, and turns red roſes pale. Beſides, I have ſeen a fixed kind of ſulphur kept, for many weeks together, in a very violent fire, that did not, when cool'd, appear black, but of a true red. If, by ſulphur, they mean the ſulphureous principle; upon this ſuppoſition, torrify'd ſulphur ſhould afford more blackneſs, than moſt other concretes; wherein that principle is ſuppoſed more deficient. Yet ſpirit of wine, tho' totally inflammable, will not, by burning, diſcolour white paper, held over it; and the ſmoke of our *Tinby* coals has been found, rather to blanch, than blacken linen. To theſe particulars, many others, of the ſame kind, might be added; but we need only look back, for further ſatisfaction, to our way of making black inks, from ſubſtances of different colours; for how can it be ſaid, that, when the component liquors thereof are put together, actually cold, and continue ſo, after their mixture, there intervenes any new aduſtion of ſulphur, to produce the emergent blackneſs? And when, by inſtilling a few drops of oil of vitriol, *&c.* the blackneſs, produced in thoſe experiments, is preſently deſtroy'd; if the colour proceeded only from the ſulphureous parts, torrify'd, in the black bodies, I demand, what becomes of them, when the colour ſo ſuddenly diſappears? for it cannot reaſonably be ſaid, that all thoſe which ſufficed to make ſo great a quantity of black matter, ſhould

reſort to ſo very ſmall a proportion of the clarifying liquor, and be diluted by it, without giving it any blackneſs. And if it be ſaid, that the inſtilled liquor diſperſed thoſe black corpuſcles ; I demand how that diſperſion comes to deſtroy their blackneſs, but by ſuch a local motion of their parts, as deſtroys their former texture? This may be a matter of ſuch moment, in caſes like ours, that I remember I have, in a few hours, without addition, from ſoot itſelf, obtain'd a large quantity of cryſtalline ſalt, and of a tranſparent liquor; yet this black ſubſtance had its colour ſo altered, by the change of its texture from the fire wherewith it was diſtilled, that it did, for a great while, afford ſuch plenty of very white exhalations, that the receiver, tho' large, ſeem'd to be almoſt fill'd with milk. Secondly, But were it granted, as it is in ſome caſes not improbable, that bodies may receive a blackneſs from a ſooty exhalation, occaſion'd by the aduſtion of their ſulphur, or oily parts; yet this is applicable only to ſome particular bodies, and will afford us no general theory of blackneſs. For if, for example, white hart's-horn, included in veſſels well luted to each other, and expoſed to the fire, be ſaid to turn black, by the infection of its own ſmoke; I think I may juſtly demand, what makes the ſmoke, or ſoot itſelf, black, ſince no ſuch colour appeared before in the hart's-horn? And, with the ſame reaſon, when we are told, that torrify'd ſulphur makes bodies black; I deſire to be told alſo, why torrifaction makes ſulphur itſelf black? Nor will there be any ſatisfactory reaſon aſſigned of theſe queries, without taking in thoſe intelligible mechanical principles of the poſition and texture of the minute parts of the body, with regard to the light and the eye; which may ſerve the turn, in many caſes, where the aduſtion of ſulphur cannot be pretended; as in the apparent blackneſs of an open window, view'd at a diſtance, *&c.* in which, and many other caſes, formerly alledged, there appears nothing requiſite to the production of blackneſs, but a prevention of the incident rays of light, from being plentifully enough reflected to the eye.* In ſhort, the doctrine I here oppoſe, is pleaded for, as chymiſts commonly argue about qualities; who content themſelves to ſay in what ingredient of a mixed body a particular quality reſides, inſtead of explaining its nature.

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* For the production of black, Sir *Iſaac Newton* obſerves, that "the corpuſcles of the body muſt be much leſs than any of thoſe which exhibit colours. For at all greater ſizes, there is too much light reflected, to conſtitute blackneſs. And from hence, ſays he, may be underſtood, why fire, and the more ſubtile diſſolver, putrefaction, by dividing the particles of ſubſtances, turn them to black; why ſmall quantities of black ſubſtances impart their colour very freely, and intenſely, to other ſubſtances, to which they are applied; the minute particles of theſe, by reaſon of their very great number, eaſily overſpreading the groſs particles of others; why glaſs, ground very elaborately with ſand, on a copper-plate, till it be well poliſhed, makes the ſand, together with what is worn off from the glaſs and copper, become very black; why black ſubſtances do ſooneſt, of all others, become hot in the ſun's light, and burn; conſidering the multitude of refractions in a little room, and the eaſy commotion of ſuch very ſmall corpuſcles, *&c.*" See *Newton. Optic. p.* 235.

SECT. III.

I Here propoſe to throw together the miſcellaneous experiments I have made, with relation to colours.

Many changes of colour produced by one ſimple ingredient.

I know of no way more likely to convince the generality of men, how great a ſhare the variable texture of bodies may have, in making them appear of differing colours, than by ſhewing how the addition of a ſingle ingredient, that either is colourleſs, or at leaſt has not any of the colours to be produced, is immediately able, by introducing a ſecret change of texture, to make the body 'tis put to, appear ſometimes of one colour, ſometimes of another; according as the parts of the body wrought upon, are diſpoſed to receive ſuch a change, as modifies the incident rays of light, after the manner requiſite to make them exhibit a blue, a green, a red, or ſome other particular colour.

For this end, I made choice of the ſpirit of ſalt, as that which is very ſimple; and which, if it be not too highly rectified, may be had clear, and colourleſs. With this ſpirit, I proceeded to make the following experiments, upon ſeveral bodies, whoſe differing textures ſeem'd to fit them for my purpoſe.

1. Some drops of well-coloured ſyrup of violets, being let fall together upon a piece of white paper; if a third, or fourth part ſo much ſpirit of ſalt be mixed with them, the ſyrup will preſently become of a red colour, uſually inclining to purple.

2. But if the liquor to be acted on, be otherwiſe diſpoſed, 'tis poſſible, with ſpirit of ſalt, to turn it from a blue colour, not to a red, but to a green; as I have ſometimes done, by letting fall into a deep ſolution of filings of copper, made with an urinous ſpirit, as that of ſal-armoniac, juſt as many drops of ſpirit of ſalt, as were requiſite to produce the change intended. A very ſmall error, either in exceſs, or defect, may leave the mixture ſtill blue, or bring it to be colourleſs.

3. Upon a few drops of good ſyrup of violets, let fall two or three drops of good ſpirit of urine, hart's-horn, or the like; and when, by mixing them well, the ſyrup has acquired a fine green colour; by putting to it a little of the ſpirit of ſalt, and ſtirring it, you may turn the green ſyrup into a red.

4. If you put a quantity of red roſe-leaves, well dried, into a glaſs vial, almoſt full of fair water; and, ſoon after, put to them as much ſpirit of ſalt as will make the water pretty ſharp; you will quickly ſee, both that liquor, and the contained leaves, brought to a fine lovely red, which they will long retain. The like effect ſpirit of ſalt will have on ſome other vegetables of a ſtiptic, or of an aſtringent nature.

5. But if, by infuſing brazil-wood in fair water, you make a tincture of it, which you may much deepen, by dropping into it a little ſpirit of hart's-horn, or of urine; and you then put to it a little ſpirit of ſalt, it will preſently

ſently change from a deep reddiſh colour, to a colour far more pale, or rather yellow; ſo that the ſame ſpirit acting upon two vegetable tinctures, differently diſpoſed, draws out, and heightens redneſs in the one, and deſtroys it in the other.

6. If you make an infuſion of true *Lignum Nephriticum*, in ſpring water, it will appear of a deep colour, like that of an orange, when you place the vial between the window and your eye; and of a fine deep blue, when you look on it with your eye placed between it and the window: but if you ſhake into this liquor a few drops of ſpirit of ſalt, the blue colour will preſently vaniſh, and appear no more, in what light ſoever you view the vial; tho' the liquor will ſtill retain the orange-colour.

7. We took common writing ink, and having let fall ſeveral drops of it upon a piece of white paper; ſo that, when it grew dry in the air, ſome parts of the ink lay thick, and ſome thinner, upon the paper, whereon it ſpread itſelf: we then put a few drops of ſtrong ſpirit of ſalt, ſome on one part of the black'd paper, and ſome on another; and obſerved, that in thoſe places, where the ſpirit had been put, or to which it reached, the blackneſs was quite deſtroyed, and was ſucceeded by an unpleaſant kind of colour, that ſeem'd, for the moſt part, to participate of yellow and blue, neither of them good in kind.

8. If in ſpirit of ſalt, you diſſolve filings of ſteel, and ſlowly evaporate the filtred ſolution, it will ſhoot into a kind of *Vitriolum Martis*, which appears green, as well as that which chymiſts vulgarly make with oil of vitriol. And if you take theſe cryſtals, made with ſpirit of ſalt, and, when they are dry, keep them in a crucible; you will find, that even a moderate fire, if duly apply'd, will make them, in a ſhort time, exchange their green colour for a red, like that of the finer ſort of *Crocus Martis*: as, indeed, this operation makes them referable to that ſort of medicine.

9. We took ſome mercury, precipitated *per ſe*; and tho' crude mercury is not, as far as I have tried, ſoluble in our *Engliſh* ſpirit of ſalt; yet this red precipitate readily diſſolved in that liquor, without at all imparting its own colour to it. I alſo found, that red lead, being boil'd a while in good ſpirit of ſalt, the redneſs totally diſappear'd. So that the ſame agent which produces redneſs in ſeveral bodies, in our two caſes quite aboliſh'd it. Thus alſo, the reddeſt coral being diſſolved in our menſtruum, the redneſs vaniſhes, and the ſolution appears colourleſs.

10. Take ſmall filings of copper, and having poured thereon good ſpirit of ſalt, till it ſwim, about two fingers breadth over them; keep the vial in a pretty ſtrong ſand-heat, till you perceive the menſtruum has diſſolved a competent part of the metal: then warily take out the vial, and holding it between your eye and the light, you will perceive the ſolution of copper to be of a dark and troubled colour, often inclining to a deep, but muddy red.

11. But if you pour this ſolution into a wide-mouth'd glaſs, and let it ſtand for a competent time, the expoſed liquor will appear of a green, much finer than that of the cryſtals of iron.

12. Take the clear limpid ſolution of ſilver, or of mercury, made in *Aqua fortis*, and drop upon it ſome ſpirit of ſalt; and you will find the clear liquor turn'd white as milk, which, after a while, will let fall a precipitate of the ſame colour.

13. And if, inſtead of a ſolution of ſilver, or quick-ſilver, you take a red ſolution, or tincture, of benjamin, or of the reſinous part of jalap root; you'll alſo have, upon the affuſion of ſpirit of ſalt, a white liquor, and a precipitate of the ſame colour.

14. Being deſirous to produce two differing colours at once, by the ſame affuſion of ſpirit of ſalt; I infuſed ſome dry'd red roſe-leaves in fair water, till it had acquired a deep colour from them: to this infuſion, poured off warily, that it might be clear, I added a conſiderable proportion of the ſweet liquor, made by digeſting ſpirit of vinegar upon red lead; by which I knew 'twould be turn'd of a bluiſh green. Upon this almoſt opake liquor, I pour'd ſpirit of ſalt, which precipitated the lead that had been diſſolved in the ſweet liquor, into a very white powder, and gave the remaining liquor, well impregnated with particles of the roſe-leaves, a very fine and durable ſcarlet colour. And if the experiment be well made, you may barely, by ſhaking together, and confounding the white powder with the red liquor, make a carnation-colour, which will appear very fine and lovely, whilſt it laſts; but, in no long time, the two ſubſtances that compoſe it, ſeparate, by degrees, and appear each in its former place and colour.

15. We took ſome ſpirit of ſalt, that, having laid long upon filings of copper, had loſt the muddy tincture it firſt acquired, by being almoſt boil'd upon them. This liquor, that look'd like common water, we pour'd into a ſmall wide-mouth'd cryſtal glaſs, and leaving it in a window, it appeared, in forty minutes, to have acquired a colour, much like that of a *German* amethyſt; and ſeem'd to have no tendency to greenneſs. But in about three hours time, it appear'd of a lovely green.

16. Precipitate a ſtrong ſolution of ſublimate made in fair water, with a ſufficient quantity of oil of tartar, *per deliquium*; put the liquor and powder into a filtre of cap-paper, and when the water is run thorough, the precipitate will remain in the filtre; which is to be ſlowly dry'd. Then take it out of the filtre, in the form of a groſs powder, and having put it into a clear glaſs, let fall on it, warily, ſome drops of ſtrong ſpirit of ſalt; and, during the conflict that will be made, the little lumps of the precipitate will loſe all their former brick-duſt colour, and turn white; tho' afterwards they will appear diſſolved into a tranſparent liquor, wherein the orange-colour is quite aboliſhed.

17. Having calcined copper, without any addition, but of fire and water; we took a quantity of it, and having poured thereon about three or four times its quantity of good ſpirit of ſalt, we obtain'd a muddy reddiſh liquor, and a white powder, whoſe quantity bore a conſiderable proportion to the part that was diſſolved; in which part itſelf, by the affuſion of common water, and the action of the air, we afterwards produced more than one change of colour.

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18. We ſometimes took a ſpoonful of the dark brown, or ſomewhat reddiſh ſolution of copper; and having put it into a cylindrical vial, that the change of colour might appear the better, we poured on it two or three ſpoons-ful of highly rectify'd vinous ſpirit; and giving the glaſs a ſhake, to mingle them, we preſently had a lovely green liquor; which, when well ſettled, was very fair, and look'd as if it were a liquid emerald.

19. Having moiſten'd a ſmall part of ſome green taffaty-ribband, twice or thrice, with good ſpirit of ſalt, we ſuffer'd it to dry of itſelf, which it did in a ſhort time; and then found, that the part wetted was no longer of a green, but a blue colour. The ſame ſpirit preſently turn'd that part of a piece of black ribband, upon which we put two or three drops of it, to a colour not unlike what is call'd *Fueille morte.*

20. 'Tis uſual to ſee books covered with paper, that looks ſometimes of a greeniſh blue colour, bordering upon purple; and ſometimes upon that of violets. The deeper coloured paper of this ſort, I have ſeveral times held in one hand, and with the other lightly touched it here and there with the end of a feather, dipped in ſpirit of ſalt; which almoſt in the twinkling of an eye, dy'd the touched parts of the paper of a lovely red, that would ſometimes long continue very vivid. And if, inſtead of a feather, I uſed a bruſh, dipt in the ſaline ſpirit, and made many drops at once fall upon the paper; 'twas pleaſant to ſee how ſuddenly it would be ſpeckled.

21. Upon well-powdered antimony, we pour'd three or four times its weight of good ſpirit of ſalt; and cauſed it to be boiled in this liquor, in a glaſs veſſel, wherein a part of it was diſſolved, and taken up into the menſtruum; when the antimony quite loſt its blackneſs. And this ſpirit of ſalt, thus impregnated, being dropped into fair water, the black mineral immediately ſubſided, in the form of a very white powder or precipitate.

Hence, by the way, it were not amiſs, if phyſicians, chymiſts, *&c.* who compound drugs, or other ingredients, would be leſs forward than they uſually are, to jumble ſeveral of them together either unneceſſarily, or without a due regard to their qualities, in reſpect of one another. For moſt of us are but too liable to be miſtaken, when we preſume before-hand to ſay what changes the coalition, or other aſſociations of differing bodies ſhall produce; eſpecially if they be either ſaline, or plentifully partake of a ſaline nature: ſince experience frequently ſhews, that by the action and re-action conſequent upon untry'd ways of compoſition, there ariſe in the mixture new and unlook'd-for conſiſtences and other qualities or accidents. And tho' it may ſometimes happen, that theſe new qualities ſhall prove advantageous, yet this may well be look'd upon but as a lucky chance; and it may ſtill be juſtly fear'd, that, ordinarily, ſuch accidental qualities of a medicine will prove to be either worſe than were expected, or at leaſt different from what was deſign'd, and conſequently leſs fit for the phyſicians or the artiſts purpoſe.

Experiments made in a darken'd room.

1. *Octob.* 11. About ten in the morning, the weather being ſun-ſhiny, we took ſeveral ſorts of ſtain'd paper, ſome of one colour, and ſome of another, and in a darkned room, whoſe window look'd ſouthward, we with them caſt the beams that enter'd at a hole about three inches and a half in diameter, upon a white wall that ſtood on one ſide, about five foot diſtance from them. The white gave much the brighteſt reflection. The green, red, and blue being compared together, the red gave much the ſtrongeſt reflection, and manifeſtly threw its colour upon the wall; the green and blue were ſcarce diſcernible by their colours, and ſeem'd to reflect an almoſt equal light. The yellow, compared with the two laſt, reflected ſomewhat more light. The red and purple being compared together, the former manifeſtly reflected much more light than the latter. The blue and purple compared together, the former ſeem'd to reflect ſomewhat the more light, tho' the purple colour was moſt viſible. A ſheet of very well ſleek'd marbled paper being apply'd as the others, did not caſt any of its diſtinct colours upon the wall, nor throw its light with an equal diffuſion thereon; but ſent the beams unſtain'd and bright to particular parts thereof, as if its poliſh had given it the nature of a ſpeculum. But comparing it with a ſheet of white paper, we found the reflection of the latter to be much the ſtronger; this diffuſing almoſt as much light to a conſiderable extent, as the marble paper did to one part of the wall. The green and purple left us ſomewhat in ſuſpence which reflected the moſt light; only the purple ſeem'd to have ſome little advantage over the green, which was dark in its kind.

2. Though a darken'd room be generally thought requiſite to make the colour of one body appear by reflection from another that is not ſuppoſed to act as a ſpeculum; yet I have often obſerv'd, that when I wore waſtcoats lined with ſilk that was very gloſſy and vividly colour'd, but eſpecially if red, I could in an enlighten'd room plainly diſcern the colour upon the pure linen that came out at my ſleeve; as if that fine white body were more of a mirror than colour'd and unpoliſh'd bodies are uſually ſuppoſed.

3. Holding ſtain'd ſheets of paper, ſometimes one, and ſometimes another, before the hole of the window in the darken'd room, betwixt the ſun and the eye, with the colour'd ſides obverted to the ſun, we found them ſingly to be ſomewhat tranſparent, and to appear of the ſame colour as before, only a little alter'd by the great light they were placed in: but applying two of them one over another to the hole, the colours were compounded as follows: The blue and yellow ſcarce exhibited any thing but a darker yellow, which we aſcribed to the coarſeneſs of the blue paper, and its darkneſs in its kind. For applying the blue parts of the marbled paper with the yellow paper after the ſame manner, they exhibited a good green. The yellow and red look'd upon together, gave us but a dark red, a little inclining to an orange-colour. The purple and red view'd together, appear'd more deeply ſcarlet. The purple and yellow made an orange. The green and red made a dark orange-tawny. The

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4. The triangular priſmatic glaſs being a very commodious inſtrument to ſhew the nature of colours, we thought proper to obſerve the ſeveral reflections and refractions which the rays of light undergo in falling upon or paſſing through it. And this we imagined might be beſt done, not in an ordinary enlighten'd room, where even the curious have left particulars unregarded; but in a darken'd one, where, by placing the glaſs in a convenient poſture, the various reflections and refractions may be diſtinctly obſerv'd; and where it may appear what rays are untinged, and which they are that paint upon the bodies whereby they are ſtopt, either the primary or ſecondary iris. In purſuance hereof, we obſerv'd in a darken'd room four reflections and three refractions, that were afforded us by the ſame priſm. And notwithſtanding the rules of catoptrics and dioptrics, we thought it would not be amiſs to find, by covering ſometimes one part of the priſm, and ſometimes another, and obſerving where the light or colour vaniſhed thereon, by which reflection and by which refraction each of the ſeveral places where the light falling, or paſſing through the priſm, and appearing either pure or ting'd, gave the phenomena. But becauſe theſe and other particulars which we obſerv'd, would be tedious, and not ſo intelligible to deliver in words, I refer to the ſcheme, where all of them may be taken in at one view.

Fig. 1.

P P P an equilatero-triangular cryſtalline priſm, one of whoſe edges P is placed next the ſun.

A B and α β, two rays from the ſun falling on the priſm, at B β; and thence partly reflected towards C and γ, and partly refracted towards D and δ.

B C β γ, thoſe reflected rays.

B D and β δ, thoſe refracted rays, which are again partly refracted towards E and ε, and there repreſent an iris 12345, denoting the five ſucceeding colours, red, yellow, green, blue, and purple, and partly reflected towards F and ζ.

D and F, and δ ζ, thoſe reflected rays, which are partly refracted towards G and η colourleſs, and partly reflected towards H and θ.

F H and ζ θ, thoſe reflected rays, which are refracted towards I and ι, and there repreſent another fainter iris; the colours of which are contrary to the former 54321, ſignifying purple, blue, green, yellow, red; ſo that the priſm in this poſture repreſents four rain-bows.

5. We obſerv'd in a room not darken'd, that the priſmatic iris might be reflected, without loſing any of its ſeveral colours, not only from a plain

looking-glaſs, and from the ſmooth ſurface of fair water, but alſo from a concave looking-glaſs; and that refraction did as little deſtroy thoſe colours, as reflection. For by the help of a large double-convex glaſs, thro' which we refracted the ſun's rays, we found that tho' one part of the iris might be made to appear either beyond or on this ſide of the other parts thereof; yet the ſame vivid colours would appear in the diſorder'd part as in the other. And by covering that ſide of the priſm obverted to the ſun, with an opake body, wherein only one ſmall hole was left for the light to paſs thro', having reduced the priſmatic iris, caſt upon white paper, into a very narrow compaſs, and view'd it thro' a microſcope, the colours appear'd the ſame, as to kind, they did to the naked eye.*

6. It may afford matter of ſpeculation, that as the colours of outward objects brought into a darken'd room, ſo greatly depend, in their viſibility, upon the dimneſs of the light they are there beheld by, that the ordinary light of the day being freely let in upon them, they immediately diſappear; ſo our experiments have ſhewn, that as to the priſmatic iris, painted on the floor by the beams of the ſun paſſing thro' a triangular glaſs, tho' the colours of it appear very vivid, even at noon-day and in ſun-ſhiny weather, yet by a more powerful light they may be made to diſappear. For having ſometimes taken a large metalline concave ſpeculum, and therewith caſt the converging beams of the ſun upon a priſmatic iris, which I had cauſed to be projected upon the floor; I found that the over-powerful light made the colours of the iris vaniſh: and if the light were ſo reflected as to croſs but the middle of the iris, the colours in that part only

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* The more general phenomena of the priſm, as obſerv'd by the illuſtrious Sir *Iſaac Newton*, are, in ſhort, as follow.

1. The rays of light paſſing thro' the priſm, paint an image of various colours on the oppoſite wall; the principal whereof are red, yellow, green, blue, and violet.

2. This image is not round, but the angle of the priſm being of 60 or 65 degrees, about five times longer than 'tis broad; the reaſon whereof was unknown, till Sir *Iſaac Newton* found it owing to the different refrangibility of the rays. See *Philoſ. Tranſact.* N° 80.

3. The rays which exhibit the yellow colour, are turn'd more out of their ſtrait courſe, than thoſe which make the red; thoſe which make the green, more than thoſe which make the yellow, *&c.* and thoſe which make the violet, moſt of all.

4. If the priſm thro' which the rays are tranſmitted, be turn'd about its axis, ſo that the red rays, the yellow, the green, *&c.* may fall thro' a ſmall hole, in order, upon another priſm, placed about twelve feet from the former, and be thrown to another part of the room, the yellow rays, for inſtance, tho' they fall in the ſame manner upon the ſecond priſm with the red, yet will not be thrown to the ſame place, but beyond them. And if at the place of the ſecond priſm, they be collected by a lens, the yellow rays, the green, *&c.* will each in their order, be thrown to a focus at a ſhorter diſtance than the red.

5. The colours of the coloured rays, when well ſeparated, can neither be deſtroy'd, nor in any manner changed by new refractions.

6. The colours of the coloured rays remain unchanged in paſſing thro' an enlighten'd ſpace, in mutually croſſing each other, in the confine of ſhadow, and in being reflected from any natural bodies in a place otherwiſe dark.

7. All the coloured rays collected together by ſeveral priſms, by a glaſs lens, or

only became inviſible; thoſe parts of the iris that were on the right hand and on the left of the reflected light, continuing to exhibit the ſame colours as before.

Objects view'd in different kinds of light.

7. I have ſometimes thought it worth while to take notice, whether the colours of opake bodies might not appear to the eye ſomewhat diverſify'd, as well by the nature of the lucid body that ſhines upon them, as by the diſpoſition of the ſuperficial parts of the bodies themſelves, and the poſition of the eye with regard to the object and the light: I have obſerv'd a manifeſt difference in ſome kinds of colour'd bodies view'd by day-light, and afterwards by the light of the moon. Several ſheets of the colour'd paper that had been view'd in the ſun-ſhine, being look'd upon at night by the light of a pretty large candle, the changes obſerv'd therein were theſe. The yellow ſeem'd much fainter than in the day, and inclinable to a pale ſtraw-colour. The red ſeem'd little chang'd, but reflected light more ſtrongly than the other colours. A fair deep green look'd upon by itſelf, ſeem'd to be a dark blue; but view'd together with a dark blue, appear'd greeniſh; and compared with a yellow, appear'd more blue than at firſt. The blue look'd more like a deep purple or murrey, than it had done in the day-light. The purple ſeem'd very little alter'd. The red compared with the yellow, made the latter look almoſt like brown cap-paper.

8. But to ſatisfy ourſelves whether the light of the candle were not unſincere, or ting'd with a yellow colour by its fuel; we took a pretty thick cylinder of a deep blue glaſs, and viewing the flame of the candle at a convenient diſtance through it, we perceived it to look green, which is the colour that emerges from the compoſition of opake bodies, one of them ſeparately blue, and the other yellow. And this perhaps may

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or a concave ſpeculum, or by any other way whatever, make white: and being again ſeparated after this union, every one exhibits its own colour.

8. If the ſun's rays fall upon the internal ſurface of the priſm in the moſt oblique manner that any rays can poſſibly be tranſmitted, thoſe reflected will be violet, and thoſe tranſmitted, red.

9. All natural bodies, eſpecially white ones, view'd thro' a priſm, appear on the one hand fringed with red and yellow; but with violet and blue on the other.

10. Two priſms being placed together, ſo that the red of the one and purple of the other may unite upon a convenient paper in the dark, the image will appear pale; and being view'd thro' a third priſm, applied at a due diſtance from the eye, it will appear as to objects, red and purple.

11. In like manner, if two powders, a perfect red and a perfect blue, be mix'd together, and laid thick upon ſome minute body, this body will appear double thro' a priſm, and of two colours, red and blue. See *Clark. Annot. in Rohault.* Ed. 3. p. 199.

We are farther to conſider, that by other means, light is found to be a body propagated in right lines; and that it ſpends about ſeven or eight minutes in paſſing from the ſun to the earth. "This was firſt obſerved "by Mr. *Roemer*, by means of the eclip-"ſes of the ſatellites of *Jupiter*. For "theſe eclipſes, when the earth is between "the ſun and *Jupiter*, happen about ſeven "or eight minutes ſooner than they ought "to do by the tables; and when the earth "is beyond the ſun, they happen about "ſeven or eight minutes later than they "ought to do: the light of the ſatellites "having farther to go in the latter caſe "than in the former, by the diameter of "the earth's orbit." *Newton. Optic.* p. 252.

be the chief reaſon why a ſheet of very white paper, view'd by candle-light, is not eaſily, at firſt, diſtinguiſhed from a light yellow, or lemon-colour; white bodies, as we formerly obſerved, being more of a ſpecular nature, becauſe tho', when unpoliſhed, they exhibit not the ſhape of the luminary that ſhines on them; yet they reflect its light more ſincere and undiſturbed, by either ſhades or refractions, than bodies of other colours do.

9. We took a piece of leaf-gold, and with the edge of a knife, lightly moiſten'd, laid upon the edge of the gold, we ſo faſten'd it thereto, that it continued extended, like a little flag; which, being held very near the eye, and obverted to the light, appear'd ſo full of pores, that it ſeem'd to have ſuch a kind of tranſparency, as that of a ſieve; but the light which paſſed thro' theſe pores was in its paſſage ſo temper'd with ſhade, and modify'd, that the eye diſcerned no more a golden colour, but a greeniſh blue. And, for further ſatisfaction, we, in the night, look'd upon a candle, thro' ſuch a leaf of gold; and, by trying the effect of ſeveral proportions of diſtance betwixt the leaf, the eye, and the light, we quickly hit upon ſuch a poſition for the gold, wherein the flame, view'd thro' the leaf, appeared of a greeniſh blue, as in the day-time. But the like experiment, try'd with a leaf of ſilver, did not ſucceed.

Experiments with the tincture of Lignum Nephriticum.

10. Druggiſts have a wood they call *Lignum Nephriticum*; becauſe the inhabitants of the country where it grows, uſe the infuſion of it, made in fair water, againſt the ſtone in the kidneys: and, indeed, an eminent phyſician of my acquaintance, who has very particularly enquired into that diſeaſe, aſſures me, he found ſuch an infuſion one of the moſt effectual remedies he ever try'd, againſt that formidable diſeaſe. The moſt ancient account I have met with of this ſimple, is given us by *Monardes*, who ſays, "We have a thick, ſmooth kind of wood, "brought us from *New Spain*, which has long been uſed in the diſeaſes "of the kidneys and urinary paſſages. To prepare a medicine from "it, we ſteep its ſhavings in clear water, and there ſuffer them to "remain, till the whole liquor is drank up; but when it has thus "ſtood for half an hour, the water appears of a blue colour, which "gradually increaſes, the longer the infuſion is continu'd; tho' the wood "itſelf be white." This wood will afford us an experiment, which, beſides the ſingularity of it, may greatly aſſiſt to diſcover the nature of colours. The experiment, as we made it, is this. Take a handful of the thin ſlices of *Lignum Nephriticum*, and put them into two, three, or four pound of the pureſt ſpring-water; let them infuſe there for a night, tho' a much ſhorter time may ſuffice; decant the impregnated water into a clear glaſs vial; and if you then hold it directly between the light, and your eye, you will ſee it tinged, (excepting the very top of the liquor, where a ſky-coloured circle ſometimes appears) of an almoſt golden colour, if the infuſion be not made too ſtrong of the wood; for, in that caſe, it will, againſt the light, appear ſomewhat dark and reddiſh, and requires to be diluted by the addition of fair water. But if you hold it from the

light, ſo that your eye be placed betwixt that and the vial, the liquor will appear of a deep lovely blue colour; as will alſo the drops, if any lie on the outſide of the glaſs.

And thus far we have try'd the experiment, and found it to ſucceed, even by candle-light. But if you hold the vial over-againſt your eye, ſo that it may have a window on one ſide, and a dark part of the room before it, on the other ſide; you will find the liquor, partly of a bluiſh, and partly of a golden colour. And if, turning your back towards the window, you pour out ſome of the liquor towards the light, and towards your eye; it will ſeem, upon coming out of the glaſs, to be perfectly blue; but when it is fallen down a little way, the drops may appear party-colour'd, according as the rays of light more or leſs fully penetrate, and illumine them. If you take a baſon, about half full of water, and having placed it ſo in the ſun-beams, that one part of the water may be illumined thereby, and the other part of it darken'd by the ſhadow of the brims of the baſon; and then drop of our tincture, made ſomewhat ſtrong, both into the ſhaded and illuminated parts of the water, you may, by viewing it from ſeveral places, and by a little agitation of the water, obſerve many pleaſing phenomena. If a little of this tincture be poured upon a ſheet of white paper, ſo that the liquor may remain of ſome depth upon it, you will perceive the adjacent drops to be partly of one colour, and partly of another, according to the poſition of your eye, with regard to the light: but if you pour off all the liquor, the paper will ſeem dy'd almoſt of a yellow colour. And if a ſheet of paper, with ſome of this liquor on it, be placed in a window, where the ſun-beams may come freely to it; then, if you turn your back to the ſun, and take a pen, or ſome ſuch ſlender body, and hold it a-thwart, betwixt the ſun and the liquor, you may perceive, that the ſhadow projected by the pen, upon the liquor, will not, all of it, be a common dark ſhadow, but, in part, curiouſly coloured; that edge of it next the body, which makes it, being almoſt of a lively golden colour; and the remoter verge of a blue one. Theſe, and other phenomena, which I have obſerved in this delightful experiment, many of my friends have beheld with wonder. And I remember an excellent oculiſt, finding, by accident, a vial full of this liquor, and having never heard of the experiment, continued apprehenſive, upon viewing it for a long time, that ſome ſtrange new diſtemper had invaded his eyes. And, I confeſs, the oddneſs of the phenomena made me very ſollicitous to find out the cauſe thereof. But tho' I am far from pretending to have diſcovered it; yet my inquiries have enabled me to give ſome conſiderable hints about it. In the firſt place, then, obſerving that this tincture, if it were too deep, kept the colours from being ſo lively, and their change from being ſo diſcernible; and finding alſo, that the impregnating virtue of this wood did, by being frequently infuſed in freſh water, gradually decay, I conjectured, that the tincture afforded by the wood, muſt proceed from ſome ſubtile parts, drawn out by the water; which, ſwimming about therein, ſo modify'd the light, as to exhibit particular colours. And becauſe theſe ſubtile parts were ſo eaſily ſoluble, even in cold water, I concluded, that they muſt abound with ſalts, and perhaps contain much of the eſſential ſalt of the wood. And, to try whether theſe ſubtile parts were volatile enough to be diſtilled, without diſſolving their texture, I carefully ſubmitted ſome of the tinged liquor, in very low veſſels, to the gentle heat of a lamp-furnace; but found all that came over, to be as limpid and colourleſs as rock-water: whilſt the liquor remaining behind, was of ſuch a deep blue, that it muſt be oppoſed to a very ſtrong light, to appear of any other colour. I took, likewiſe, a vial, fill'd with ſpirit of wine, and a little ſalt of hart's-horn, and found, that there was a certain proportion to be met with, betwixt that liquor, and the ſalt, which render'd the mixture fit to exhibit ſome little variety of colours, not obſervable in ordinary liquors, as it was variouſly ſituated, with regard to the light and the eye: but this change of colour came very far ſhort of that we had admired in our tincture. However, I ſuſpected that the tinging particles abounded with ſalts, whoſe texture, and the colour thence ariſing, would probably be altered by piercing acids; and pouring into a ſmall vial of the impregnated water a very little ſpirit of vinegar, I found, that the blue colour immediately vaniſhed, while the golden one remained: and which way ſoever I turn'd the vial, either to, or from the light, I found the liquor conſtantly appear'd of a yellowiſh colour, and no other. Upon this, I imagin'd, that the acid ſalts of the vinegar, having been able to deprive the liquor of its blue colour, a ſulphureous ſalt, which is of a contrary nature, would mortify the ſaline parts of the vinegar, and deſtroy their effects. And, accordingly, having placed myſelf betwixt the window and the vial, and let fall into the ſame liquor a few drops of oil of tartar, *per deliquium*: I found, that immediately upon the diffuſion of this liquor, the impregnated water was reſtored to its former blue colour. And this oil of tartar, being very ponderous, and falling directly to the bottom of the vial; it was eaſy to obſerve, that for a little while the lower part of the liquor appeared of a deep blue, whilſt all the upper part retained its former yellowneſs; which it immediately loſt, as ſoon as the oil of tartar was diffuſed thro' the whole: and the liquor thus reſtored, being view'd, either againſt, or from the light, exhibited the ſame phenomena with the tinged water, before the adventitious fluids were poured into it.

Kircher ſpeaks of this nephritic wood, in the following manner: "'Tis a white *Mexican* wood, and tho', generally, ſuppoſed to communicate only a blue colour to water; yet we have found, by repeated experiments, that 'twill turn it into all kinds of colours. The "tree itſelf, 'tis ſaid, commonly grows to a large ſize, with a thick even "trunk, like a pear-tree, leaves reſembling thoſe of rue, and ſmall oblong yellow flowers, growing in cluſters. The plant is cold and moiſt, "and nearly of a middle temper. The wood of this tree, made into a "cup, turns the water put into it, firſt of a perfect blue colour, like the "bugloſs-flower; and the longer it ſtands herein, the deeper the colour

“ grows. But if this colour’d water be put into a glaſs globe, and expoſed to the light, there will not appear the leaſt ſign of blueneſs therein; but the liquor will be clear and limpid, like pure water. Yet if this globe of glaſs be turn’d to a more ſhady place, the whole liquor will appear of a pleaſant green; and if to a place ſtill more ſhaded, of a reddiſh colour. And thus ’twill change its hue, according to the objects whereto ’tis expoſed. But, after being placed in the dark, ’twill regain its own blue caſt.” In this account, I obſerve the following particulars: Firſt, the author calls it a white *Mexican* wood; tho’, not to mention that *Monardes* ſays, ’tis brought from *New Spain*, the wood that we have met with, and employ’d as *Lignum Nephriticum*, was not white, but, for the moſt part, of a dark colour, not unlike that of the ſadder-colour’d wood of juniper. ’Tis true, *Monardes* ſays, that the wood is white; and it is alſo affirmed, that what is of a ſadder colour, is adulterated, by being imbued with the tincture of a vegetable. But having enquired of the moſt eminent of our *Engliſh* druggiſts, he peremptorily deny’d it. And, indeed, having conſidered ſome of the faireſt round pieces of this wood, that I could procure, I took notice, in one or two of them, that ’twas the external part only that appeared white, whilſt the more internal part was of the other colour; the contrary whereto would, probably, have appeared, if the wood had been adulterated after the manner juſt mentioned. And I have, at preſent, by me, a piece of ſuch wood, which, for about an inch next the bark, is white, and then ſuddenly paſſes to the above-ſaid colour: and yet this wood, by the tincture it affords in water, ſeems to have its colour’d part genuine; for the white part appears, upon trial, to be much leſs endow’d with the tinging property.

Next, our author tells us, that the infuſion of this wood expos’d in a ſpherical vial to the light, looks like ſpring-water; and adds, that there is no tincture to be ſeen in it: but herein our obſervation and his do not agree; for the liquor, which oppos’d to the darker part of a room exhibits a ſky-colour, did conſtantly with us, when held againſt the light, appear yellowiſh or reddiſh, according as its tincture was more or leſs deep. So that if there be no miſtake in the caſe, his white nephritic wood, and the ſadder-colour’d one, which we employ’d, were not of the ſame nature. What he mentions of the cup made of this wood, we have not try’d; but as for what he ſays, that this wood tinges the water with all ſorts of colours, that is much more than any of thoſe pieces we have hitherto employ’d was able to do: the change of colours diſcernible in a vial full of water impregnated by any of them, and directed towards a place more light or obſcure, being far from affording any ſuch variety. And as for what he tells us, that in the dark this infuſion will reſume a blue colour, I wiſh he had inform’d us how he try’d it. But having ſometimes brought a round long-neck’d vial fill’d with the tincture of this wood into the darken’d room already often mention’d, and holding it ſometimes near the ſun-beams that enter’d at the hole, and ſometimes partly in them and partly out of them; varying alſo the poſition of the glaſs, and viewing it from ſeveral parts of the room, it diſclos'd a much greater variety of colours, than it does in an ordinary enlighten'd room: for beſides the uſual ones, it exhibited a red in ſome parts, and a green in others; with intermediate colours produced by the different degrees and odd mixtures of light and ſhade.

'Tis ſurprizing in this experiment, that the blue tincture may be deſtroy'd or reſtor'd, whilſt the yellowiſh or reddiſh one continues as it was. That ſalts are of a conſiderable uſe in ſtriking colours, the many experiments which the dyers trade affords us, will ſhew: and as far as we have hitherto try'd, thoſe liquors, in general, which greatly abound in acid ſalts, have the power of deſtroying the blue colour in this infuſion; as thoſe that abound in ſulphureous ones, have the virtue to reſtore it. And by the way, this obſervation hints to us a new and eaſy method of diſcovering in many liquors whether it be an acid or a ſulphureous ſalt that is predominant. That ſuch a diſcovery is often very difficult, and may frequently be of great uſe, he who is no ſtranger to the various properties and effects of ſalts, and knows of how great moment it is to be able to diſtinguiſh their tribes, will readily conceive.

To find whether an acid or a ſulphureous ſalt predominates in a liquor.

Now ſuppoſe I would try whether alum, tho' it be plainly a mix'd body, abounds rather with an acid than a ſulphureous ſalt, I turn my back to the light, and holding a ſmall vial full of the tincture of *Lignum Nephriticum* in my hand, which view'd in that poſition appears blue; then dropping into it a little of a ſtrong ſolution of alum made in fair water, and finding upon the affuſion and ſhaking of the whole, that the blueneſs formerly conſpicuous in our tincture preſently vaniſhes, I am thereby induced to ſuppoſe, that the ſalt predominant in alum is acid. But if, on the other hand, I would examine, whether ſalt of urine, or of hart's-horn, is rather of a ſalino-ſulphureous, than of an acid nature; I drop a little of the ſaline ſpirit of either, into the nephritic tincture; and finding that the blue colour is thereby rather deepened than deſtroyed, I collect, that the ſalts which conſtitute theſe ſpirits, are rather ſulphureous than acid. And, to ſatisfy myſelf yet farther in this particular, I take a ſmall vial of freſh tincture, and placing both it and myſelf, with regard to the light, as before; I drop into the infuſion juſt as much diſtilled vinegar, or other acid liquor, as will ſerve to deprive it of its blueneſs; then, without changing my poſture, I drop, and ſhake into the ſame vial, a ſmall proportion of ſpirit of hart's-horn, or urine: and finding, upon this, the tincture immediately to recover its blue colour, I am thereby confirmed in my opinion of the ſulphureous nature of theſe ſalts.

It is much doubted to what ſort of ſalt that which is predominant in quick-lime belongs; but we have been perſuaded to refer it rather to the lixiviate, than acid kind, by obſerving, that tho' an evaporated infuſion of it will ſcarce yield ſuch a ſalt as aſhes, and other alkaline bodies do; yet if we deprive our nephritic tincture of its blueneſs, by juſt ſo much diſtilled vinegar as is requiſite to make that colour vaniſh; the lixivium of quick-lime will, immediately, upon its affuſion, recal the baniſhed colour, tho' not ſo powerfully as either of the ſulphureous liquors, formerly mentioned. And I gueſs at the ſtrength of the liquors, thus examined, by the quantity of them; which is ſufficient to deſtroy, or reſtore the blue colour of our tincture. But whether as to liquors, wherein neither acid nor alkaline ſalts are eminently predominant, our tincture will enable us to conjecture any thing more, than that ſuch ſalts do not abound in them, I leave to further trial; for I find not, that ſpirit of wine, ſpirit of tartar, freed from acidity, or chymical oil of turpentine, have any remarkable power to deprive our tincture of its blue colour, or to reſtore it, when, upon the affuſion of vinegar, it has diſappeared.

Different colours obſerved in the ſame piece of glaſs.

11. We may alſo ſee in a mineral body, ſomething very near of kin to the changeable quality of the tincture of *Lignum Nephriticum*; for I have ſeveral flat pieces of glaſs of the thickneſs of ordinary panes for windows, one of which being interpoſed betwixt the eye, and a clear light, appears of a golden colour, not much unlike that of the moderate tincture of our wood: but being viewed, when the beams of light paſs not ſo much thro' it, as they are reflected from it to the eye, the yellow ſeems to degenerate into a pale blue, ſomewhat like that of a turquoiſe. And, what is ſtrange, if in a certain poſition, you hold one of theſe plates perpendicular to the horizon, ſo that the ſun may ſhine upon one half of it, whilſt the other half is ſhaded, you may ſee the part illumin'd, of a much fainter yellow than the ſhaded part, which will appear more richly coloured. And if the glaſs be not held perpendicular, but parallel to the horizon, you may ſee the ſhaded part of a golden colour, whilſt the other appears conſiderably blue: and as you remove any part of the glaſs, thus held horizontally, into the ſun-beams, or the ſhade, it will, in the twinkling of an eye, ſeem to paſs from one of thoſe colours to the other; and the rays paſſing thro' it, and received by a ſheet of white paper held near, colour it with a yellow, ſomewhat bordering upon a red: yet the glaſs may be ſo oppoſed to the ſun, as to throw a mixed colour upon paper, in ſome parts more inclined to yellow, and in others to a blue.

In making theſe experiments with glaſs, you muſt take notice, that as one of the ſides has its ſuperficial parts diſpoſed to reflect the blue colour, that ſide muſt be held next to the eye. I have myſelf made glaſſes proper to exhibit an experiment, not unlike the laſt mentioned, by laying ſome ſilver, very finely foliated, upon glaſs, and giving it, by degrees, a much ſtronger fire than is requiſite to tinge glaſs of other colours. And this experiment, tho' made without a furnace, is the more conſiderable; becauſe a ſkilful painter, who allow'd 'twas with ſilver he coloured his glaſſes yellow, told me, that, when to burn them, he lays on the plates nothing but a calx of ſilver, calcined without corroſive liquors, and temper'd with fair water, the plates are tinged of a fine yellow, that looks of a golden colour, which part ſoever be turn'd to, or from the light; whilſt we have found, more than once, that ſome pieces of glaſs, prepared after our manner, tho', when held againſt the light, they appeared of a tranſparent yellow; yet viewed with one's back turn'd to the light, exhibited an opake blue.

The ſimple and primary colours but few.

12. There are but few ſimple, and primary colours; from the various compoſitions whereof, all the reſt reſult: for tho' painters imitate the hues of thoſe numerous different colours, to be met with in the works of nature, and of art; yet I have not found, that to exhibit this ſtrange variety, they need employ any more than white, black, red, blue and yellow; theſe five, variouſly compounded, and re-compounded, being ſufficient to exhibit ſuch a variety, as thoſe, who are altogether ſtrangers to the painter's pallet, can hardly imagine. Thus black and white, differently mixed, make a vaſt number of lighter and darker greys; blue and yellow make a great variety of greens; red and yellow make orange-tawny; red, with a little white, makes a carnation; red, with an eye of blue, makes a purple: and, by theſe ſimple compoſitions again compounded, the skilful painter can produce what kind of colour he pleaſes; and a great many more than we have names for. But, to render the rules about the production of colours fit to be relied on, the corpuſcles, whereof the ſeveral pigments conſiſt, muſt be ſuch as do not deſtroy one another's texture; for, in caſe they do, the emerging colour may be very different from what would reſult from the mixture of other agreeing pigments of the ſame colours.

The ſun's light ſtain'd with the colours of tranſparent bodies, in paſſing thro' them.

13. It may alſo help to diſcover the nature of colours, to know, that the light of the ſun, paſſing thro' diaphanous bodies of different hues, may be tinged of the ſame compound colour, as if it proceeded from painters colours of the ſame denomination; tho' the latter be exhibited by reflection, and manifeſtly compounded of material pigments. Wherefore, to try the compoſition of colours, we provided ſeveral plates of tinged glaſs, which being laid, two at a time, upon one another; the object, view'd thro' them both, appeared of a compound colour: which agrees with what we deliver'd of looking againſt the light thro' paper of different colours. But we thought the experiment would be more ſatisfactory, if we procured the ſun-beams to be ſo tinged in their paſſage thro' plates of glaſs, as to exhibit the compound colour upon white paper. And tho', by reaſon of the thickneſs of the glaſſes, the effect was but faint, even when the ſun ſhone ſtrong; yet we eaſily remedy'd that, by collecting his rays, with a convex glaſs; which increaſed the light, at the point where they met, ſufficiently to make the compound colour very manifeſt upon the paper. By this means we obſerved, that the rays paſſing thro' blue and yellow, compoſed a green; that an intenſe and moderate red, did, with yellow, make different degrees of ſaffron, and orange-tawny; that green and blue made a colour partaking of both, like what ſome *Latin* writers call *Pavonaceus*; and that red and blue made a purple. To which we might add other colours that we produced by the combinations of glaſſes differently tinged, did I not want proper words to expreſs them in our language. And having expoſed four or five ſorts of coloured glaſs, and other tranſparent bodies to the ſun, and caſt the reflected light upon white paper, held near them; the light

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appeared not manifeſtly tinged, but as if it had been reflected from the impervious parts of a colourleſs glaſs; only that reflected from the yellow, was here and there ſtained with the ſame colour; as if thoſe rays were not all reflected from the ſuperficial, but ſome from the internal parts of the glaſs. And a skilful tradeſman, who makes ſuch coloured glaſs, told me, that whereas the red pigment was but ſuperficial, the yellow penetrated to the very midſt of the plate. But, for further ſatisfaction, we foliated a plate of *Muſcovy* glaſs, and laying on it a little tranſparent varniſh of a gold-colour, we expoſed it to the ſun-beams, ſo as to caſt them upon a proper body; on which the reflected light appearing yellow, manifeſted, that, being reflected from the ſpecular part of the ſelenites, it was tinged, in its return, with the colour of the tranſparent varniſh, thro' which it paſſed.

Apparent colours compound as the genuine.

14. We now proceed to ſome experiments made in favour of thoſe colours, that are taught in the ſchools, not to be real, but only apparent, and fantaſtical. And, upon trial, we found, that theſe colours might be compounded both with true and ſtable colours; and, with one another, as well as thoſe which are unqueſtionably genuine and laſting; and that the colours reſulting from ſuch compoſitions, would reſpectively deſerve the ſame denominations. For, firſt, having, by means of a glaſs priſm, thrown an iris on the floor; I found, that, by placing a blue glaſs at a convenient diſtance, betwixt the priſm and the iris, that part of the iris, which before was yellow, might be made to appear green; tho' not of a graſs-green, but more diluted and yellowiſh. And it ſeems not improbable, that the narrow greeniſh liſt, uſually ſeen between the yellow and blue parts of the iris, is made by the confuſion of thoſe two bordering colours. And tho' the want of a ſufficient livelineſs in either of the compounding colours, or a ſmall error in the manner of making the following trials, was enough to render ſome of them unſucceſsful; yet when all neceſſary circumſtances were duly conſidered, the event was anſwerable to our expectation. As a red and blue compound a purple, ſo I could produce the latter of the three colours, by caſting, at ſome diſtance from the glaſs, the blue part of the priſmatic iris, upon a lively red. And ſometimes, when I try'd this upon a piece of red cloth, that part of the iris, which would have been blue, and, if compounded with the red of the cloth, appeared of a fair purple; did, when I came to view it near, look very oddly, as if there were ſome ſtrange reflection, or refraction, or both, made in the hairs of which that cloth was compoſed. Caſting, likewiſe, the priſmatic iris upon a very vivid blue, I found that part of it, which would otherwiſe have been the yellow, appear green. But it may ſeem more ſtrange, that tho' this iris, being made by the refraction of light thro' a body that has no colour, muſt, according to the doctrine of the ſchools, conſiſt of the pureſt emphatical colours; yet even theſe may be compounded of one another, as well as real colours in the groſſeſt pigments. For I took, at once, two triangular glaſſes, and one of them being kept fixed in the ſame poſture, that the iris it projected on the floor might not waver, I caſt on the ſame floor another iris with the other priſm, and moving it to and fro, to bring what part of

the ſecond iris I pleas'd, to fall upon what part of the firſt I thought fit; we ſometimes, by this means, obtain'd a green colour in that part of the more ſtable iris that before was yellow or blue; and, frequently, by caſting thoſe rays, that in one iris made the blue, upon the red parts of the other, we produced a lovely purple; which might be deſtroyed, or re-compoſed at pleaſure, by ſeparating and re-joining the edges of the two.

Experiments made with a coloured priſm.

15. Conſidering the priſm as the moſt uſeful inſtrument hitherto employ'd about colours, and that thoſe generally uſed, are made of glaſs, tranſparent and colourleſs; I thought proper to try what change the ſuperinduction of a colour, without deſtroying the tranſparency, would produce in the phenomena exhibited by it. But, being unable to procure one of coloured glaſs, and fearing alſo, that if it were not carefully made, the thickneſs of it would render it too opake; I endeavoured to ſupply its place by one of clarify'd roſin, or of turpentine brought to the conſiſtence of a tranſparent gum. Our attempts this way were not wholly loſt indeed, yet we found it ſo difficult to give theſe materials their true ſhape, that we choſe rather to varniſh over an ordinary priſm with ſome tranſparent pigment; and this we did, firſt with a yellow, and then with a red, or crimſon one, made of lac, temper'd with a convenient oil: the event was, that, for want of good tranſparent colours, of which there are but very few, both the yellow and the red rendered the glaſs ſo opake, tho' prudently laid on, that, unleſs I look'd upon ſome very vivid object, I could ſcarce diſcern any colours at all, eſpecially when the glaſs was covered with red. And, upon viewing ſuch objects, I found, that the colours of the pigment had vitiated, or drown'd, ſome of thoſe which the priſm would otherwiſe have exhibited; and, mixing with others, altered them. Thus, when covered with yellow, it made thoſe parts of bright objects, where the blue ſhould have been conſpicuous, appear of a light green. But the nature of the colours, the degree of tranſparency, and darkneſs in the pigment, with many other circumſtances, ſo vary'd the phenomena of theſe trials, that, till I can procure ſmall coloured priſms, or hollow ones, that may be filled with tinged liquors, or obtain ſome better pigments, than thoſe I was reduced to employ; I ſhall forbear to build any thing upon what I have done in this way: only deſire the inquiry may be further proſecuted.

Limpid liquors may afford coloured vapours.

16. There are ſome liquors which, tho' colourleſs themſelves, yet, when they come to be elevated, and diſperſed into exhalations, exhibit a conſpicuous colour; which they loſe again, when re-united into a liquor. Thus, good ſpirit of nitre, or ſtrong *Aqua fortis*, tho' they have no appearance of redneſs, whilſt in the form of a liquor; if a little heat chance to turn the minute parts of them into vapours, the ſteam will appear of a reddiſh, or deep yellow colour; which vaniſhes, when thoſe exhalations reſume the form of a liquor. And if you look upon a glaſs, half full of *Aqua fortis*, or ſpirit of nitre, and the other half full of ſteams, proceeding from either; you will ſee the upper part of the glaſs, of the colour juſt mentioned, if turn'd to the light. But, what is much more conſiderable, I have try'd,

that putting *Aqua fortis* in a long clear glaſs, and adding thereto a little copper, or ſome ſuch open metal, to excite heat and fumes; the light paſſing thro' thoſe fumes being caſt upon a ſheet of white paper, appear thereon of the colour of the fumes, when directly view'd; as if the light were as well tinged in its paſſage thro' theſe fumes, as it would have been by paſſing thro' ſome glaſs, or liquor, in which the ſame colour was inherent. Having, alſo, ſometimes obſerved, whether the beams of the ſun, near the horizon, paſſing thro' a very red ſky, would not exhibit the like colour; I found, that the rays falling in a room, upon a very white object, placed directly oppoſite to the ſun, diſcloſed a manifeſt redneſs, as if they had paſs'd thro' a coloured medium.

17. The reſulting of colours, upon the coalition of the particles of ſuch bodies, as were neither of them coloured like that mixture whereof they are the ingredients, is very well worth our niceſt obſervation; being of great ſervice, both in ſpeculation and practice. For the mechanical uſe of colours, among painters and dyers, very much depends upon knowing which of them are producible by mixing pigments of different hues. And 'tis of advantage to the naturaliſt to know how many, and which colours are primitive and ſimple; becauſe it both eaſes his labour, by confining his inquiry to a ſmall number of thoſe upon which the reſt depend; and aſſiſts him to judge of the nature of particular compound colours, by ſhewing, from the mixture of what more ſimple ones, and in what proportions to one another, the particular colour to be conſidered, reſults. But becauſe to inſiſt on the proportions, the manner, and the effects of ſuch mixtures, would oblige me to conſider a great part of the painter's art, and dyer's trade; I ſhall confine myſelf to mention ſeveral ways of producing green, by the compoſition of blue and yellow.

Several ways of producing a green, with blue and yellow.

Firſt, then, as painters make a green, by tempering blue and yellow, reduced to a ſoft conſiſtence, with water, oil, or ſome liquor of kin to theſe; I found, that by chuſing fit ingredients, and mixing them in the form of dry powders, I could do more this way, than if the ingredients were tempered with a liquor. But the blue and yellow powders, beſides being finely ground, muſt be ſuch, that the corpuſcles of the one may not be too unequal to thoſe of the other; leſt, by their diſproportionate minuteneſs, the ſmaller cover and hide the greater. We uſed, with good ſucceſs, a ſlight mixture of the fine powder of biſe, with that of orpiment, or good yellow oker, tho' an exquiſite mixture did not ſucceed ſo well; but, by lightly mixing the two in ſeveral little parcels, thoſe of them in which the proportion and manner of mixture was more luckily hit, afforded us a good green.

(2.) I have alſo learned, at the dye-houſe, that cloth, dy'd blue with woad, is afterwards, by the yellow decoction of *Luteola*, turn'd to a green.

(3.) We formerly ſaid, that having in a darkned room taken two bodies, a blue and a yellow, and caſt the light reflected from the one upon the other; we thereby obtained a green.

(4.) We alſo formerly obſerv'd a green to be produced, when in the ſame darken'd room we look'd at the hole where the light enter'd, thro' the green and yellow parts of a ſheet of marbled paper laid one upon the other.

(5.) We found too, that the beams of the ſun paſſing thro' two pieces of glaſs, the one blue, and the other yellow, laid upon one another, did upon a ſheet of white paper, on which they were made to fall, exhibit a lovely green: and other experiments have been before deliver'd to the ſame purpoſe.

(6.) We have likewiſe contrived a way of trying whether metalline ſolutions, tho' one of them had its colour adventitious, might not, by the mixture of the menſtrua employ'd, be made to compound a green, after the manner of other bodies. To a high yellow ſolution of pure gold in *Aqua regia*, I put a due proportion of a fine deep blue ſolution of crude copper, made in ſpirit of urine; and theſe two liquors, tho' at firſt they ſeem'd to coagulate each other a little, yet being thoroughly mix'd by ſhaking, they preſently united into a tranſparent green liquor; and ſo continued for ſeveral days; only letting fall a little blackiſh powder to the bottom. This experiment we more than once repeated with ſucceſs.

(7.) And laſtly, to try whether this way of compounding colours would hold even in ingredients actually melted by the violence of the fire, provided their texture were capable of enduring fuſion; we cauſed ſome blue and yellow ammel to be long and well wrought together in the flame of a lamp, which being ſtrongly and inceſſantly blown, kept them in ſome degree of fuſion; whence at length we obtain'd an amel of a green colour.

The manner wherein this colour may poſſibly be produced.

I have ſometimes conjectured, that the mixture of the biſe and the orpiment, lately mentioned, produced a green, by altering the ſuperficial aſperity which each of thoſe ingredients had a-part; ſo that the light falling on the mixture, was reflected with different ſhades, as to quantity or order, or both, from thoſe of either of the ingredients; and ſuch wherewith the light is modify'd, when it reflects from graſs or leaves, *&c.* that we call green. And ſometimes too I have doubted whether the green thus produced, might not be partly derived from hence; that the rays reflected from the corpuſcles of the orpiment giving one kind of ſtroke upon the retina, whoſe perception we call yellow, and thoſe reflected from the corpuſcles of the biſe giving another ſtroke upon the ſame retina, like objects that are blue; the contiguity and minuteneſs of theſe corpuſcles may make the appulſe of the reflected light fall upon the retina within ſo narrow a compaſs, that the part they ſtrike being as it were a phyſical point, they might give a compound ſtroke, and conſequently exhibit a new compound kind of ſenſation; as we ſee that two ſtrings of a muſical inſtrument being ſtruck together, give two ſounds, which arrive at the ear in the ſame time; and cauſe a differing ſenſation from either of them, and as it were one compounded of both. I ſhall only here farther obſerve, that the firſt of theſe ways of compounding a green, agrees much better with our conjectures about colours, than either with the doctrine of the ſchools, or with that of the chymiſts. For firſt, in the mixture of the two powders I could, by the help of an excellent microſcope, diſcover that what ſeem'd to the naked eye a green body, was but a heap of diſtinct, tho' very ſmall grains of yellow orpiment and blue biſe confuſedly blended together; whence it appears, that the colour'd corpuſcles of either kind did each retain its own nature and colour: ſo much may mere tranſpoſition and juxta-poſition of minute and ſingly unchanged particles of matter, contribute to produce a new colour. Secondly, the green thus made, being mechanically produced, there is no pretence to derive it from any unintelligible ſubſtantial form; nor does this green, tho' a real and permanent colour, ſeem to be a very inherent quality, as by that doctrine it ought to be; ſince each part of the mixture remains unalter'd in colour, and conſequently of a different one from the heap they compoſe: for, if the eye be aſſiſted by a microſcope, it no longer ſees a green body, but a heap of blue and yellow corpuſcles. And thirdly, I demand what ſulphur, ſalt, or mercury have to do in the production of this green? for neither the biſe nor the orpiment were of that colour before; and the bare juxta-poſition of the corpuſcles of the two powders that operate not upon each other, but might, if we had convenient inſtruments, be ſeparated unalter'd, cannot with any probability be imagin'd either to increaſe or diminiſh any of the three hypoſtatical principles.

The mixture of every yellow and every blue will not afford a green.

18. But it is not every yellow and every blue that will by their mixture afford a green. For in caſe either of the ingredients has a power to alter the texture of the other, ſo as to indiſpoſe it to reflect the light, as bodies that exhibit a blue or a yellow, reflect it; the emerging colour may not be green, but ſuch as the change of texture in the corpuſcles of one or both of the ingredients qualifies them to give. Thus, for inſtance, if you let fall a few drops of ſyrup of violets upon a piece of white paper, tho' the ſyrup will appear blue thereon, yet by mixing with it two or three drops of the above-mention'd ſolution of gold, I obtain'd a reddiſh mixture from the power of the acid ſalts in the ſolution. And having made a very ſtrong and high-colour'd ſolution of copper-filings with ſpirit of urine, tho' the menſtruum ſeem'd fully ſaturated with the metal; yet having let three or four drops of ſyrup of violets fall upon white paper, I found that the deep blue ſolution proportionably mix'd with this other blue liquor, made not a blue, but, upon account of the urinous ſalt in the menſtruum, a fair green.

The colours of the rain-bow exhibited in very thin ſubſtances.

19. To ſhew the chymiſts that colours may be made to appear or vaniſh where there happens no acceſſion or change either of the ſulphureous, the ſaline, or the mercurial principle of bodies, I ſhall not make uſe of the iris afforded by the glaſs priſm, nor of the colours to be ſeen in a fair morning in thoſe drops of dew, that in a convenient manner reflect and refract the rays of light to the eye; but remind them of what they may obſerve in their own laboratories. For all chymical eſſential oils, as alſo good ſpirit of wine, being ſhaken till they riſe in bubbles, thoſe bubbles appear with various fine colours, which all immediately vaniſh upon the

relapſing of the liquor that affords them their films, into the reſt of the oil or ſpirit of wine; ſo that a colourleſs liquor may immediately be made to exhibit a variety of colours, and loſe them in a moment, without the acceſſion or diminution of any of its hypoſtatical principles. And, by the way, 'tis worthy our notice, that ſome bodies, as well colourleſs as colour'd, by being brought to a great thinneſs of parts, acquire colours they had not before. For not to inſiſt on the variety of colours that water, render'd glutinous by ſoap, acquires, when blown into ſpherical bubbles; turpentine will, by being blown into after a certain manner, afford bubbles adorn'd with various colours; which tho' they ſoon vaniſh after the bubbles break, yet theſe would, probably, always variouſly exhibit colours upon their ſuperficies, if their texture were durable enough: for I have ſeen a perſon skill'd at faſhioning glaſſes, by the help of a lamp, blow ſome ſo vehemently, as to burſt them; whereupon we found the tenacity of the metal ſuch, that before it broke, it ſuffer'd itſelf to be reduced into films ſo extremely thin, that being kept clean, they conſtantly ſhew'd on their ſurfaces the varying colours of the rain-bow, which were exceedingly vivid. Taking, alſo, a feather of a convenient ſhape and bigneſs, and holding it at a due diſtance betwixt my eye and the ſun, when near the horizon, there appear'd to me a variety of little rain-bows, with different and very vivid colours, none of which were conſtantly to be ſeen in the feather. The like phenomenon I have at other times produced, by interpoſing, at a proper diſtance, a piece of black ribband betwixt the ſetting ſun and my eye.

Syrup of violets, and the juice of blue-bottles, by a change of colour, diſtinguiſh an acid from an alkali.

20. Drop a little good ſyrup of violets upon white paper, and on this liquor let fall two or three drops of the ſpirit either of ſalt, vinegar, or other eminently acid liquor; and upon the mixture of theſe, you ſhall find the ſyrup immediately turn'd red. But if inſtead of ſpirit of ſalt, or that of vinegar, you drop upon the ſyrup a little oil of tartar *per deliquium*, or of the ſolution of pot-aſhes, and ſtir them together with your finger, the blue colour of the ſyrup will in a moment be turn'd into a perfect green. And this ſyrup may be ſubſtituted for the infuſion of *Lignum Nephriticum*, when we would examine whether the ſalt predominant in a liquor, or other body, wherein 'tis looſe and in plenty, belongs to the tribe of acid ſalts. For if ſuch a body turns the ſyrup of a red or reddiſh purple colour, it for the moſt part argues that body to abound with acid ſalt. But if the ſyrup be turn'd green thereby, the predominant ſalt ſeems to be of a nature contrary to acid. For as ſpirit of ſalt, oil of vitriol, *Aqua fortis*, ſpirit of vinegar, juice of lemons, *&c.* will turn ſyrup of violets red or reddiſh; ſo I have found that not only all the volatile ſalts of animal ſubſtances which I have uſed, as ſpirit of hart's-horn, of urine, of ſal-armoniac, of blood, *&c.* but all the alkaline ſalts I have employ'd, as the ſolution of ſalt of tartar, of pot-aſhes, of common wood-aſhes, lime-water, *&c.* will immediately change that blue ſyrup into a perfect green. And by the ſame way, the changes that nature and time produce in the more ſaline parts of ſome bodies, may be diſcover'd;

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and ſuch bodies chymically prepared, as belong neither to the animal kingdom, nor to the tribe of alkalies, have their new nature ſucceſsfully examined. But to change the colour of this ſyrup, it requires that the body be more ſtrong of the acid or other ſalt predominant in it, than is neceſſary to work upon the tincture of *Lignum Nephriticum*: and tho' the actions of theſe contrary ſalts will deſtroy each other, yet neither of them will reſtore the ſyrup to its native blue, as in the nephritic tincture; but each of them changes it into the colour which itſelf affects.

21. By dropping on the freſh juice of blue-bottles, or the *Cyanus vulgaris minor*, a little ſpirit of ſalt, it immediately turn'd to a red. And if inſtead of the acid ſpirit, I mix'd with it a little ſtrong ſolution of an alkaline ſalt, it preſently diſcloſed a lovely green; the ſame changes being, by thoſe different ſorts of ſaline liquors, producible in this juice, as in ſyrup of violets. And finding this blue juice, when freſh, to be capable of ſerving for an ink of that colour; I attempted, by moiſtening one part of a piece of white paper with the ſpirit of ſalt, and another with ſome alkaline or volatile liquor, to draw a line on the paper after it was dry, that ſhould appear partly blue, partly red, and partly green: but the latter part of the experiment did not ſucceed well, tho' the blue and red were conſpicuous enough to ſurprize thoſe who were unacquainted with the trick. But leſt it ſhould be thought that volatile or alkaline ſalts change blue into green rather upon the ſcore of the eaſy tranſition of the former colour into the latter, than upon account of the texture, wherein moſt vegetables, that afford a blue, ſeem to be allied; I ſhall add, that having diſſolv'd blue vitriol in fair water, and put a lixiviate liquor and an urinous ſalt to diſtinct parcels of it, each of them turn'd the liquor not green, but of a deep yellowiſh colour, almoſt like that of yellow oker; which colour, the corpuſcles thereby precipitated, retain'd.

22. The hint of the following experiment was afforded us by the practice of ſome *Italian* painters, who counterfeit ultra-marine azure * by grinding

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* The *Pruſſian* blue is allow'd to excel the ultra-marine. And the preparation of it being very curious and uſeful, we ſhall here give an extract of that receipt for it which was lately communicated to the Royal Society by Dr. *Woodward*, as ſent him from *Germany*. Take of crude nitre and crude tartar, each four ounces; powder them fine, mix them together; and after decrepitation, there will remain four ounces of a ſalt of tartar: whilſt this is hot, pulverize it, and add thereto four ounces of well-dry'd ox-blood, in fine powder. Calcine the whole in a cover'd crucible, whereof it may fill two thirds. After this operation, lightly grind the matter in a mortar, and put it hot into four pints of boiling water; and let it continue boiling for half an hour. Strain the decoction, dilute the black remaining ſubſtance with water, and boil and ſtrain as before, till the water pour'd on becomes inſipid. Add the ſeveral ſtrainings together, and evaporate them to four pints. Then diſſolve an ounce of *Engliſh* vitriol, calcined white, in ſix ounces of rain-water, and filtre the ſolution. Diſſolve, likewiſe, half a pound of crude alum in two quarts of boiling water; and add this to the ſolution of the vitriol taken hot from the fire; pouring to them alſo the firſt lixivium, whilſt thoroughly hot, in a large veſſel. A great ebullition, and a green colour, will immediately enſue.

The production of a blue colour.

grinding verdigreaſe with ſal-armoniac and ſome other ſaline ingredients, and ſuffering them to lie for a good while together in a dunghill; for we ſuppoſed, that the change of colour wrought in the verdigreaſe by this way of preparation, muſt proceed from the action of certain volatile and alkaline ſalts, abounding in ſome of the mixed concretes, and brought to make a further diſſolution of the copper contained in the verdigreaſe; and therefore conjectured, that if both the verdigreaſe, and ſuch ſalts, were diſſolved in fair water, the ſmall parts of both being therein more ſubdivided and ſet at liberty, would have better acceſs to each other, and thence incorporate much the ſooner. And, accordingly, we found, that if, upon a ſtrong ſolution of good *French* verdigreaſe, we poured a juſt quantity of oil of tartar, and ſhook them well together, a notable change of colour immediately ſucceeded; the mixture growing thick, and not tranſparent; but if you ſtay till the groſſer part be precipitated, and ſettled at the bottom, you may obtain a clear liquor of an exceeding delightful colour. You muſt drop in a competent quantity of oil of tartar, otherwiſe the colour will not be ſo deep and rich; but if, inſtead of this oil, you employ a clear lixivium of pot-aſhes, you may have an azure, ſomewhat lighter or paler than the former. And if, inſtead of either of theſe liquors, you make uſe of ſpirit of urine, or of hart's-horn, you will, according to the quantity and quality of the ſpirit poured in, obtain ſome further variety of blue liquors; and, by the help of this urinous ſpirit, we have made a ſurprizing blue liquor. But theſe azure-coloured liquors ſhould be freed from the ſubſiding matter, which the ſalts of tartar, or urine, precipitate out of them, rather by being decanted, than by filtration; for, in the latter method, we have ſometimes found the colour of them very much impaired.

Of a red.

23. That roſes, held over the fume of ſulphur, may quickly be thereby deprived of their colour, and have as much of their leaves, as the fume works upon, turned pale, we have already hinted; and it is a known experiment. But it may ſeem ſtrange to one who has never conſidered the compound nature of brimſtone, that though the fume of it, as we

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enſue. Whilſt this ebullition continues, pour the mixture out of one veſſel into another, and afterwards let it reſt. Then run the liquor thro' a linen, and let the pigment remain in the ſtrainer; and when 'tis thus freed from its moiſture, put it, by means of a wooden ſpatula, into a ſmall new pot; pour upon it two or three ounces of ſpirit of ſalt, and there will immediately ariſe a moſt beautiful blue. Let the matter be well ſtirr'd, and ſet to reſt for a night; and afterwards thoroughly edulcorate it with repeated affuſions of rain-water; allowing a competent time for the precipitate to ſubſide. Thus, at length, it will become exquiſitely blue. Laſtly, let it drain upon the linen ſtrainer, and dry it gently for uſe. The ſucceſs of this proceſs greatly depends upon the calcination. The crucible is firſt to be ſurrounded with coals, that it may grow gradually hot, and the matter leiſurely flame and glow. This degree of heat muſt be continued till the flame and glowing decreaſe: then the fire ſhould again be raiſed, that the matter may glow with an exceeding white heat; and but little flame appear above the crucible. The lixivia muſt be vehemently hot; and ought to be mix'd together in an inſtant. See *Philoſ. Tranſact.* N° 381. p. 15—24.

PHYSICS. have ſaid, whitens the leaves of roſes, yet the oil of ſulphur *per campanam*, powerfully heightens the tincture of red roſes, and makes it more red and vivid; as we have eaſily try'd, by putting ſome red roſe-leaves, that having been long dry'd, had loſt much of their colour, into a vial of fair water: for a while after the affuſion of a convenient quantity of the oil, both the leaves themſelves, and the water they were ſteep'd in, diſcover'd a very freſh and lovely red colour.

What quantity of limpid liquor a pigment may tinge.

24. It may ſerve to illuſtrate not only the doctrine of pigments, and of colours, but many other parts of mechanical philoſophy, as odors, and other qualities are explain'd by the aſſiſtance of bodies extremely minute, to examine to how much of a colourleſs liquor, a very ſmall parcel of a pigment may impart a diſcernible colour. And tho' ſcarce any thing of exactneſs can be expected from ſuch trials, yet I preſum'd I ſhou'd hence be able to ſhew a much further ſubdiviſion of the parts of matter into viſible particles, than ſeems hitherto taken notice of, or imagin'd.

The moſt promiſing bodies, for ſuch a purpoſe, might ſeem to be metals, eſpecially gold, becauſe of the multitude and minuteneſs of its parts, which might be argued from the great cloſeneſs of its texture. But tho' we try'd a ſolution of gold, made in *Aqua regia* firſt, and then in fair water, yet becauſe we were to determine the pigment we employ'd, not by bulk, but weight; and becauſe alſo the colour of gold is but weak, in compariſon of that of cochineal, we rather choſe this to make our experiments with. But, from a number of theſe, it may ſuffice to ſelect one which was carefully made in veſſels conveniently ſhap'd; to which I ſhall only premiſe, that the cochineal will be better diſſolv'd, and have its colour far more heighten'd by ſpirit of urine, than by common water, or even rectified ſpirit of wine. One grain of cochineal, diſſolved in a conſiderable quantity of ſpirit of urine, and then further diluted by degrees with fair water, imparted a diſcernible colour to ſix glaſſes of water, each containing forty-three ounces and a half; which amounts to above one hundred twenty-five thouſand times its weight.

Acid, alkaline, and urinous ſalts, change the colours of many vegetable productions.

25. It may afford a conſiderable hint towards improving the art of dying, to know what change of colours are producible by the three ſeveral ſorts of ſalts we have mention'd, in the juices, decoctions, infuſions, and the more ſoluble parts of vegetables. The blue liquors lately made uſe of in our experiments, are far from being the only vegetable ſubſtances upon which acid, urinous, and alkaline ſalts, have the like operations to thoſe above recited. Ripe privet-berries, for inſtance, being cruſh'd upon white paper, tho' they ſtain it with a purpliſh colour, yet if we let fall on one part of it, two or three drops of ſpirit of ſalt, and on the other, ſome ſtrong ſolution of pot-aſhes, the former liquor immediately turns that part of the thick juice, or pulp, on which it fell, into a lovely red; and the latter changes the part whereon it falls, into a delightful green. This experiment is, perhaps, very extenſive, and ſerviceable to thoſe who wou'd know how dying ſtuffs may be wrought upon by ſaline liquors. For I have found it to ſucceed in ſo many various berries, flowers, bloſſoms, and other finer

parts of vegetables, that my memory will not ſerve me to enumerate them; and it is ſurprizing to ſee, by what differently colour'd flowers or bloſſoms; for example, paper being ſtain'd, may, by an acid ſpirit, be immediately turn'd red, and by any alkali, or urinous ſpirit; green; ſo that even the bruis'd bloſſoms of *Mezereon*, gather'd in froſty weather, and thoſe of peaſe cruſh'd upon white paper, how remote ſoever their colours are from green, wou'd in a moment paſs into a deep degree of that colour, upon the touch of an alkaline liquor. And either of theſe new pigments may, by a ſufficient affuſion of a contrary liquor, be preſently chang'd from red to green, and from green to red; which obſervation holds alſo in ſyrup of violets, the juice of blue-bottles, *&c.*

26. There are, however, ſome caſes wherein theſe experiments will not uniformly ſucceed. And firſt, I try'd the operation of acid ſalts upon ſuch vegetable ſubſtances as are in their own nature red; as ſyrup of clove-july-flowers, the clear expreſs'd juice of buckthorn-berries, red-roſes, infuſion of brazil, and many others; on ſome of which, ſpirit of ſalt either made no conſiderable change, or only alter'd the colour from a darker to a lighter red. And as to the operation of the other ſorts of ſalts, upon theſe red ſubſtances, I found it not very uniform; ſome red or reddiſh infuſions, as of roſes, being thereby turn'd into a dirty colour, inclining to green. Nor was the ſyrup of clove-july-flowers turn'd by the ſolution of pot-aſhes, to a much better, tho' a ſomewhat greener colour. Another ſort of red infuſion was by an alkali not turn'd into a green, but advanc'd to a crimſon; tho' there were other kinds of them, particularly the juice of buckthorn-berries, that readily paſs'd into a lovely green.

27. Among other vegetables, which ſeem'd likely to afford exceptions to the general obſervation about the different changes of colours, produced by acid and ſulphureous ſalts, we made trial upon the flowers of jeſſemin; they being both white, as to colour, and eſteem'd of a more oily nature than other flowers. Taking, therefore, only the white parts of the flowers, and rubbing them ſomewhat hard with my finger, upon a piece of clean paper, it appear'd very little diſcolour'd thereby; nor had ſpirit of ſalt, wherewith I moiſten'd one part of it, any conſiderable operation thereon: tho' ſpirit of urine, and, particularly, a ſtrong alkaline ſolution, immediately turn'd the paper, tho' it had remain'd almoſt colourleſs, of a deep greeniſh yellow; which experiment I ſeveral times repeated with the like ſucceſs. But a great degree of unctuouſneſs ſeems unneceſſary to the production of theſe effects; for when we try'd the experiment with the leaves of thoſe pure white flowers that appear about the end of winter, and are commonly call'd ſnow-drops, the event was much the ſame with the laſt mention'd.

28. Another ſort of inſtances, to ſhew how much the changes of colours effected by ſalts, depend upon the particular texture of the colour'd bodies, we have from ſeveral yellow flowers, and other vegetables, as marygold-leaves, primroſes, freſh madder, *&c.* For theſe being rubb'd upon white paper, till they imbued it with their colour, I cou'd not find,

by the addition of alkaline liquors, or of an urinous ſpirit, that they would turn either green or red; even the ſpirit of ſalt would not conſiderably alter their colour, only dilute it a little: tho', in ſome early primroſes, it deſtroy'd the greateſt part of the colour, and made the paper almoſt white again. Madder, alſo, afforded ſomething peculiar; for having gather'd ſome roots of it, whilſt its yellow juice was freſh expreſs'd upon white paper, an alkaline ſolution being dropp'd upon it, turn'd it neither green nor white, but red; and the bruis'd madder it ſelf, drench'd with the like alkaline ſolution, exchang'd its yellowneſs for a redneſs.

29. It may be of uſe, towards diſcovering the nature of the changes which the alimental juices undergo in different vegetables, in their different degrees of maturity, to obſerve what operation acid, urinous, and alkaline ſalts will have upon thoſe juices. To ſhew my meaning by an example, I took from the ſame cluſter one black-berry full ripe, and another that had not yet out-grown its redneſs; and rubbing a piece of white paper with the former, I obſerved, that the juice adhering to it, was of a dark reddiſh colour, full of little black ſpecks; and that this juice, by a drop of a ſtrong lixivium, would immediately turn into a deep greeniſh colour; by as much urinous ſpirit, into a colour near allied to the former, tho' fainter; and by a drop of ſpirit of ſalt, into a fine light red: but the red berry, rubbed in like manner upon paper, left on it a red colour, which was very little altered by the ſame acid ſpirit; and from the urinous and lixiviate ſalts, received changes of colour, different from thoſe produced in the dark juice of the ripe black-berry.

I remember alſo, that though the infuſion of damask roſes would be heighten'd by acid ſpirits, to an intenſe degree of redneſs, and, by lixiviate ſalts, be brought to a darkiſh green; yet putting a roſe, whoſe leaves were perfectly yellow, in a ſolution of ſalt of tartar, it afforded a green bluiſh tincture; but, by means of an acid liquor, I could not obtain a red one; the ſaline ſpirit I employ'd, only a little diluting the yellowneſs of the leaves. And if I were in the iſlands of *Banda*, where cloves ſo greatly proſper, I ſhould try what operation our three differing kinds of ſalts would have upon the juice of this ſpice; which eminent authors inform us is at firſt white, afterwards green, and then reddiſh, before 'tis beaten off the tree; after which, being dry'd, it grows blackiſh, as we ſee it. And one of the lateſt botanic writers informs us, that the flower grows upon the top of the clove itſelf, conſiſting of ſmall leaves, like a cherry bloſſom, but of an excellent blue.

'Tis very proper to take notice of the particular ſeaſons wherein the vegetables, deſigned for the nicer experiments, are gathered. That diligent botaniſt, Mr. *Parkinſon*, tells us, that "of buckthorn-berries are made three "ſeveral ſorts of colours; being gathered green, and kept dry, they are "call'd ſap-berries; which being ſteep'd in alum-water, give a fair yellow "colour, uſed by painters, book-binders, and leather-dreſſers, who alſo "make a green colour, called ſap-green, taken from the berries, when "they are black; that being bruiſed, and put into a braſs kettle, and

" there ſuffered to remain for three or four days, with ſome beaten alum " put to them, they are afterwards preſſed, and the liquor uſually put into " bladders, and hung up till it be dry: this, he ſays, is afterwards diſſol- " ved in water, or wine, but canary is the beſt to preſerve the colour " from ſtarving. The third is a purpliſh colour, made of the berries, " ſuffered to grow upon the buſhes, till the middle, or end of *November*, " when they are ready to fall of themſelves." And I try'd, with ſucceſs, to make ſuch a kind of pigment as painters call ſap green, by a way not unlike that here delivered by our author. Much after the ſame manner, alſo, they make ſap-green in the colour-ſhops.

Changes of colour by digeſtion, &c. particularly a redneſs.

30. Many bodies, digeſted in well-cloſed veſſels, change their colour in tract of time, as rectify'd ſpirit of hart's-horn; and the ſame is evident in the precipitations of amalgams of gold and mercury, without addition; where, by the continuance of a due heat, the ſilver-coloured amalgam is reduced into a ſhining red powder. And many other inſtances of the like kind, might be produced. Now, in theſe operations, there appears no reaſon why we ſhould attribute the new colours to the action of a new ſubſtantial form; nor to any increaſe, or decreaſe of the ſalt, ſulphur, or mercury of the matter that acquires them. For the veſſels are cloſed, and theſe principles, according to the chymiſts, are ingenerable, and incorruptible. So that the effect ſeems to proceed from the heat, agitating and diſcompoſing the corpuſcles of the body expoſed to it; which, in proceſs of time, ſo changes its texture, that the tranſpoſed parts modify the incident light, otherwiſe than when the matter appeared of another colour.

31. Among the ſeveral changes of colour which bodies acquire, or diſcloſe, by digeſtion, it is very remarkable, that chymiſts find a redneſs, rather than any other colour, in moſt of the tinctures they draw; and even in the more groſs ſolutions they make, of almoſt all concretes that abound either with mineral or vegetable ſulphur; tho' the menſtruum employ'd about theſe ſolutions, or tinctures, be never ſo limpid. This we have obſerved in abundance of tinctures, drawn with ſpirit of wine from jalap, guaiacum, and many other vegetables; and not only in the ſolutions of amber, benjamin, *&c.* made with the ſame menſtruum; but alſo in ſeveral mineral tinctures. And, not to urge that familiar inſtance of the ruby of ſulphur, as chymiſts call the ſolution of flowers of brimſtone, made with the ſpirit of turpentine; nor to take notice of other more known examples of the aptneſs of chymical oils to produce a red colour with the ſulphur they extract or diſſolve; 'tis remarkable, that both acid and alkaline ſalts, tho', in moſt other caſes, of contrary operations, will, with many bodies that abound in ſulphureous, or oily parts, produce a red; as is manifeſt in the vulgar inſtances of the tinctures, or ſolutions of ſulphur, made with lixiviums, either of calcined tartar, or pot-aſhes, and other obvious examples; and in that the true glaſs of antimony, extracted with ſome acid ſpirits, yields a red tincture. And a certain acid liquor will, in a moment, turn oil of turpentine into a deep

PHYSICS. red*. Among the many inſtances I could produce of the eaſy production of redneſs, by the operation of a ſaline ſpirit, as well as of ſpirit of wine, two or three of them deſerve to be particularly mentioned.

32. But, before I ſet them down, 'tis proper to premiſe, that there ſeems to be a manifeſt diſparity betwixt red liquors; for ſome of them may be ſaid to have a genuine redneſs, in compariſon of others that have it yellowiſh. Thus if a good tincture of cochineal be diluted never ſo much with fair water, it will not become a yellow liquor. But balſam of ſulphur, tho' in a large quantity, it appear to be of a deep red; yet if you ſhake the containing glaſs, or pour a few drops on a ſheet of white paper, ſpreading them on it with your finger, what falls back along the ſides of the glaſs, or ſtains the paper, will appear yellow. And there are many tinctures, ſuch as that of amber, made with ſpirit of wine, which will appear either yellow or red, according as the veſſels that contain them are ſlender or broad.

33. To come now to the experiments we deſigned. Firſt, oil or ſpirit of turpentine, tho' clear as fair water, being digeſted upon the pure white ſugar of lead, has, in a ſhort time, afforded us a high red tincture; probably a good medicine.

34. Secondly, take common brimſtone, and ſal-armoniac, of each, finely powdered, five ounces; of beaten quick-lime, ſix ounces; mix theſe powders exquiſitely, and diſtil them in a retort, placed in ſand, by degrees of fire; giving, at length, as intenſe an heat as you can in ſand; and there will come over a volatile tincture of ſulphur, which alſo may prove an excellent medicine. Now, tho' none of the ingredients here be red, the diſtilled liquor is of that colour; and, if it be well drawn, will, upon a little agitation of the vial, firſt unſtopped, ſend out a great white fume, which ſometimes ſpreads wide, and is very offenſive: and tho' the liquor itſelf be red, and its fumes white; yet it will dye the fingers black.

35. The laſt experiment, I ſhall now produce, to ſhew how apt bodies, abounding in ſulphureous parts, are to afford a red colour, is one wherein the operation of a clear ſaline ſpirit upon a white, or whitiſh body, may produce a redneſs in the twinkling of an eye. We took then, a little eſſential oil of aniſeeds congealed, and ſpread it with a knife upon a piece of white paper; when mixing with it a drop or two of well rectified oil of vitriol, there immediately emerged, with ſome heat and ſmoke, a blood-red colour.

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* Mr. *Coles* found, that a certain ſulphureous ſpirit, mixed with a volatile alkali, gives a red colour in a moment. This ſpirit he made, by diſtilling two or three pound of benjamin with a little ſand, in a retort, *ad ſiccitatem*; and putting the oil, ſpirit, and flowers together in a filtre of paper, when the ſpirit came firſt thorough. Put two parts of this ſpirit to one of ſpirit of ſal-armoniac, and ſhake the containing glaſs, and the liquor will inſtantly turn red; tho' they both were clear before. The more the veſſel is ſhook, the deeper will be the red. And this effect is produced without any efferveſcence; whence the inventor conceives the experiment may be of uſe in accounting for ſanguification. *Philoſ. Tranſ.* N° 228. p. 542.

36. Let

The degeneration of colours.

36. Let it be here obſerved, once for all, that, in many of theſe experiments the colour produced is often very ſubject to degenerate. However, ſince the changes we have ſet down happen preſently upon the operation of the bodies on each other, or at the times ſpecified; that is ſufficient to ſhew what we intend. For it is not eſſential to the genuineneſs of a colour to be durable; a fading leaf, that is ready to rot and moulder into duſt, may have as true a yellow as a wedge of gold. And I have ſeveral times obſerved, that the mixture made by the oils of vitriol, and of aniſeeds, tho' it acquire a thicker conſiſtence than either of the ingredients, quickly loſes its colour, and turns to a dark grey; at leaſt in the ſuperficial parts, where 'tis expoſed to the air. This degeneration of colours may, in many caſes, indeed, proceed from the further action of the ſaline corpuſcles, and other ingredients upon one another; yet much of the ſudden change may often be aſcribed to the air. Thus we have ſometimes obſerved window-curtains of a light colour, to have that part of them which was expoſed to the air, when the window ſtood open, of one hue; and the lower part, that was ſcreen'd from the air by the wall, of another colour. And *Parkinſon* ſays, of the plant turnſol, that "its berries, when "full ripe, have within them, between the outward skin and the inward "kernel, a certain juice, which rubbed upon paper or cloth, at the firſt "appears of a freſh lovely green, but preſently changes into a kind of "bluiſh purple. He adds, that the ſame cloth, afterwards wet in water, "and wrung out, will turn the water into a claret colour. And theſe "rags of cloth, ſays he, are thoſe uſually call'd turnſol in the druggiſts "ſhops." I alſo remember, that letting ſome of the deep red juice of buckthorn berries drop upon a piece of white paper, and leaving it there for many hours, till the paper was grown dry again; I found the juice degenerated to a dirty kind of greyiſh colour; which, in a great part of the ſtain'd paper, ſeem'd not to have ſo much as an eye of red: tho' a little ſpirit of ſalt, or diſſolved alkali, would turn this unpleaſant colour into a red or green. And, to ſatisfy myſelf that this degeneration of colour did not proceed from the paper, I dropp'd ſome of the ſame juice upon a white glazed tile; and, permitting it to dry thereon, I found it then alſo loſt its colour.

Having diſſolved good ſilver in *Aqua fortis*, and precipitated it with ſpirit of ſalt, upon firſt decanting the liquor, the remaining matter was purely white; but, after it had lain a while uncovered, the part of it that lay contiguous to the air, not only loſt its whiteneſs, but appeared of a very dark, and almoſt blackiſh colour; but if the part that was contiguous to the air, were gently taken off, the ſubjacent part of the ſame maſs would appear very white; till that alſo, having continued a while expoſed to the air, would likewiſe degenerate. Whether the air produce this effect by the means of a ſubtile ſalt, by a penetrating moiſture, by ſolliciting the avolation of certain parts of the bodies to which 'tis contiguous, or by ſome other way, I leave to be further conſidered. 'Twere eaſy here to add many other inſtances of redneſs, reſulting from the digeſtion of bodies.

I have often ſeen, upon the borders of *France*, a ſort of pear, which, digeſted with a little wine in a veſſel exactly cloſed, will ſoon appear throughout of a deep red colour. Nay, even pure white ſalt of tartar, and limpid ſpirit of wine, will, by long digeſtion, acquire a redneſs. Some other obvious changes of colours frequently happen; ſuch as is the blackneſs of bodies burned in the open air, *&c.* But I ſhall not at preſent examine into the cauſes of theſe changes; tho' certainly the reaſon why the ſoots of different bodies are almoſt all of them black; why ſo much the greater part of vegetables ſhould be rather green than of any other colour; and, particularly, why gentle heat, ſo frequently, in chymical operations, produces rather a redneſs, than another colour in digeſted menſtrua, may very well deſerve a ſerious inquiry.

Different effects of an acid, in the production of colours, reconciled.

37. It may ſeem ſtrange, that if the crimſon ſolution of cochineal, the juice of black-cherries, or of ſome other vegetables that afford the like colour, be let fall upon a piece of paper; a drop or two of an acid ſpirit, ſuch as ſpirit of ſalt, or *Aqua fortis*, will immediately turn it into a fair red; whilſt an infuſion of brazil, in fair water, will have its redneſs deſtroy'd by a little ſpirit of ſalt, or *Aqua fortis*; and be turn'd either yellow or pale. But if we conſider the caſe attentively, the action of the acid ſpirit ſeems, in both theſe experiments, only to weaken the colour of the liquor wherein it falls; and ſo tho' it deſtroys redneſs in the tincture of brazil, but produces it in the tincture of cochineal, its operations may be ſtill uniform: ſince as crimſon is little elſe than a very deep red, with perhaps an eye of blue; ſo ſome kind of reds ſeem to be little elſe than heightned yellow; and conſequently in ſuch bodies the yellow ſeems to be but a diluted red. And accordingly, alkaline ſolutions, and urinous ſpirits, which ſeem diſpos'd to deepen the colours of moſt vegetable liquors, will not only reſtore the ſolution of cochineal, and the infuſion of brazil to the crimſon, from which the ſpirit of ſalt had changed them into a truer red; but will alſo heighten the yellow juice of madder into red, and advance the red infuſion of brazil to a crimſon. But perhaps it will be much ſafer to derive theſe changes from the vary'd texture, than from the peculiar kinds of bodies.

The colours of the fumes of bodies, and of the ſubſtances they form, obſerved in diſtillations, &c.

38. It might greatly contribute to the hiſtory of colours, if chymiſts would give us a faithful account of thoſe to be obſerved in the ſteams of bodies, ſublimed, or diſtilled, and of the productions made by the coalition of thoſe ſteams. Thus, for inſtance, we find in diſtilling pure ſalt-peter, that at a certain juncture of the operation, the body, tho' it ſeem either cryſtalline, or white, affords very red fumes; whilſt tho' vitriol be either green or blue, its ſpirit comes over in whitiſh vapours. The like colour I have found in the fumes of ſeveral other concretes, of different colours and natures; eſpecially when diſtill'd with ſtrong fires. And even ſoot, as black as it is, has fill'd our receivers with white fumes. No leſs obſervable may the diſtill'd liquors be, into which ſuch fumes convene: for tho', by ſkill and care, a reddiſh liquor is attainable from nitre, yet the common ſpirit of it, in the preparation whereof abundance of theſe red fumes paſs

over into the receiver, has no appearance of red. Neither the ſpirit of vitriol, nor of ſoot, is white; and the empyreumatical oils of woods, and other concretes, are either of a deep red, or of a colour between red and black. But 'tis very remarkable, that notwithſtanding the great variety of colours, to be met with in herbs, flowers, and other bodies uſually diſtill'd *in Balneo*; yet all the waters and ſpirits that firſt come over by that way of diſtillation, leave the colours of the concretes behind them; tho', indeed, there are one or two vegetables, not commonly taken notice of, whoſe diſtill'd liquors carry over the tincture of the concrete with them. And as in diſtillation, ſo in ſublimation, it were worth while to take notice of what happens to our purpoſe, by performing that operation in conveniently ſhap'd glaſſes, wherein the colour of the aſcending fumes may be diſcern'd; for it might afford a naturaliſt good information, to obſerve the agreements, or differences betwixt the colours of the aſcending fumes, and thoſe of the flowers they compoſe by their convention. It is evident, that theſe flowers do, many of them, greatly differ in point of colour; not only from one another, but often from the concretes that afford them. Thus, tho' camphire and brimſtone afford flowers much of their reſpective colours, except that thoſe of brimſtone are a little paler than the lumps that yielded them; yet the flowers of red benjamin are either white, or whitiſh. And, to omit other inſtances, even that black mineral, antimony, may be made to afford flowers, ſome of them red, others grey, and which is more ſtrange, ſome of them purely white. And glaſs-men, by exquiſitely mixing a convenient proportion of brimſtone, ſal-armoniac, and quick-ſilver, and ſubliming them together, make a ſublimate of an excellent blue. And though, upon making this experiment, we found the ſublimate to be far from a lovely colour, yet, in ſome parts, it ſeem'd bluiſh, and was, at leaſt, of a colour very different from either of the ingredients; which is ſufficient for our preſent purpoſe. But a much finer colour is promiſed by ſome empirics, who tell us, that orpiment ſublim'd, will afford, among the parts of it that fly upwards, ſome little maſſes, which, though the mineral itſelf be of a good yellow, will be red enough to emulate rubies, both in colour and tranſparency. This experiment may, for ought I know, ſometimes ſucceed; for I remember, that having in a ſmall bolt-head, purpoſely ſublim'd ſome powder'd orpiment, we could in the lower part of the ſublimate, diſcern, here and there, reddiſh lines; tho' much of the upper part of it conſiſted of a matter not only purely yellow, but tranſparent. And we have alſo, by this means, obtain'd a ſublimate, which, though it conſiſted not of rubies, yet ſmall pieces of its lower part, that were numerous, afforded a pleaſant reddiſh ſparkling colour.

Various changes of colour, cauſed by ſaline ſpirits in the tinctures of vegetables.

39. Take the dry'd buds, or bloſſoms, of the pomegranate-tree, commonly called balauſtins, pull off the reddiſh leaves, and, by a gentle ebullition, or a competent infuſion of them in fair water, extract a faint reddiſh tincture, which, if the liquor be turbid, you may clarify by the filtre; into this, if you pour a little ſpirit of urine, or ſome other

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On the other hand, we took ſome dryed red roſe-leaves, and ſhaking them in a glaſs of fair water, they communicated no redneſs to it; but upon the affuſion of a little oil of vitriol, the water was immediately turn'd red; which it would not have been, if inſtead of oil of vitriol, we had employ'd oil of tartar. Our author tells us, there was no redneſs either in the water, the leaves of ſena, or the oil of tartar; yet we have found that by ſteeping ſena for a night in cold water, it would afford a very deep yellow, or reddiſh tincture, without the help of oil of tartar; which ſeems to do little more than aſſiſt the water more ſuddenly to extract a plenty of that red tincture wherewith the leaves of ſena abound: for having made a tincture of ſena, only with fair water, and decanted it from the leaves before it grew reddiſh, we could not perceive, by dropping ſome oil of tartar into it, that the colour was conſiderable, tho' it were thereby a little heighten'd into a redneſs. And the ſame thing may be alledg'd in the experiment with red roſe-leaves; for we found that ſuch leaves, by bare infuſion, for a night and a day, in fair water, afforded a tincture bordering upon redneſs; and that colour being conſpicuous in the leaves themſelves, ſeem'd not wholly produc'd, but extracted by the oil of vitriol. But to improve the experiment, take the tincture of red roſe-leaves, made with a little oil of vitriol, and a large quantity of fair water, pour off this liquor into a clear vial, half fill'd with limpid water, till the water held againſt the light hath acquired a competent redneſs, without loſing its tranſparency: into this tincture, drop, leiſurely a little ſpirit of urine; and ſhaking the vial, which you muſt ſtill hold againſt the light, you will ſee the red liquor immediately turn'd into a fine greeniſh blue; a colour not to be found in any of the bodies upon whoſe mixture it enſued. And this change is the more remarkable, becauſe tho' the degeneration of blue into red, be uſual, the turning of red into blue, is very unfrequent. If, upon the falling of each drop of ſpirit of urine, you ſhake the vial containing the red tincture, you may obſerve a pretty variety of colours in the paſſage of that tincture from a red to a blue. And ſometimes we have thus obtain'd ſuch a liquor, as being view'd againſt, and from the light, ſeem'd faintly to emulate the tincture of *Lignum Nephriticum*. And if you make the tincture of red-roſes very high, and, without diluting it with fair water, pour on the ſpirit of urine, you may have a blue ſo deep as to render the liquor opake; but if it be dropp'd upon white paper, the colour will ſoon diſcloſe itſelf. Having, alſo, made the red, and conſequently the blue tincture very tranſparent, and ſuffer'd it to reſt in a ſmall open vial for a day or two; we found that not only the blue, but the red colour, likewiſe, was vaniſh'd; the clear liquor remaining of a bright amber colour, at the bottom whereof ſubſided a large, light feculency, almoſt of the ſame colour; which ſeems to be nothing but the ting'd parts of the roſe-leaves, drawn out by the acid ſpirits of the oil of vitriol, and precipitated by the volatile ſalt of the ſpirit of urine. This makes it the more probable, that the redneſs drawn by the oil of vitriol, was as well an extraction of the tinging parts of the roſes, as a production of redneſs. And laſtly, the colour of the tincture of roſes, may be chang'd by many other ſulphureous ſalts; as a ſtrong ſolution of pot-aſhes, oil of tartar, *&c.* tho' theſe are ſeldom ſo free from feculency, as the ſpirit of urine becomes by diſtillation. And I have a way of producing a full purple, by employing a liquor not made red artificially, inſtead of the tincture of red-roſes; for having by infuſing the powder of log-wood for a while in fair water, made that liquor red, I dropp'd into it a little urinous ſpirit, as that of ſal-armoniac, by which the colour was inſtantly turn'd into a rich and lovely purple. But care muſt be taken not to let fall into a ſpoonful of the tincture, above two or three drops of the ſpirit, leſt the colour become ſo deep as to make the liquor opake. And if inſtead of fair water, I infus'd the log-wood in water, made a little ſour by the acid ſpirit of ſalt; I obtain'd neither a purple, nor a red, but only a yellow liquor.

40. The following experiment is very ſurprizing; and, of all I have yet met with, ſeems the fitteſt to enforce our doctrine of colours, and to ſhew them not to be inherent qualities, flowing from the ſubſtantial forms of the bodies whereto they are ſaid to belong; ſince by a bare mechanical change of texture, in the minute parts of bodies, colours may, in a moment, be generated quite *de novo*, and utterly deſtroy'd: for there is this difference betwixt the enſuing experiment, and moſt of the others we deliver, that in this, the colour of a body is not chang'd into another, but betwixt two bodies, each of them, a-part, colourleſs, there is, in a moment, generated a very deep colour, which, if it were let alone, would be permanent; and yet, by a very ſmall parcel of a third body, that has no colour of its own, this otherwiſe permanent colour, will inſtantly be ſo utterly deſtroy'd, as to leave not the leaſt ſign of any colour in the whole mixture. The experiment is very eaſy, and thus perform'd. Take good common ſublimate, and fully ſatiate with it what quantity of water you pleaſe, carefully filtre the ſolution thro' clean cloſe paper, that it may drop down clear and colourleſs as fountain water. Then, when you would ſhow the experiment, put about a ſpoonful of it into a ſmall wine-glaſs, or other convenient veſſel, and letting fall therein three or four drops of good oil of tartar *per deliquium*, well filtred, that it may likewiſe be without colour, theſe two limpid liquors will, in the twinkling of an eye, turn into an opake

A colour inſtantly generated, and perfectly deſtroy'd.

mixture of a deep orange-colour; which, by continually ſhaking the glaſs in your hand, you muſt prevent from ſettling too ſoon at the bottom. And when the ſpectators have view'd this firſt change, preſently drop in four or five drops of oil of vitriol, and continuing to ſhake the glaſs pretty ſtrongly, that it may the ſooner diffuſe itſelf, the whole colour will now immediately diſappear, and all the liquor in the glaſs become clear and colourleſs, as before, without the leaſt ſediment at the bottom. But for the more neat performance of this experiment, obſerve, Firſt, not to take too much of the ſolution of ſublimate, nor drop in too much of the oil of tartar, to avoid the neceſſity of putting in ſo much oil of vitriol, as to cauſe an ebullition.

Secondly, 'tis convenient to keep the glaſs conſtantly in motion, both for the better mixing of the liquors, and hindering, as we ſaid, the yellow ſubſtance from ſubſiding, which it would otherwiſe ſoon do; tho' when 'tis ſubſided, it will retain its colour, and be deprived of it by the oil of vitriol.

Thirdly, if any yellow matter ſtick to the ſides of the glaſs, 'tis but inclining the veſſel, till the clarify'd liquor can waſh along it, and the liquor will preſently imbibe it, and deprive it of its colour.

The chymical reaſon of this phenomenon.

My notion of the different natures of the ſeveral tribes of ſalts, having led me to deviſe this experiment; I can eaſily aſſign the chymical reaſon of the phenomenon. Having then obſerved, that mercury, diſſolved in ſome menſtrua, would yield a dark yellow precipitate; and ſuppoſing that common water, and the ſalts that ſtick to the mercury, would, in this reſpect, be equivalent to thoſe acid menſtrua which work upon the quickſilver, on account of their ſaline particles; I ſubſtituted a ſolution of ſublimate in fair water, inſtead of a ſolution of mercury in *Aqua fortis*, or ſpirit of nitre: then, conſidering that what makes the yellow colour, is indeed but a precipitate, obtain'd by means of the oil of tartar, which generally precipitates metalline bodies corroded by acid ſalts; ſo that the colour in our caſe reſults from the coalition of the mercurial particles with the ſaline ones, wherewith they were formerly aſſociated; and with the alkaline particles of the ſalt of tartar, that float in the oil. Conſidering alſo, that very many effects of lixivious liquors upon the ſolutions of other bodies may be deſtroyed by acid menſtrua, I concluded, that if I choſe a very ſtrong acid liquor, which by its inciſive power might undo the work of the oil of tartar, and diſperſe again thoſe particles which the other had, by precipitation, aſſociated into ſuch minute corpuſcles as were, before, ſingly inviſible, they would diſappear again, and conſequently leave the liquor as colourleſs as before the precipitation was made. This ſeems to be the chymical reaſon of the experiment, and is that which induced me to make it. But to give a perfect mechanical ſolution of the whole phenomenon, is more than I pretend to. However, the yellowneſs of the mercurial ſolution, and oil of tartar, ſeems produced by the precipitation occaſioned by the affuſion of the latter; as the deſtruction of the colour proceeds from the diſſipation of that curdled matter, whoſe texture is deſtroy'd, and the matter itſelf diſſolved into minute and inviſible particles, by the ſtrong acid menſtruum: which is the reaſon why there remains no ſediment at the bottom; the infuſed oil taking it up, and reſolving it into inviſible parts, as water diſſolves ſalt or ſugar.

But, to confirm my conjecture as to the chymical reaſon of our experiment, I might add, that I cannot only make the mercury precipitate out of the firſt ſimple ſolution quite of a different colour from that mentioned; but if, inſtead of altering the precipitating liquor, I altered the texture of the ſublimate in ſuch a manner, as my notion about ſalts requires, I can produce the ſame phenomenon. For having ſublimed together equal parts of ſal-armoniac, and ſublimate, firſt diligently mixed, the aſcending flowers diſſolved in fair water, and filtred, gave a ſolution limpid and colourleſs, like that of the other ſublimate; and yet an alkali dropped into this liquor, did not turn it yellow, but white. And, upon the ſame foundation, we may with quickſilver, unaſſiſted by common ſublimate, prepare another ſort of flowers diſſoluble in water, without diſcolouring it; which will alſo change its colour in the ſame manner. And ſo much does the colour depend upon the texture reſulting from the convention of the ſeveral ſorts of particles, that tho', in our experiment, oil of vitriol deſtroys the yellow colour; yet with quickſilver, and fair water, by the help of oil of vitriol alone, we may eaſily make a kind of precipitate, of a fair and permanent yellow; as we ſhall ſee hereafter. I here make choice of oil of vitriol, becauſe, when well rectified, 'tis not only colourleſs and ſcentleſs, but extremely ſtrong and inciſive. And common *Aqua fortis* will not perform the ſame thing ſo well.

This experiment may be ſeveral times repeated with the ſame parcels of the liquors; for I found, that after I had clarify'd the orange-coloured mixture, by the addition of as little of the oil of vitriol as would ſuffice to produce the effect, I could, at pleaſure, re-produce the opake colour, by dropping in freſh oil of tartar; and deſtroy it again, by the re-affuſion of more of the acid menſtruum: tho', by the addition of ſo much new liquor, in proportion to the mercurial particles, the colour will, at length, appear weak and faint.

The preceding experiment varied.

By boiling crude antimony in a ſtrong and clear lixivium, you may ſeparate a ſubſtance from it, call'd by ſome chymiſts its ſulphur; which, when let fall by the liquor, upon its refrigeration, often ſettles in flakes of a yellow ſubſtance. Conſidering therefore, that common ſulphur, boiled in a lixivium, may be precipitated out of it by *Rheniſh*, or white-wine, which are ſouriſh liquors, and contain an acid ſalt; and having found, by tryal, that with other acid liquors I could precipitate out of lixivious ſolvents, ſome other mineral concretions, abounding with ſulphureous parts, of which ſort is crude antimony; I concluded it eaſy to precipitate the antimony, thus diſſolved, with the acid oil of vitriol. And tho' common ſulphur yields a white precipitate, called *Lac Sulphuris*; yet I ſuppoſed the precipitated antimony would be of a deep yellow colour; as well, if made with oil of vitriol, as by refrigeration, and length of time. From hence 'twas eaſy to deduce this experiment, that if we put into one glaſs ſome of this freſh filtred ſolution of antimony; and into another, ſome of the orange-coloured mixture, made with a mercurial ſolution, and oil of tartar; a few drops of oil of vitriol, let fall into the latter glaſs, would turn the deep yellow mixture into a clear liquor; whilſt a little of the ſame oil dropped into the other glaſs, preſently turn'd the moderately clear ſolution into a deep yellow ſubſtance. But this ſucceeds not well, unleſs you employ a lixivium that has lately diſſolved the antimony, and before it ſuffers it to fall: yet, in ſummer-time, if the lixivium have been duly impregnated, and well filtred after it is quite cold, it will for ſome days retain antimony enough to exhibit, upon the affuſion of the corroſive oil, as much of a good yellow ſubſtance as is neceſſary to ſatisfy the ſpectators of the poſſibility of the experiment.

To find what kind of ſalt, whether acid, volatile or fix'd, predominates in an aſſign'd liquor, or ſaline body.

The diſtinction of ſalts into acid, volatile, and fixed or alkaline, may poſſibly be ſo uſeful in natural philoſophy, * as to render it an acceptable corollary of the preceding experiment, from thence to deduce a method of trying which, or whether any one of thoſe ſalts is predominant in chymical liquors. We have already ſhewn a way, by means of the tincture of *Lignum Nephriticum*, or of ſyrup of violets, to diſcover whether a ſalt propoſed be acid or not; yet we can thereby only find in general, that particular ſalts belong not to the tribe of acids: but cannot determine whether they belong to the tribe of the urinous; (under which I comprehend all thoſe volatile ſalts of animal, or other ſubſtances, that are contrary to acids) or to that of alkalies: for both theſe ſalino-ſulphureous ſalts will reſtore the blue colour to that tincture, and turn the ſyrup of violets green. The preſent experiment, therefore, commodiouſly ſupplies this deficiency. I found, that all thoſe chymical ſalts I thought fit to make tryal of, would, if they were of a lixivious nature, make, with ſublimate diſſolved in fair water, an orange-tawny precipitate;

* About the nature of ſalts, Sir *Iſaac Newton* thus argues. "When mercury "ſublimate is re-ſublimed with freſh mercury, and becomes *Mercurius dulcis*, "which is a white taſtleſs earth, ſcarce diſſolvable in water; and *Mercurius dulcis* "re-ſublimed with ſpirit of ſalt, returns "into mercury ſublimate; and when metals corroded with a little acid, turn into "ruſt, which is an earth taſtleſs, and indiſſolvable in water; and this earth, imbibed with more acid, becomes a metallic "ſalt; and when ſome ſtones, as ſpar of "lead, diſſolved in proper menſtrua, become ſalts; do not theſe things ſhew, "that ſalts are dry earth and watery acid "united by attraction; and that the earth "will not become a ſalt without ſo much "acid as makes it diſſolvable in water? Do "not the ſharp and pungent taſtes of acids "ariſe from the ſtrong attraction, whereby "the acid particles ruſh upon, and agitate "the particles of the tongue? And when "metals are diſſolved in acid menſtrua, and "the acids in conjunction with the metal, "act after a different manner, ſo that the "compound has a different taſt much milder "than before, and ſometimes a ſweet one; "is it not becauſe the acids adhere to the "metallic particles, and thereby loſe much "of their activity? And if the acid be in "too ſmall a proportion to make the compound diſſolvable in water; will it "not, by adhering ſtrongly to the metal, "become unactive, and loſe its taſt, and "the compound be a taſtleſs earth? "For ſuch things as are not diſſolvable "by the moiſture of the tongue, act not "upon the taſt. As gravity makes the "ſea flow round the denſer and weigh-"tier

tate; but if they were of an urinous nature, the precipitate would be white and milky. So that having always at hand ſome ſyrup of violets, and a ſolution of ſublimate; I can, by the help of the former, immediately diſcover whether a propoſed ſalt, or ſaline body, be of an acid nature; if it be, I need inquire no further; but if not, I can very eaſily and readily diſtinguiſh between the other two kinds of ſalts, by the white, or orange-colour, immediately produced by letting fall a few drops or grains of the ſalt to be examined, into a ſpoonful of the clear ſolution of ſublimate. Thus, for example, it has been ſuppoſed, that when ſal-armoniac, mixed with an alkali, is forced from it by the fire in cloſe veſſels, the volatile ſalt thence obtained, is but a more fine and ſubtile ſort of ſal-armoniac; which, 'tis preſumed, this operation only purifies more exquiſitely than common ſolutions, filtrations, and coagulations. But this opinion may eaſily be proved erroneous, as by other arguments, ſo particularly by our method of diſtinguiſhing the tribes of ſalts. For the ſaline ſpirit of ſal-armoniac, as it is in many other manifeſt qualities very like the ſpirit of urine; ſo, like that, it will inſtantly turn ſyrup of violets to a lovely green; a ſolution of good verdigreaſe into an excellent azure; and make the ſolution of ſublimate depoſite a white precipitate. Therefore, in moſt experiments, where I only deſign to produce a ſudden change of colour; I ſcruple not to uſe ſpirit of ſal-armoniac, when at hand, inſtead of ſpirit of urine; as indeed it ſeems chiefly to conſiſt of the volatile urinous ſalt. And by this way of examining chymical liquors, we may not only, in general, conclude affirmatively, but, in ſome caſes, negatively.

Thus, ſince ſpirit of wine, and, as far as I have try'd, the chymical oils will not turn ſyrup of violets red, or green; nor the ſolution of ſublimate,

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tier parts of the globe of the earth; ſo the attraction may make the watry acid flow round the compacter particles of earth, for compoſing the particles of ſalt. For otherwiſe the acid would not do the office of a medium between the earth and common water, for making ſalts diſſolvable in the water; nor would ſalt of tartar readily draw off the acid from diſſolved metals, nor metals the acid from mercury. Now, as in the great globe of the earth and ſea, the denſeſt bodies by their gravity ſink down in water, and always endeavour to go towards the centre of the globe; ſo in particles of ſalt, the denſeſt matter may always endeavour to approach the centre of the particle: ſo that a particle of ſalt may be compared to a chaos, being denſe, hard, dry and earthy in the centre; and rare, ſoft, moiſt, and watry in the circumference. And hence it ſeems to be, that ſalts are of a laſting nature, being ſcarce deſtroyed, unleſs by drawing away their watry parts by violence, or by letting them ſoak into the pores of the central earth by a gentle heat in putrefaction, till the earth be diſſolved by the water, and ſeparated into ſmaller particles; which, by reaſon of their ſmallneſs, make the rotten compound appear of a black colour. Hence alſo it may be, that the parts of animals and vegetables preſerve their ſeveral forms, and aſſimilate their nouriſhment; the ſoft and moiſt nouriſhment eaſily changing its texture, till it becomes like the denſe, hard, dry, durable earth in the centre of each particle. But when the nouriſhment grows too moiſt to be aſſimilated, or the central earth grows too feeble to aſſimilate it, the motion ends in confuſion, putrefaction, and death." *Newton. Optic.* p. 360—362.

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white or yellow, I infer, with probability, that either they are deſtitute of ſalt, or have ſuch as belongs not to either of the three grand families mention'd.

And thus, upon examining the ſpirit of oak, or of ſuch like ſubſtances, forc'd over the retort, I found by this means, amongſt others, that thoſe chymiſts are much miſtaken, who make it a ſimple liquor, and one of their hypoſtatical principles; for, not to mention what phlegm it may have, a few drops of one of this ſort of ſpirits, mix'd with a large proportion of ſyrup of violets, turn'd it purpliſh; by the affinity of which colour to redneſs, I conjectur'd that the ſpirit contain'd ſome acid corpuſcles, and accordingly found, that it would deſtroy the blueneſs of the nephritic tincture; and that being put upon corals, it would corrode them like common ſpirit of vinegar, and other acid liquors. And to examine whether there were not a great part of the liquor of other than an acid nature; having ſeparated the ſour part from the reſt, we concluded, the remaining part, though it had a ſtrong taſte, as well as ſmell, to be of a nature different from that of any of our three ſorts of ſalts; for it did as little as ſpirit of wine, and chymical oils, alter the colour of ſyrup of violets, and a ſolution of ſublimate: whence we alſo inferr'd, that the change made of that ſyrup into a purple colour, was effected by the vinegar, that proved one of the ingredients of the liquor which uſually paſſes for a ſimple, or uncompounded ſpirit.

And by the ſame way we may examine and diſcover many changes that are produced in bodies, either by nature or art; for both are able, by changing the texture of ſome ſubſtances, to qualify them to operate after a new manner, upon the ſyrup, or ſolution. Thus I have diſcover'd, that there are factitious bodies, which, tho' they run as readily as ſalt of tartar, belong, in other reſpects, not to the family of alkalies, much leſs to that of volatile or acid ſalts. Perhaps, too, I know a way of making a highly operative ſaline body, that ſhall neither change the colour of ſyrup of violets, nor precipitate the ſolution of ſublimate. And I can, likewiſe, conceal the liquors wherewith I make ſuch changes of colours as thoſe lately mention'd, by quite altering the texture of ſome ordinary chymical productions.

We may here obſerve, that the reaſon why our method of examining ſalts, ſucceeds in the ſolution of ſublimate, depends upon the particular texture of that ſolution, as well as upon the different natures of the ſaline liquors employ'd to precipitate it. For gold diſſolv'd in *Aqua regia*, whether it be precipitated with oil of tartar, which is an alkali, or ſpirit of ſal-armoniac, which is a volatile ſalt, will either way afford a yellow ſubſtance; tho' oil of vitriol, itſelf, would not precipitate the metal out of the ſolution, or deſtroy the colour of it; notwithſtanding that oil will readily precipitate ſilver diſſolv'd in *Aqua fortis*. And if you diſſolve pure ſilver in *Aqua fortis*, and ſuffer it to ſhoot into cryſtals, the clear ſolution of theſe made in fair water, will afford a very white precipitate, whether occaſion'd by an alkali, or an acid ſpirit; tho' with ſpirit of ſal-armoniac, made with quick-lime, I cou'd obtain nothing like it; this volatile ſpirit, as alſo that of urine, ſcarce doing any more than ſtriking down a very ſmall quantity of matter, which was not ſo much as whitiſh; ſo that the remaining liquor, being ſuffer'd to evaporate its ſuperfluous moiſture, the greateſt part of the metalline corpuſcles, with the ſaline ones that had imbibed them, coagulated together into ſalt; as is uſual in ſuch ſolutions, when the metal has not been precipitated.

41. From ſpirit of urine, made after fermentation, and a due proportion of copper-filings, having obtain'd a very lovely azure ſolution; I pour'd into a clean glaſs, about a ſpoonful of this liquor, and found I could, by ſhaking into it ſome drops of ſtrong oil of vitriol, immediately deprive it of its deep colour, and make it look like common water. And if into a ſmall ſpoonful of a ſolution of good *French* verdigreaſe, made in fair water, I dropp'd and ſhook ſome ſtrong ſpirit of ſalt, or dephlegm'd *Aqua fortis*, the greenneſs of the ſolution would, in a trice almoſt, totally diſappear, and the liquor, held againſt the light, ſcarce appear other than clear, or limpid; which is therefore remarkable, becauſe we know, that *Aqua fortis*, by corroding copper, the thing which gives the colour to verdigreaſe, uſually reduces it to a greeniſh blue ſolution. But if into this almoſt colourleſs liquor, you drop a juſt quantity either of oil of tartar, or ſpirit of urine, you may find, that after the ebullition is ceas'd, the mixture will diſcloſe a lively colour, tho' ſomewhat different from that the ſolution of verdigreaſe had originally.

One body changed into more, of different colours, by a colourleſs ingredient.

42. But theſe experiments tending either to alter the colour of a body, or entirely to deſtroy it, without giving it a ſucceſſor; I had a deſire alſo, to turn a body of one colour into two, very different both in conſiſtence and colour, by help of one that was colourleſs. In order to this, finding the acidity of ſpirit of vinegar to be wholly deſtroy'd, by working upon *Minium*, I concluded, that the ſolution of lead, in ſpirit of vinegar, would alter the colour of the juices and infuſions of ſeveral plants, after the manner of oil of tartar; and, accordingly, I was quickly ſatisfied, upon trial, that the infuſion of roſe-leaves would, by a ſmall quantity of this ſolution, well mix'd with it, be immediately turn'd into a ſaddiſh green.

And further, I had often found, that oil of vitriol will precipitate many bodies, both mineral and others, diſſolv'd not only in *Aqua fortis*, but particularly in ſpirit of vinegar. I had alſo found, that the powders precipitated by this liquor, were uſually fair and white. Laying theſe things together, 'twas not difficult to conclude, that if upon a tincture of red roſe-leaves, made with fair water, I dropp'd a quantity of a ſtrong ſolution of *Minium*, the liquor would be turn'd into the like muddy green ſubſtance, whereto oil of tartar would reduce it; and that a convenient proportion of oil of vitriol, would have two diſtinct operations upon the mixture; the one, to precipitate the diſſolv'd lead in the form of a white powder; the other, to clarify the muddy mixture, and both reſtore, and exceedingly heighten the redneſs of the infuſion of roſes:

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and making the experiment accordingly, in a conical glaſs, that the ſubſiding powder might ſeem to poſleſs the more ſpace, and be the more conſpicuous; I found, upon ſhaking the green mixture, that the colour'd liquor might be the more equally diſpers'd, a few drops of the rectified oil of vitriol, preſently turn'd the opake liquor into one that was clear and red, almoſt like a ruby; and threw down a large quantity of a powder, which, when ſettled, would have appeared very white, if ſome interſpers'd particles of the red liquor had not a little allay'd the purity, tho' it did not blemiſh the beauty of the colour.

And to ſhow that theſe effects flow from the oil of vitriol, as it is a ſtrong acid menſtruum, that has the property both to precipitate lead, as well as ſome other ſubſtances, out of ſpirit of vinegar, and to heighten the colour of red roſe-leaves; I have done the ſame thing, tho', perhaps, not quite ſo well, with ſpirit of ſalt, but could not do it with *Aqua fortis*; becauſe tho' that potent menſtruum, as well as the others, heightens the redneſs of roſes, yet it would not, like them, precipitate lead out of ſpirit of vinegar, but rather diſſolve it. And as, by this way, we have produc'd a red liquor, and a white precipitate, out of a dirty green magiſtery of roſe-leaves; ſo, by the ſame method, we may produce a fair yellow, and ſometimes a red liquor, and the like precipitate out of an infuſion of a curious purple colour. For, I lately intimated, that I had, with a few drops of an alkali, turn'd the infuſion of log-wood into a lovely purple; but if, inſtead of that alkali, I ſubſtituted a very ſtrong, and well-filtred ſolution of *Minium*, made with ſpirit of vinegar; and took about half as much of this liquor as there was of the infuſion of log-wood; a convenient proportion of ſpirit of ſalt, would, if the liquors were briskly ſhook together, preſently ſtrike down a precipitate like the former, and turn the liquor that ſwam above it, for the moſt part, into a lovely yellow.

But to advance this experiment a little further, I conſider'd, that in caſe I firſt turn'd a ſpoonful of the infuſion of log-wood purple, by a convenient proportion of the ſolution of *Minium*; the affuſion of ſpirit of ſal-armoniac, would precipitate the corpuſcles of lead, conceal'd in the ſolution of *Minium*, and yet not deſtroy the purple colour of the liquor; whereupon I thus proceeded. I took about a ſpoonful of the freſh tincture of log-wood, (for if it be ſtale, the experiment will not always ſucceed) and having put to it a convenient proportion of the ſolution of *Minium*, to turn it into a deep and almoſt opake purple; I then dropp'd in as much ſpirit of ſal-armoniac as I gueſs'd would precipitate about half the lead; and immediately ſtirring the mixture well together, I mix'd the precipitated parts with the others, ſo that they fell to the bottom, partly in the form of a powder, and partly in the form of a curdled ſubſtance, that retain'd, as well as the upper liquor, a bluiſh purple colour, ſufficiently deep; and then inſtantly pouring in a pretty quantity of ſpirit of ſalt, the matter firſt precipitated, was, by the conical figure of the glaſs, preſerv'd from the ſpirituous ſalt, which ſuddenly precipitated upon it a new bed of white powder, or the remaining corpuſcles of the lead, that the urinous ſpirit had not ſtruck down; ſo that there appear'd in the glaſs, three diſtinct and very differently colour'd ſubſtances; a purple, or violet-colour'd precipitate at the bottom; a white and carnation precipitate over that; and, at the top of all, a tranſparent liquor, of a lovely yellow, or red.

Thus, tho' to ſome, I may ſeem to have hit on this, and the like experiments, by chance; whilſt others imagine they proceed from ſome extraordinary inſight into the nature of colours; yet, indeed, the contrivance of them need not to be look'd upon as any great matter, in one who is a little vers'd in my notions about the differences of ſalts.

Changes of colours produced in a dry, white body, by ſpring-water.

43. That the colour of a body may be chang'd by a liquor, which, of itſelf, is colourleſs, provided it be ſaline, we have already ſhewn by a multitude of inſtances; and the thing does not appear ſtrange, becauſe ſaline particles ſwimming up and down in liquors, have been often obſerv'd to act powerfully in the production and change of colours: but it has ſeem'd ſurprizing to many, unacquainted with chymical operations, that a white dry body ſhould immediately acquire a new rich colour, upon the bare affuſion of clear ſpring water. And yet the way of producing ſuch a change of colours, may be eaſily hit on by thoſe who often make ſolutions of mercury. For we have try'd, that tho' by evaporating a ſolution of quick-ſilver in *Aqua fortis*, till the remaining matter began to be tolerably dry, fair water, pour'd on the remaining calx, turn'd it but a little yellowiſh; yet, when we took good quick-ſilver, and three or four times its weight of oil of vitriol, and in a glaſs retort drew off the ſaline menſtruum from the metalline liquor, till there remain'd a dry ſnow-white calx at the bottom; upon pouring on it a large quantity of fair water, we did, almoſt in a moment, perceive it to paſs to one of the lovelieſt light yellows that ever we beheld. And the turbith mineral is of a colour not much inferior to this; tho' it be often made with a different proportion of the ingredients, and after a more troubleſome manner. But this colour, tho' ſo exquiſitely fine, and ſo greatly wanted by painters, is, I fear, too coſtly to be employ'd by them, unleſs about curious pieces; tho' I do not know how well it will agree with every pigment, eſpecially with oil-colours.

And whether this experiment be really of another nature, than thoſe wherein ſaline liquors are employ'd, may be ſo plauſibly doubted, that whether the water pour'd on the calx, do barely, upon imbibing ſome of its ſaline parts, alter its colour, changing its texture; or whether by diſſolving the coagulated ſalts, it becomes a ſaline menſtruum, and, as ſuch, operates upon the mercury, I leave to be conſider'd. I have, however, ſeveral times, with fair water, waſh'd from this calx abundance of ſtrongly-taſted corpuſcles, which, by the abſtraction of the menſtruum, I could reduce into ſalt.

But, to ſhew how much a real and permanent colour may be call'd out by a liquor that has neither colour, nor ſo much as ſaline, or other active parts; provided it can but bring the parts of the body it imbibes, to convene into cluſters, diſpos'd after a particular manner, we made the following experiment.

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A permanent colour produced by a particular arrangement of parts.

44. We put ſome powder of good common vitriol into a crucible, and kept it melted in a gentle heat, till, by the evaporation of ſome parts, and the tranſpoſition of the reſt, it had quite loſt its former colour; what remain'd we took out, and found it to be a friable calx, of a dirty grey colour: on this we pour'd fair water, which it did not tinge either green or blue, but only ſeem'd to make a muddy mixture with it; then ſtopping the vial wherein the ingredients were, we let it ſtand in a quiet place for ſome days. The water having now diſſolv'd a large part of the imperfectly calcin'd body; the vitriolic corpuſcles ſwimming to and fro in the liquor, had time, by their occurſions, to conſtitute many little maſſes of vitriol, which gave the water they impregnated, a fair vitriolic colour; and this liquor being pour'd off, the remaining dirty powder, in time, communicated the like colour, but not ſo deep, to a ſecond parcel of clear water, that we pour'd on it.

Various colours produced in different parts of the ſame liquor.

45. It may contribute to ſhew how much ſome colours depend upon the leſs or greater mixture, and contemperation of light with ſhades, to obſerve how the number of particles of the ſame colour, either receiv'd into the pores of a liquor, or ſwimming up and down in it, may ſeem greatly to vary the colour of it. I could produce ſeveral inſtances of ſolid bodies, wherein, if the colour be not a light one, as white, yellow, or the like, the cloſeneſs of parts in the pigment, makes it look blackiſh; tho' when laid on but thinly, it will, perhaps, appear blue, green, or red: but I ſhall rather inſiſt upon liquors, than dry bodies.

If, then, you put a little fair water into a clear ſlender vial, and let fall into it a few drops of a ſtrong decoction, or infuſion, of cochineal, or of brazil, the ting'd drops will deſcend like little clouds into the liquor, thro' which, if by ſhaking the vial, you diffuſe them, they will turn the water of a pink colour; by dropping in a little more of the decoction, you may heighten the colour into a fine red, almoſt like that of rubies; and by continuing the affuſion, the liquor may be brought to a kind of crimſon, and afterwards to a dark opake redneſs. And whilſt the liquor paſſes from one of theſe colours to the other, you may obſerve many leſs noted colours, bordering upon red, to which it is not eaſy to aſſign names; eſpecially conſidering how much the proportion of the decoction to the fair water, and the ſtrength of that decoction, may, with other circumſtances, vary the phenomena of this experiment. But, to make it with the greater conveniency, we uſe, inſtead of a vial, a ſlender pipe of glaſs, of about a foot in length, and about the thickneſs of a man's little finger: for, if leaving one end of this tube open, you ſeal up the other hermetically, or otherwiſe exactly ſtop it, you have a glaſs, wherein may be obſerv'd, the variations of the colours of liquors, much better than in large vials; and wherein experiments of this nature, may be made with very ſmall quantities of liquor. And in this pipe, may be produc'd various colours, in the various parts of the liquor, and be kept long ſwimming upon one another, unmix'd.

It has excited the admiration of ſome perſons to ſee what a variety of colours we have ſometimes produced in ſuch glaſſes, by the bare infuſion of brazil, variouſly diluted with fair water, and altered by the infuſion of ſeveral chymical ſpirits, and other ſaline colourleſs liquors; and when the whole mixture is reduced to an uniform degree of colour, I have made it appear to be of colours gradually differing, by pouring it into glaſſes of a conical figure. Or even take a large round vial, fill it with the red infuſion of brazil, hold it againſt the light, and you will diſcern a notable difference betwixt the colour of that part of the liquor which is in the body of the vial, and that which is more pervious to the light, in the neck.

I once had a glaſs, and a blue liquor, which was chiefly a certain ſolution of verdigreaſe, ſo fitted, that tho' in other glaſſes the experiment would not ſucceed; yet when this particular glaſs was filled with that ſolution, it appeared in the body of the vial of a lovely blue, and in the neck of a manifeſt green. I had alſo, a broad piece of glaſs, which being viewed againſt the light, ſeem'd clear enough; and, held from the light, appeared very little diſcoloured: yet it was a piece knock'd off from a great lump of glaſs, to which if we rejoin'd it, where it had been broken off, the whole maſs appeared green as graſs.

I have, likewiſe, ſeveral times uſed bottles and ſtopples, both made of the very ſame metal; and yet whilſt the bottle appeared only inclining to green, the ſtopple was of ſo deep a colour, that it could hardly be thought poſſible they ſhould be the ſame materials. And I have by me a flat glaſs, on which, if I look againſt the light with the broad ſide obverted to my eye, it appears like a good ordinary window-glaſs; but if I turn the edge of it to my eye, and ſtand in a convenient poſition, with regard to the light, it emulates an emerald.

I have ſometimes made a fluid kind of pigment, which, dropped on a piece of white paper, appears, where any quantity of it falls, of a crimſon colour; but, being ſpread thinly on the paper, preſently exhibits a fair green.

Let me add, that having made many experiments with that blue ſubſtance, called by the painters litmaſe; we have ſometimes obſerved, that being diſſolved in a due proportion of fair water, the ſolution, either oppoſed to the light, or dropped upon white paper, appeared of a deep colour, betwixt crimſon and purple; yet, when ſpread very thin on the paper, and ſuffered to dry there, the paper was thereby ſtain'd of a fine blue. This experiment alſo ſucceeded, when made on a flat piece of pure white glazed earth. And having let fall a few drops of the ſtrong infuſion of this litmaſe, in fair water, into a fine cryſtal glaſs, ſhaped like an inverted cone, and almoſt fill'd with clear water; I had the pleaſure to ſee theſe few tinged drops, variouſly diſperſing themſelves thro' the limpid water, exhibit many colours, or varieties of purple and crimſon. But when the corpuſcles of the pigment ſeem'd to have equally diffuſed themſelves thro' the whole; by adding to it two or three drops of ſpirit of ſalt, we perceived it firſt made an odd change in the colour of the liquor, as well as a viſible commotion among its ſmall parts, and in a ſhort time changed it wholly into a very glorious yellow, like that of a topaz. After this, if I let fall a few drops of a ſtrong heavy ſolution of pot-aſhes, whoſe weight would quickly ſink it to the ſharp bottom of the glaſs; there would ſoon appear four very pleaſant and diſtinct colours: *viz.* a faint bright one, at the ſharpeſt part of the glaſs; a purple, a little higher; a deep and glorious crimſon, in the confines betwixt the purple and the yellow; and an excellent yellow, the ſame that before adorn'd the whole liquor, reaching from thence to the top of the glaſs. And if I poured, very gently, a little ſpirit of ſal-armoniac upon the upper part of this yellow, there would alſo ariſe there a purple, or a crimſon, or both; ſo that the unaltered part of the yellow liquor appeared intercepted betwixt the two neighbouring colours. Hence we need not be ſurprized at the tricks of thoſe mountebanks, who are commonly called water-drinkers. For tho' not only the vulgar, but many perſons far above that rank, have wondered to ſee a man, after drinking large quantities of fair water, return it in the form of claret, ſack and milk; yet having by chance had occaſion to oblige a wanderer, who made a profeſſion of this, and other juggling tricks, he ingenuouſly confeſſed to me, that the art conſiſted rather in a few tricks than any great skill in altering the nature and colours of things. And I ſuſpect there may be a great deal of truth in a little pamphlet, printed long ago in *Engliſh*; wherein the author undertakes to diſcover, from the confeſſion of ſome of the accomplices themſelves, that a famous water-drinker, then much admired in *England*, performed his pretended tranſmutations of liquors by the help of two or three inconſiderable preparations and mixtures of obvious fluids; and chiefly of an infuſion of brazil variouſly diluted and made pale, yellowiſh, *&c.* with vinegar. And, for my part, what moſt ſurprizes me in this affair, is, that the drinkers can take down ſo much water, and ſpout it out again with violence; tho' cuſtom, and a vomit ſeaſonably taken before-hand, may in ſome of them greatly facilitate the work. But as for the changes they make in liquors, thoſe are but few and ſlight.

Changes of colour may greatly depend upon the peculiar texture of the menſtruum.

46. *Helmont* uſed to make a preparation of ſteel, which a very ingenious chymiſt ſometimes employs for a *Succedaneum* to the ſpaw-waters; diluting this *Eſſentia Martis liquida*, as he calls it, with a due proportion of water. And tho' this preparation be almoſt of the colour of a *German* amethyſt, and conſequently remote from green; yet a very few drops of it being let fall into a large proportion of *Rheniſh*, or white-wine, it immediately turns them to a lovely green. By which phenomenon we may learn how requiſite it is in experiments, about the changes of colours, carefully to regard the circumſtances of them; for water will not, as I have purpoſely try'd, concur to the production of any ſuch green; nor did it give that colour to moderate ſpirit of wine, wherein I diſſolved it: and wine itſelf is a liquor that few would ſuſpect able, of a ſudden, to work any ſuch change in a metalline preparation of this nature. And to ſatisfy myſelf that this new colour proceeds rather from the peculiar texture of

the wine, than from any greater acidity that *Rheniſh*, or white-wine has, in compariſon of water; I ſharpened the ſolution of this eſſence in fair water, with a large quantity of ſpirit of ſalt; and then the mixture acquired no greenneſs.

To vary the experiment a little, I try'd, that if into ſome *Rheniſh* wine made green by this eſſence, I dropped an alkaline ſolution, or urinous ſpirit; the wine would preſently grow turbid, and of an odd dirty colour. But if inſtead of diſſolving the eſſence in wine, I diſſolved it in fair water, ſharpen'd with a little ſpirit of ſalt; then either the urinous ſpirit of ſal-armoniac, or the ſolution of the fixed ſalt of pot-aſhes, would immediately turn it of a yellowiſh colour; the fixed or ruinous ſalt precipitating the vitriolic ſubſtance contained in the eſſence. And as our eſſence imparts a greenneſs to wine, but not to water; *Olaus Wormius* tells us of a rare kind of turn-ſol he had, whoſe beautiful redneſs would be eaſily communicated to water, but ſcarce to wine, and not at all to ſpirit of wine: in which laſt circumſtance it agrees with our eſſence, tho' they diſagree in other particulars.

The different colours of metals in different ſtates.

47. I have often taken notice, that metals, as they appear to the eye, before they come to be altered by other bodies, exhibit colours very different from thoſe which the fire, or a menſtruum, either ſeparately or conjointly, produce in them; eſpecially conſidering, that theſe metalline bodies are, after all their diſguizes, reducible not only to their former conſiſtence, and other more eſſential properties, but to their colour too; as if nature had given them an external and an internal colour. But, upon a more attentive conſideration of this difference of colours, it ſeems probable to me, that many of thoſe we call internal, are rather produced by the coalition of metalline particles with thoſe of the ſalts, or other bodies employ'd to work on them, than by the bare alteration of the parts of the metals themſelves. Of theſe adventitious colours of metalline bodies, the chief ſorts ſeem to be three; *viz.* ſuch as are produced by the ſole action of the fire; ſuch as emerge from the coalition of metalline particles, with thoſe of ſome menſtruum employ'd to corrode, or precipitate the metal; and, laſtly, the colours afforded by metalline bodies, either melted with, or otherwiſe penetrating into others, eſpecially ſuch as are fuſible.

As to the firſt of theſe colours; 'tis well known to chymiſts, that tin calcined by fire alone, affords a white calx; and lead, by the ſame means, yields that common red powder we call *Minium*; copper alſo, calcined *per ſe*, by a long or violent fire, gives a very dark or blackiſh powder; iron, likewiſe, may, by the action of reverberated flames, be turn'd into a colour almoſt like that of ſaffron, as we ſee in the preparation of *Crocus Martis per ſe*; and mercury, by the power of fire, will be turn'd into a red powder, call'd precipitate *per ſe*.

Beſides, the ſame metal may, by the ſucceſſive operations of the fire, receive ſeveral adventitious colours; as is evident in lead, which, before it arrives at ſo deep a colour as that of *Minium*, may paſs thro' ſeveral others.

And, not only the *Calces*, but the glaſſes of metals, vitrify'd *per ſe*, may have colours different from the obvious or natural colours of the metal; as I have obſerved in the glaſs of lead, made by long expoſing that metal crude to a violent fire. I have likewiſe ſeen a piece of very dark glaſs, which an ingenious artificer, who ſhewed it me, profeſſed he made of ſilver alone, by an extreme violence of the fire.

Minerals alſo, by the action of the fire, may be brought to afford colours very different from their own; as was obſerved about the variouſly coloured flowers of antimony. To which we may add, the whitiſh grey colour of its calx, and the yellow or reddiſh one of the glaſs, into which that calx may be fluxed. And vitriol, calcined with a very gentle heat, and afterwards with higher degrees of it, may be made to paſs thro' ſeveral colours, before it deſcends to a dark purpliſh one, whereto a ſtrong fire will at length reduce it.

48. The adventitious colours produc'd in metals, by ſaline liquors, are many of them, well known to chymiſts. That gold, diſſolv'd in *Aqua regia*, communicates its own colour to the menſtruum, is a common obſervation; but the ſolutions of mercury, in *Aqua fortis*, are not generally obſerv'd to give any notable tincture to the menſtruum; tho' ſometimes, when the liquor firſt falls upon the quickſilver, I have obſerv'd a very remarkable greenneſs, or blueneſs to be produc'd. Tin, corroded by *Aqua fortis*, till the menſtruum will work no farther on it, becomes exceeding white; and eaſily, of itſelf, acquires the conſiſtence not of a metalline calx, but of a coagulated matter, ſo like either to curdled milk, or curdled whites of eggs, that a perſon unacquainted with ſuch ſolutions, might eaſily be miſtaken in it. But when I purpoſely prepar'd a menſtruum that wou'd diſſolve it, as *Aqua fortis* diſſolves ſilver, not barely corrode it, and quickly let it fall again; I remember no particular colour in the ſolution: as if the more whitiſh metals did not much tinge their menſtrua, tho' the high-colour'd ones, as gold and copper, do. For lead diſſolv'd in ſpirit of vinegar, or *Aqua fortis*, gives a clear ſolution: and, if the menſtruum be abſtracted, appears either diaphanous, or white. And 'tis worth noting, that tho' when iron is diſſolv'd in oil of vitriol, diluted with water, it affords a ſalt, or magiſtery, ſo like in colour, as well as ſome other qualities, to green vitriol, that chymiſts properly call it, *Vitriolum Martis*; yet, by changing the menſtruum, and pouring upon the filings of ſteel, *Aqua fortis*, inſtead of oil of vitriol, I obtain'd not a green, but a ſaffron-colour'd ſolution, or rather a thick liquor, of a deep yellowiſh red. Common ſilver, diſſolv'd in *Aqua fortis*, yields a ſolution ting'd like that of copper; which is not to be wonder'd at, becauſe, in coining ſilver, they give it an allay of copper; and what is ſold for refin'd ſilver, is not ſo perfectly free from that ignobler metal, but that a ſolution of it in *Aqua fortis* will give its tincture to the menſtruum. But we could not obſerve, upon the ſolution of ſome ſilver perfectly refin'd, that the menſtruum, tho' held againſt the light, in a cryſtal vial, manifeſtly diſclos'd any tincture; only it ſometimes ſeem'd not quite deſtitute of a very faint bluiſh caſt.

But, of all the metals, there is not one which ſo eaſily and conſtantly diſcloſes its colour as copper. For, in acid menſtrua, as *Aqua fortis*, and ſpirit of vinegar, it not only gives a bluiſh green ſolution; but if it be almoſt any way corroded, it appears of one of thoſe two colours. And ſo conſtant is the diſpoſition of copper, notwithſtanding the diſguize artiſts put upon it, to diſcover its colour, that we have, by forcing it up with ſal-armoniac, obtain'd a ſublimate of a bluiſh caſt. Nay, a famous chymiſt affirms, that the very mercury of it is green; but till he teaches us an intelligible way of making ſuch a mercury, we muſt content ourſelves to ſay, that we have had a cupreous body precipitated out of a diſtill'd liquor, which ſeem'd to be the ſulphur of that metal, and even when flaming, appear'd of a greeniſh colour. And, indeed, copper is a metal ſo eaſily wrought upon, by liquors of ſeveral kinds, that, I might ſay, I know not any mineral which concurs to the production of ſuch a variety of colours, as copper diſſolv'd in ſeveral menſtrua, *viz.* ſpirit of vinegar, *Aqua fortis*, *Aqua regia*, ſpirit of nitre, of urine, of ſoot, oils of ſeveral kinds, *&c.* if the variety of them were not comprehended within the limits of greeniſh blue, or bluiſh green.

But being deſirous to try if I could not with crude copper make a green ſolution, without the bluiſhneſs that uſually accompanies it, I concluded upon two menſtrua, which, tho' I had not known employ'd to work on this metal, prov'd ſucceſsful; the one was ſpirit of ſugar, and the other, oil, or ſpirit of turpentine, which affords a fine green ſolution, uſeful on ſeveral occaſions. And yet to ſhew that the adventitious colour may reſult as well from the true and permanent copper itſelf, as the ſalts wherewith 'tis corroded; if you take a piece of good *Dantzick* copperas, or any other vitriol wherein copper is predominant, and having moiſten'd it with fair water, rub it upon a bright piece of iron, or ſteel; it will (as we have formerly ſaid) preſently ſtain it with a reddiſh colour, like that of copper.

We have ſometimes try'd what colours ſuch minerals as tin-glaſs, antimony, ſpelter, *&c.* would yield in ſeveral menſtrua. The like we have alſo done with ſtones; among which, that famous one call'd by *Helmont*, *Paracelſus's Ludus*, has afforded in menſtrua able to diſſolve ſo ſolid a ſtone, ſometimes a yellowiſh, and ſometimes a red ſolution. And from minerals I have obtain'd, with ſeveral menſtrua, very different colours; and ſome ſuch, as, perhaps, would ſcarce be expected from ſuch bodies.

The colours of metals may, in many caſes, be further alter'd, by employing either precipitating ſalts, or other convenient ſubſtances, to act upon their ſolutions. If quickſilver be diſſolv'd in *Aqua fortis*, and precipitated out of the ſolution, either by water impregnated with ſea-ſalt, or the ſpirit of that concrete, it falls to the bottom in the form of a white powder; but if precipitated with an alkali, it affords a yellowiſh powder; and if no precipitation be made, and the menſtruum be drawn off with a convenient fire, the corroded mercury will remain at the bottom, and may be made to appear of different colours, by different degrees of heat.

Thus having abſtracted *Aqua fortis* from ſome quick-ſilver that we had diſſolv'd in it, till there remain'd a white calx: by expoſing that to ſeveral degrees of fire, and afterwards to a naked one, we obtain'd ſome new colours; and at length, the greateſt part of the calx lying at the bottom of the vial, and being brought partly to a deep yellow, and partly to a red colour, the reſt appear'd elevated to the top and neck of the vial; ſome in the form of a reddiſh, and ſome of an aſh-colour'd ſublimate. And even the ſuccedaneum to a menſtruum, may ſometimes ſerve to change the colours of a metal. The lovely red, which painters call vermilion, is made of mercury, which appears of the colour of ſilver; and of brimſtone, which reſembles that of gold, ſublim'd up together in a certain proportion.

49. The third of the principal kinds of adventitious colours in metals, is that produc'd by aſſociating them, eſpecially when calcin'd with other fuſible bodies, and eſpecially *Venice*, and other fine colourleſs ſorts of glaſs.

I formerly gave an example of a metal, imparting a colour to glaſs, very different from its own, in ſhewing how ſilver turns it to a lovely golden colour; I ſhall now add, that tho' I learn'd from one of the chief artificers in painted glaſs, that thoſe of his trade colour it yellow, with a preparation of the calx of ſilver; yet, having mix'd a few grains of ſhell-ſilver, ſuch as is employ'd with the pencil and pen, with a convenient proportion of powder'd cryſtal-glaſs, and kept them for two or three hours in fuſion, I was ſurpriz'd to find the melted maſs appear, upon breaking the crucible, of a lovely ſapphirine blue: which made me ſuſpect my ſervant might have brought me a wrong crucible; but he conſtantly affirm'd it to be the ſame wherein the ſilver was put; and conſiderable circumſtances countenanc'd his aſſertion: whence, till farther trial, I ſuſpect either that ſilver, which is not very probable, brought to a perfect fuſion with glaſs, may impart other colours thereto, than when neal'd upon it; or elſe, which is leſs unlikely, that tho' ſilver-beaters uſually chuſe the fineſt coin they can get, as that which ſpreads moſt under the hammer; yet the leaf-ſilver, whereof this ſhell-ſilver was made, might retain ſo much copper, as to give the predominant tincture to the glaſs.

Take it, as another inſtance of the adventitious colours of metals, that tho' copper, calcin'd *per ſe*, affords but a dark and baſely colour'd calx, yet the glaſs-men tinge their glaſs green therewith. And, I remember, when taking ſome crude copper, and by frequent ignition, and quenching it in water, till we had reduced it to a darkiſh-colour'd powder; and afterwards keeping it in fuſion in about a hundred times its weight of fine glaſs, we obtain'd a blue-colour'd maſs, which wou'd, perhaps, have been green, if we had hit upon the right proportion of the materials, the degree of fire, and the time wherein it ought to be kept in fuſion. But tho' copper thus gives ſomewhat near the like colour to glaſs, that it communicates to *Aqua fortis*; yet it ſeems worth inquiring, whether thoſe new colours, which mineral bodies diſcloſe in melted glaſs, proceed from the coalition of the corpuſcles of the mineral with the particles of the glaſs, or from the action of the alkaline ſalt (a principal ingredient of glaſs) upon the mineral body; from the concurrence of both theſe cauſes, or from any other.

We may obſerve, that putty, made by calcining together a proportion of tin and lead, as it is itſelf a white calx; ſo it turns the matter of the purer ſort of glaſs, wherewith it is melted, into a white maſs; which, if it prove opake enough, is employ'd for white amel.

When the materials of glaſs, melted with calcin'd tin, have compoſed an opake white maſs; 'tis made the baſis of all thoſe fine concretes the goldſmiths, and ſeveral artificers employ in the curious art of enamelling: for this white fuſible ſubſtance receives and preſerves the colours of many other mineral ſubſtances, which, like it ſelf, will endure the fire.

And, as it appears, that ſeveral minerals will impart to fuſible maſſes, colours different from their own; ſo, by the making and compounding of amels, it is manifeſt, that many bodies will both retain their colour in the fire, and impart the ſame to ſome others wherewith they were vitrify'd. Thus we formerly ſaw, that a blue and yellow compoſed a green amel. And 'tis pretty to ſee, that ſome colours are of ſo fixed a nature, as to be capable of mixture, without receiving any damage from the fire; and that mineral pigments may be mix'd by it, almoſt as regularly and ſucceſsfully as the vulgar colours, by the help of water, in the vats of dyers.

Both metalline, and other mineral bodies, may be employ'd to give tinctures to glaſs; and 'tis worth obſerving, how ſmall a quantity of ſome mineral ſubſtances will tinge a comparatively vaſt proportion of it. We have ſometimes attempted to colour glaſs even with precious ſtones; and had no cauſe to think the experiment ill beſtow'd. 'Tis known that the artificers in glaſs tinge their metal blue, with that dark mineral, zaffora, which ſome would have to be a mineral earth; others, a ſtone; and others, again, neither; but it is confeſſedly of a dark, not a blue colour. 'Tis, likewiſe, a remarkable practice among them, that they employ magnaneſe to exhibit in glaſs, not only other colours than its own, (wherein it is ſo like to the load-ſtone, that it is given by mineraliſts for one reaſon of its *Latin* name *Magneſia*) but colours different from one another: for tho' they uſe it to clarify their glaſs, and free it from that bluiſh greeniſh colour, which it wou'd otherwiſe be ſubject to; yet they alſo employ it in certain proportions, to tinge their metal, both with a red colour, and with a purpliſh, or murrey; and, by putting in a greater quantity, they alſo make with it that deep obſcure glaſs, which paſſes for black. And this agrees very well with what we formerly obſerv'd of the apparent blackneſs of thoſe bodies that are over-charg'd with the corpuſcles of ſuch pigments, as are red, blue, green, *&c.*

An eaſy method of examining ores.

And as by ſeveral metals, and other minerals, we can give various colours to glaſs; ſo, on the other hand, from the different colours that mineral ores, or other mineral powders, by being melted with glaſs, diſcloſe therein, a probable conjecture may be often made of the metal, or known mineral, that a propos'd ore either contains, or is neareſt allied to. And this eaſy method of examining ores, may, in ſome caſes, prove very ſerviceable; but great circumſpection is requiſite to keep it from proving fallacious, upon account of the variations of colour, producible by the different proportions that may happen, betwixt the ore and the glaſs; by the richneſs and poorneſs of the ore itſelf; by the degree of fire; and eſpecially by the length of time, during which the matter is kept in fuſion.

There is another way, different from thoſe already mentioned, whereby metals may be brought to exhibit adventitious colours. And, in this caſe, the metal does not ſo much impart a colour to another body, as receive one from it; or rather, both bodies, by the new texture reſulting from their mixture, produce a new colour. I will not here inſiſt upon the examples afforded us by yellow orpiment, and common ſea-ſalt; from which, ſublimed together, chymiſts unanimouſly affirm their white or cryſtalline arſenic to be made: but 'tis worth noting, that tho' yellow orpiment be acknowledged by far the moſt predominant of the two ingredients of arſenic; yet arſenic, duly added to the higheſt coloured metal, copper, when in fuſion, gives it a whiteneſs both within and without. Thus, alſo, *Lapis Calaminaris* changes and improves the colour of copper, by turning it into braſs. And I have ſometimes, by the help of zink, duly mixed after a certain manner, given to copper as rich a golden colour as ever I ſaw in the beſt true gold.

The way of making counterfeit gems.

Upon knowing the different methods of producing the adventitious colours of metals and minerals in bodies capable of vitrification, depends the pretty art of making counterfeit gems: for whilſt pure ſand, or calcined cryſtal, gives the body in their preparation; 'tis, for the moſt part, ſome metalline or mineral calx, mixed in a ſmall proportion with it, that gives the colour. Calcined lead, fuſed with fine white ſand, or cryſtal, reduced by ignitions and ſubſequent extinctions in water, to a ſubtile powder, will, of itſelf, be brought, by a due decoction, to give a clear maſs, coloured like a *German* amethyſt. But this colour may be eaſily over-powered by thoſe of ſeveral other mineral pigments, ſo that with a glaſs of lead you may emulate the freſh and lovely green of an emerald; tho', in many caſes, the colour which the lead itſelf, upon vitrification, tends to, may vitiate that of the pigment deſign'd to appear in the maſs. Theſe colours alſo depend ſo much upon the texture of the materials, that we have made the glaſs of lead itſelf, compoſed of about three parts of litharge, or *Minium*, fuſed with one of cryſtal, or ſand, very finely powdered, paſs thro' different colours, according as we kept it more or leſs in fuſion. But the degrees of coction, and other circumſtances, may ſo vary the colour produced, that, in a ſmall crucible, I have had fragments of the ſame maſs; in ſome of which, perhaps, not ſo big as a hazel-nut, four diſtinct colours might be diſcerned.

Beſides the three mentioned ſorts of adventitious colours in metals, there may be others reducible under the ſame head; of which I ſhall inſtance only in two.

The firſt is afforded us from the practice of ſcarlet-dyers. A moſt famous maſter in this art aſſured me, that neither he, nor others, can ſtrike the lovely colour, called the bow-dye, unleſs their materials be boiled in veſſels of a particular metal. Secondly, metals will afford uncommon colours, by imbuing ſeveral bodies with ſolutions of them, made in proper menſtrua. Thus, tho' copper, plentifully diſſolved in *Aqua fortis*, will communicate to ſeveral bodies the colour of that ſolution; yet ſome other metals will not; as I have often try'd. Gold, diſſolved in *Aqua regia*, dyes the nails and skin, the hafts of knives, and other things made of ivory, not with a yellow, but a purple colour; which, tho' it manifeſt itſelf but ſlowly, is very durable, and can ſcarce ever be waſhed out. Thus we formerly ſaid, that the purer cryſtals of fine ſilver made with *Aqua fortis*, tho' they appear white, will preſently dye the skin and nails of a black, not to be waſhed off like ordinary ink. And many other bodies may, in the ſame manner, be dy'd, ſome of a black, and others of a blackiſh colour.

Mineral ſolutions may give colours different from their own.

Mineral ſolutions, alſo, may produce colours different from thoſe of the liquors themſelves. I ſhall not fetch an example of this from what happens in the ſalting of beef, which often appears green, and ſometimes of a reddiſh colour, when boil'd; nor ſhall I inſiſt on the practice of giving, by ſalt-peter, and a certain proportion of common ſalt, a fine redneſs to ſuch fleſh as would otherwiſe appear purely white: I rather chuſe to ſay, that I have ſeveral times found a ſolution of the ſulphur of vitriol, or even of common ſulphur, tho' the liquor appeared clear, immediately to tinge a piece of new coin, or other clean ſilver, ſometimes with a golden, ſometimes with a deeper, and more reddiſh colour; according to the ſtrength of the ſolution, and the quantity of it, that chanced to adhere to the metal. This renders it the leſs ſurprizing, that the water of the hot ſpring at *Bath*, abounding with diſſolved ſubſtances of a very ſulphureous nature, ſhould, for a while, tinge ſuch pieces of coin yellow, as are for a due time immerſed in it. And even vegetable liquors, whether by degeneration, or by altering the texture of the body that imbibes them, may ſtain other ſubſtances with colours very different from their own. 'Tis affirmed, that the green juice of *Alcanna* dyes the skin and nails of a laſting red.

The method of preparing a yellow vegetable lac.

50. Meeting, in an *Italian* author, with a way of preparing what he calls a *Lacca* of vegetables, by which the *Italians* mean a kind of extract fit for painting, like that rich *Lacca*, in *Engliſh* commonly call'd lac, employ'd by painters as a glorious red; and finding the experiment conſiderable, but very imperfectly related, we improved upon it in the following

manner: tho' the thing, as there delivered, is, by the *Italians*, eſteem'd a great ſecret.

Put what quantity of powdered turmeric you pleaſe into fair water; adding to every pound of that liquor a large ſpoonful of a very ſtrong lixivium of pot-aſhes, clarify'd by filtration; let all this ſimmer over a ſoft fire in a clean glazed earthen veſſel, till you find, by the immerſion of a ſheet of white paper, that the liquor is ſufficiently impregnated with the golden tincture of the turmeric: then take the decoction off the fire, and filtre or ſtrain it; and afterwards, leiſurely dropping into it a ſtrong ſolution of roch-alum, the decoction will, as it were, be curdled; and the tinged part of it either emerge, ſubſide, or ſwim up and down in little yellow flakes: when, if you pour this mixture into a funnel, lined with cap-paper, the liquor that formerly filtred yellow, will now paſs clear, and leave its tinged parts behind in the filtre; into which fair water muſt be ſo often poured, till the matter therein contain'd be dulcify'd; that is, till the water paſſes thro' it, as taſteleſs as when it was poured on. But if, without filtration, you would obtain the flakes of this vegetable lac, pour a large quantity of fair water upon the decoction, after the affuſion of the aluminous ſolution; and you ſhall find the liquor to grow clearer, and the lac to ſettle together at the bottom, or emerge to the top of the water: tho', ſometimes, having not employ'd a ſufficient quantity of fair water, the lac has partly ſubſided, and partly emerged, leaving all the liquor clear in the middle. But, to make this lac fit for uſe, it muſt, by repeated affuſions of freſh water, be dulcify'd from the adhering ſalts, as well as that ſeparated by the filtre; and be ſpread to dry leiſurely upon pieces of cloth, with brown paper, chalk, or bricks under them, to imbibe the moiſture.

Alum, being a ſtony matter diſſolved by acid, may, when uſed as a precipitate, be, itſelf, precipitated.

'Tis ſuppoſed, that the magiſtery of vegetables, obtain'd by this means, conſiſts only of the more ſoluble and coloured parts of the reſpective plant that affords it. But I muſt take the liberty to queſtion the ſuppoſition: for, according to my notion of ſalts, alum, tho' to ſenſe a homogeneous body, ought not to be reckoned among true ſalts, but is to be look'd upon as a kind of magiſtery; ſince, as native vitriol contains both a ſaline ſubſtance, and a metal corroded and aſſociated with it; ſo alum, (which is ſo near allied to vitriol, that, in ſome places of *England*, the ſame ſtone will ſometimes afford both) ſeems manifeſtly to contain a peculiar kind of acid ſpirit generated in the bowels of the earth, and a kind of ſtony matter diſſolved by it. In making our ordinary alum, 'tis true, the workmen uſe the aſhes of a ſea-weed, vulgarly called kelp, and urine; yet I am inform'd, that here in *England*, there is, beſides the factitious alum, another ſort made by nature, without the help of thoſe additions. Now, conſidering this compoſition of alum, and that alkaline ſalts precipitate what acid ones have diſſolved; I could not but ſuſpect that the curdled matter, call'd the magiſtery of vegetables, may have in it a conſiderable proportion of a ſtony ſubſtance, precipitated out of the alum by the lixivium wherein the vegetable had been boiled. And, to ſhew there is no neceſſity that all the curdled ſubſtance muſt belong to the vegetable; I took a ſtrong ſolution of alum, and having filtred it; by pouring in a convenient quantity of a ſtrong ſolution of pot-aſhes, I preſently turn'd the mixture into a white curdled matter; which, committed to the filtre, left a great quantity of a very white ſtony calx, that ſeem'd to be of a mineral nature, as well from other ſigns, as that little bits of it being put upon a live coal, they did neither melt, nor fly away, whilſt that was blown upon; and a quantity of this white ſubſtance, being for a long time kept in a red-hot crucible, was neither diminiſhed nor ſpoiled: hot water too, wherein I kept another parcel of ſuch calx, ſeem'd only to waſh away the looſer ſalts from the ſtony ſubſtance. And I have, by gentle exhalation, recovered from the liquor that paſſed thro' the filtre, and left this calx behind, a body, in appearance like ſalt; for 'twas very white, and conſiſted of innumerable exceeding ſlender ſhining particles, which would, in part, eaſily melt at the flame of a candle, and, in part, fly away with ſome little noiſe. I have likewiſe, with urinous ſalts, ſuch as the ſpirit of ſal-armoniac, as well as with the ſpirit of urine, and even with ſtale urine undiſtilled, eaſily precipitated ſuch a white calx out of a limpid ſolution of alum. Upon the whole, circumſpection is required in judging of the nature of aluminous liquors by precipitation; otherwiſe, we may ſometimes imagine that to be precipitated out of a liquor by alum, which is rather precipitated out of alum by the liquor.

This method of making lacs we have alſo practiſed with madder, which yielded us a red lac; and with rue, which afforded an extract nearly of the ſame colour with that of its leaves. But, becauſe 'tis here, principally, the alkaline ſalt of the pot-aſhes, which enables the water ſo powerfully to extract the tincture of the vegetables; I fear the decoction will not always be of the very ſame colour with the vegetable it is made of. For lixivious ſalts, tho', by penetrating and opening the bodies of vegetables, they prepare and diſpoſe them to part with their tincture readily; yet they not only draw out ſome tinctures, but likewiſe alter them: as will eaſily appear from ſeveral of the experiments already delivered. And tho' alum be of an acid nature, and may, in ſome caſes, deſtroy the adventitious colours produced by the alkali, and reſtore the former; yet we have produced examples, that, in many caſes, an acid will not reſtore a vegetable ſubſtance to the colour deſtroy'd by an alkaline ſalt; but make it aſſume a third, very different from both. Beſides, I have, after this manner, made magiſteries of brazil, of cochineal, and of other things, red, yellow, or green; that appeared, ſome of them, of a rich colour, and others of no bad one: in ſome, however, the colour of the lac ſeem'd rather inferior to that of the plant; and in others again, both very different, and much worſe.

But 'tis time to conclude this sketch of a hiſtory of colours.

I ſhall attempt to build no theory * upon the experiments and obſervations here delivered; but leave the ſubject to the proſecution of others. I only make it my requeſt, that the reader would not preſently conclude me miſtaken in giving the matters of fact concerning the changes of colours ſet down, tho' he ſhould not immediately find them exactly agreeable to his own experiments. For, beſides the contingencies to which this kind of trials is obnoxious, the omiſſion or variation of a ſeemingly inconſiderable circumſtance, may hinder the ſucceſs of an experiment,

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* From the diſcoveries of Sir *Iſaac Newton* we may now ſettle the whole theory of colours; which, as it regards natural bodies, is, in ſhort, this.

Firſt, 'tis found by experience, that the rays of light are compoſed of diſſimilar particles; that is, ſome of them are, probably, larger than others: as appears from their different refrangibility.

2. Thoſe particles of light which are moſt refracted, make rays of a violet colour; that is, the moſt minute particles of light, being thus ſeparately collected, probably excite the ſhorteſt vibrations in the *Retina*; which are thence propagated to the brain, along the ſolid *Capillamenta* of the optic nerves, and there excite the ſenſe of a violet colour, the moſt faint and languid of all colours; whilſt the particles which are leaſt refracted, make a red ray; that is, the greateſt particles of light make the longeſt vibrations in the *Retina*; and ſo excite the ſenſation of red, the moſt vivid colour: the other particles, according to their ſeveral intermediate degrees of magnitude and refrangibility, exciting the intermediate colours; almoſt in the ſame manner as the vibrations of the air, by their different magnitudes, cauſe different ſounds.

3. The colours of theſe rays are not adventitious modifications of them, but original, primitive, and neceſſary properties, depending, probably, upon the magnitudes of their parts; and being conſtant and immutable, they are unalterable by any farther refraction, reflexion, or other modification whatever.

4. As light is ſeparated into rays of different colours by the refractions of a glaſs priſm, and other groſs bodies; ſo it is differently ſeparable by very thin plates of any tranſparent matter: for all ſuch plates that have leſs than a determinate thickneſs, ſuffer the rays of all colours to paſs thro' them, and reflect none: but as their thickneſs is increaſed in an arithmetical proportion, they begin to reflect, firſt the blue rays, then the green, yellow, and pure red; next, the blue, green, yellow, and red, more dilute and mixed; till at length, at a certain thickneſs, they reflect all the rays of light intimately united together, as they fall thereon; that is, white. But on whatever parts theſe thin plates reflect any colour, as blue for inſtance, they there tranſmit the contrary colour; as, in this caſe, a red or yellow.

5. Now all natural bodies conſiſt of very thin tranſparent plates, which being ſo laid together, that no reflections or refractions can be made in their interſtices, the body becomes tranſparent: but if the diſtances between them are ſo great, or fill'd with ſuch a matter, or are ſo empty of matter, that many reflexions and refractions are made within the body, this body will be opake. And, therefore, thoſe opake bodies which conſiſt of the thinneſt plates, are black; thoſe which conſiſt either of the thickeſt, or of ſuch as differ greatly in their thickneſs from one another, whence conſequently they become fit to reflect all colours, are white: but ſuch as conſiſt of plates of different intermediate thickneſſes, are blue, green, yellow, or red; becauſe they ſeverally reflect the rays of thoſe colours more copiouſly than the other rays, which they either in great meaſure ſtifle and abſorb, or ſometimes tranſmit. And hence it is, that ſome liquors appear red, or yellow, by reflected light, but blue by tranſmitted light; and that leaf-gold appears yellow by the former, but green by the latter, &c. See *Clark. Annotat. in Rohault. p.* 194, 195. & *Newton. Optic. p.* 320,——323. & *alibi paſſim.*

wherein no other fault has been committed. Thus in dying ſcarlet, tho' you ſhould ſee every ingredient that is uſed about it; tho' I ſhould particularly inform you of the weight of each; and tho' you ſhould be preſent at the kindling of the fire, and at the increaſing and remitting of it, whenever the degree of heat is to be altered; and tho', in a word, you ſhould ſee every thing done ſo particularly, that you would ſcarce harbour the leaſt doubt of your comprehending the whole art: yet if I ſhould not tell you, that the veſſels, which immediately contain the tinging ingredients, are to be made of, or lined with tin; you would never be able, by a due obſervance of all the other circumſtances, to bring the tincture of cochineal to give a perfect ſcarlet.

A
FREE INQUIRY
Into the Vulgar
NOTION
OF
NATURE.

SECT. I.

As the human ſoul is, itſelf, a true and poſitive Being, 'tis apt to conceive all other things, as ſuch. But this propenſity, I fear, makes us think and ſpeak of chimerical things, and of negations or privations, as of true and poſitive Beings.

We ſhould, therefore, be very careful of being inſenſibly miſ-led by ſuch an innate and unheeded temptation to error, as we bring into the world with us. And, perhaps, among other particulars, in which this deluding propenſity of our minds has too great an influence; it may have impos'd on us, in the notion we uſually frame of nature. Now, this notion being the fruitful parent of others; and being ſo general in its applications, and important in its influence, we ought not over-eaſily to admit it: doubtleſs, it moſt highly deſerves to be warily examin'd, before it be throughly entertain'd.

The vulgar notion of nature, prejudicial to religion and philoſophy.

I have ſometimes ſeriouſly queſtion'd, whether the vulgar notion of nature, has not been both injurious to the glory of God, and a great impediment to the ſolid and uſeful diſcovery of his works.

For, it ſeems to detract from the honour of the great author and governor of the world; that men ſhould aſcribe moſt of the admirable things to be met with in it, not to him, but to a certain nature, which themſelves do not well know what to make of. 'Tis true, many confeſs that this nature is a thing of his eſtabliſhing, and ſubordinate to him; but, tho' they own it, when they are ask'd the queſtion, yet there are ſeveral, who ſeldom or never regarded any higher cauſe. And whoever takes notice of their way of aſcribing things to nature, may eaſily diſcern, that, whatever their words ſometimes are, the agency of God is little in their thoughts. Doubtleſs, it ſhews the wiſdom of God, to have ſo fram'd things at firſt, that there can ſeldom or never need any extraordinary interpoſition of his power; or the employing, from time to time, an intelligent overſeer, to regulate, aſſiſt, and control the motions of matter.

Ariſtotle, by introducing the notion of the eternity of the world, in moſt men's opinion, at leaſt, openly deny'd to God the production of it; and by aſcribing the admirable works of the divine Being, to what he calls nature, tacitly denies him the government thereof.

But my opinion hinders me not at all from acknowledging God to be the author of the univerſe, and the continual preſerver and upholder of it (which is much more than the peripatetic hypotheſis allows) for thoſe things which the ſchool-philoſophers aſcribe to the agency of nature, interpoſing upon emergencies, I aſcribe to the wiſdom of God, in the firſt fabric of the univerſe; which he ſo admirably contriv'd, that, if he but continue his ordinary and general concourſe, there will be no neceſſity of extraordinary interpoſitions: ſo that mere matter, particularly determin'd, ſhall, in certain conjunctures of circumſtances, do all that philoſophers aſcribe on ſuch occaſions, to nature.

This notion is more reſpectful to a divine Being, than to imagine, as we commonly do, that he has appointed an intelligent and powerful agent, as his vice-gerent, continually to watch for the good of the univerſe in general, and of the particular bodies that compoſe it; whilſt this Being appears not to have the skill, or the power, to prevent ſuch irregularities as often prove deſtructive to multitudes of animals, and other noble creatures, as in plagues, *&c.* and ſometimes prejudicial to greater portions of the univerſe, as in earth-quakes, *&c.*

But farther, whilſt men indulge themſelves ſo general and eaſy a way of ſolving difficulties, as to attribute them to nature; ſhame will not reduce them to a more induſtrious ſearch after the reaſons of things; nor curioſity itſelf greatly move them to it. Thus the cauſe of the aſcent of water in pumps, and other phenomena of that kind, had never been known, if the moderns had acquieſced in that imaginary one, that the world was govern'd by a watchful Being, call'd nature, who abhors a *Vacuum*; and conſequently is always ready to do whatever is neceſſary to prevent it.

And the veneration men commonly have for what they call nature, has obſtructed and confined the empire of man over the inferior creatures. For many have look'd upon it as impoſſible to compaſs, and others, as impious to attempt, the removing thoſe boundaries which nature ſeems to have ſettled among her productions. And whilſt they look upon her as ſuch a venerable thing, ſome make a ſcruple of conſcience to endeavour ſo to imitate any of her works, as to excel them.

'Twill here, I doubt not, be ſaid, that I contradict the ſenſe of the generality of mankind: I anſwer, that in philoſophical inquiries, we are not ſo ſollicitous about what has been, or is believ'd, as what ought to be; and certainly the ſenſe of the generality of men, ought little to ſway us in ſome particular queſtions. But 'tis no wonder men ſhould be generally prepoſſeſs'd with ſuch a notion of nature, as I call in queſtion; ſince education has imbued them with it from their infancy: and even in their maturer years, they find it taken for granted, and employed by the moſt learned writers, and never hear it call'd in queſtion. Beſides, it exceedingly complies with our innate propenſity, to think that we know more than we do: to vouch nature for a cauſe, is an expedient, that can ſcarce be wanting upon any occaſion, to be produced as a reaſon for any thing we are ignorant of.

But to talk of a thing as a real and poſitive Being, and attribute great matters to it, weighs but little with me, when I conſider, that, tho' fortune be only a certain looſe and undetermin'd notion, which a modern metaphyſician would refer to the claſs of non-entities; yet not only the *Gentiles* made it a goddeſs, whom many of them ſeriouſly worſhipp'd, but eminent writers, both heathen and chriſtian, ancient and modern, and all ſorts of men, in their common diſcourſe, ſeriouſly talk of it, as of a kind of antichriſt, that uſurped a great ſhare in the government of the world; and aſcribe little leſs to it, than to nature. And to paſs over what poets, moraliſts, and divines tell us of the powers of ignorance and vice, which are but moral defects; the generality of mankind ſeriouſly attribute a great and fatal dominion to death; which, tho' ſaid to do ſo many and ſuch wonderful things, is neither a ſubſtance, nor a poſitive entity, but a mere privation. As for revelation, it countenances no ſuch notion as that vulgarly receiv'd of nature: the word is not once mention'd in ſcripture; no, not by *Moſes*, in his account of the creation. And, indeed, till the *Iſraelites* were over-run, and corrupted by idolatrous nations, there was, for many ages, a deep ſilence as to ſuch a Being.

There are two things whereof I muſt advertiſe the reader, before I proceed farther.

1. That when, in the preſent diſcourſe, I ſpeak of the opinions of *Ariſtotle*, I mean thoſe that are by the generality of ſcholars taken for his, or the *Ariſtotelian* and *Peripatetic* doctrines; by which, if he be miſ-repreſented, the blame ought to light upon his commentators, and followers.

2. That I here ſet aſide the conſideration of the rational ſoul or mind of man, all other parts of the univerſe being, according to the receiv'd opinion, the works of nature.

A conſidering perſon may well ſuſpect, that men have generally had but imperfect and confuſed notions concerning nature; ſince they apply that name to ſeveral things, and thoſe too ſuch, as have, ſome of them, very little dependance on, or connexion with the others. *Ariſtotle* has a whole chapter expreſly written to enumerate the various acceptations of the *Greek* word Φύσις, commonly render'd nature; of which, he there mentions ſix; and, in *Engliſh*, we have more ſignifications of that term. Sometimes, we uſe it,

1. For that author of nature, whom the ſchool-men call *Natura naturans*; as when 'tis ſaid, that nature hath made man partly corporeal, and partly immaterial.

2. Sometimes we mean by the nature of a thing, the eſſence, or that which the ſchool-men call the quiddity of a thing; that is, the attribute which makes it what it is, whether the thing be corporeal, or not; as when we attempt to define the nature of an angel, a triangle, or a fluid.

3. Sometimes we confound that which a man has by nature, with what accrues to him by birth; as, when we ſay, that ſuch a man is noble by nature.

4. Sometimes we take nature for an internal principle of motion; as when we ſay, that a ſtone let fall in the air, is, by nature, carried towards the centre of the earth; and, on the contrary, that fire, or flame, naturally moves upwards.

5. Sometimes we underſtand by nature, the eſtabliſh'd courſe of things; as when we ſay, that nature makes the night ſucceed the day; or that nature hath made reſpiration neceſſary to the life of men.

6. Sometimes we take nature for an aggregate of powers belonging to a body, eſpecially a living one; as, when phyſicians ſay, that nature is ſtrong, weak, or ſpent; or that in ſuch or ſuch diſeaſes, nature left to herſelf, will perform the cure.

7. Sometimes we take nature for the univerſe, or ſyſtem of the corporeal works of God; as when 'tis ſaid of a phenix, or a chimera, that there is no ſuch thing in nature.

8. Sometimes, too, and that moſt commonly, we would expreſs by the word nature, a ſemi-deity, or other ſtrange kind of Being; which is the notion we here examine.

And, beſides theſe more abſolute acceptations of the word nature, it has ſeveral others more relative. Nature is ſet in oppoſition, or contradiſtinction to other things; as when a ſtone falls downwards, we ſay, it does it by a natural motion; but that if it be thrown upwards, its motion that way is violent. So chymiſts diſtinguiſh vitriol into natural and factitious. In like manner 'tis ſaid, that water kept ſuſpended in a ſucking-pump, is not in its natural place, as that is, which ſtagnates in the well. We ſay, alſo that wicked men are ſtill in the ſtate of nature; but the regenerate, in a ſtate of grace: that cures wrought by medicines, are natural operations; but the miraculous ones, wrought by Chriſt and his apoſtles, ſupernatural. Nor are theſe the only forms of ſpeech that might be alledg'd, to manifeſt the ambiguity of the word nature; tho' ſome of theſe already mention'd, ſhould be judged ſo near as to be co-incident. Among *Latin* writers, the acceptations of the word nature are ſo many, that I remember, one author reckons up no leſs than fourteen or fifteen. Hence we ſee how eaſy 'tis for the generality of men, without excepting thoſe who write of natural things, to impoſe upon others and themſelves, in the uſe of a word ſo apt to be miſ-employ'd.

I have often look'd upon it as an unhappy thing, and prejudicial both to philoſophy and phyſic, that the word nature hath been ſo frequently, and yet ſo unskilfully employ'd, by all ſorts of men. For the very great ambiguity of this term, and the promiſcuous uſe made of it, without ſufficiently attending to its different ſignifications, render many of the expreſſions wherein 'tis employ'd, either unintelligible, improper, or falſe. I, therefore, heartily wiſh, that philoſophers, and other leading men, would, by common conſent, introduce ſome more ſignificant, and leſs ambiguous terms and expreſſions, in the room of the licentious word nature; and the forms of ſpeech that depend on it: or, at leaſt, decline the uſe of it, as much as conveniently they can; and where they think they muſt employ it, declare in what clear and determinate ſenſe they uſe it. For unleſs ſomewhat of this kind be done, men will very hardly avoid being led into miſtakes, both of things, and of one another; whence ſuch wranglings about words and names will be ſtill kept on foot, as are uſually managed with much heat, and little advantage.

But 'tis far more difficult than any one, who hath not try'd, would imagine, to diſcourſe long of the corporeal works of God, and eſpecially of the operations and phenomena attributed to nature, and decline the frequent uſe of that term, or forms of ſpeech whereof 'tis a principal part; without frequent, and tedious circumlocutions. But to avoid, as much as poſſible, this inconvenience; I ſhall hereafter call thoſe who maintain the vulgar notion of nature *Naturiſts:* an appellation I rather chuſe than that of *Naturaliſts*, becauſe many, even of the learned among them, are not philoſophers. This inconvenience might perhaps be a little farther remedied, if,

Means of avoiding this ambiguity.

1. Inſtead of the word nature, taken for *Natura naturans*, we uſe the term God, which 'tis put to ſignify.

2. If, inſtead of nature, uſed for that which makes a thing what it is, we employ the word eſſence; ſometimes alſo we may make uſe of the word quiddity; which, tho' a barbarous term, is yet frequently employ'd, and well underſtood in the ſchools; and tho' very comprehenſive, is free from ambiguity.

3. If what is meant by the word nature, taken for what belongs to a living creature at its nativity, or accrues to it by its birth, be expreſſed, ſometimes, by ſaying, that an animal is born ſo; and, ſometimes, by ſaying, that a thing has been generated ſuch; and, ſometimes alſo, that 'tis thus or thus qualified by its original temper and conſtitution.

4. If, inſtead of the word nature, taken for an internal principle of local motion; we ſay, ſometimes, that this or that body moves as it were, or ſeems to move ſpontaneouſly, upwards, downwards, *&c.* or that it is

put into this or that motion, or determined to this or that action, by the concourſe of particular cauſes.

5. If, inſtead of nature, uſed for the eſtabliſhed courſe of things corporeal, we ſubſtitute, what it denotes, the eſtabliſhed order, or the ſettled courſe of things.

6. If, inſtead of nature, taken for an aggregate of the powers belonging to a body, we employ the conſtitution, temper, mechaniſm, or the complex of the eſſential properties or qualities; and, ſometimes, the condition, the ſtructure, or the texture of that body: and, ſpeaking of the greater portions of the world, we may uſe the terms, fabric of the world, ſyſtem of the univerſe, *&c.*

7. If, where men employ the word nature for the univerſe, or the ſyſtem of the corporeal works of God; we uſe the word world, or univerſe; and, inſtead of the phenomena of nature, ſubſtitute, the phenomena of the univerſe, or of the world.

8. And, laſtly, if, inſtead of uſing the word nature, taken for either a goddeſs, or a kind of ſemi-deity; we wholly reject, or very ſeldom employ it.

Whether the nature of a thing be the law it receives from the creator.

Some, I know, will have the nature of every thing, to be only the law that it receives from the creator, and according to which it acts on all occaſions. And, indeed, this opinion, tho' neither clear nor comprehenſive, ſeems capable of a fair conſtruction. There is often ſome reſemblance between the orderly and regular motions of inanimate bodies, and the actions of agents, that proceed conformably to laws. And I, ſometimes, ſcruple not to ſpeak of the laws of motion and reſt, that God has eſtabliſhed among things corporeal, and, now and then, to call them the laws of nature.

But, in ſtrictneſs, to ſay, that the nature of this or that body, is but the law of God preſcribed to it, is an improper, and figurative expreſſion. For this gives us but a very defective idea of nature, ſince it omits the general fabric of the world, and the contrivances of particular bodies; which yet, are as neceſſary as local motion itſelf, to the production of particular effects and phenomena: and, to ſpeak properly, a law being but a notional rule of acting according to the declared will of a ſuperior, 'tis plain, that nothing but an intellectual Being can be properly capable of receiving and acting by a law. For if it does not underſtand, it cannot know what the will of the legiſlator is; have any intention to accompliſh it; or act with regard thereto. Now 'tis intelligible, that God ſhould, at the beginning, impreſs determinate motions upon the parts of matter, and guide them, as he thought requiſite, for the primordial conſtitution of things: and that ever ſince he ſhould, by his ordinary and general concourſe, maintain thoſe powers, which he gave the parts of matter to tranſmit their motion to one another. But I cannot conceive, how a body, deſtitute of underſtanding and ſenſe, truly ſo called, can moderate and determine its own motions; eſpecially ſo, as to make them conformable to laws, that it has no knowledge of. And that inanimate bodies, how ſtrictly ſoever called natural, properly act by laws, cannot be proved by their acting ſometimes regularly, and, as men think, in order to determinate ends: ſince, in artificial things, we ſee many motions very orderly perform'd, and with a manifeſt tendency to particular and deſign'd ends. Thus, in a watch, the motions of the ſpring, wheels, and other parts, are ſo fitted and regulated, that the hand upon the dial-plate moves with great uniformity, and ſeems to moderate its motion, ſo as not to arrive at the points that denote the time of the day, either a minute ſooner, or a minute later than it ſhould do. And when a man ſhoots an arrow at a mark, ſo as to hit it; tho' the arrow moves towards the mark, as it would, if it could, and did deſign to ſtrike it; yet none will ſay, that this arrow moves by a law, but by an external impulſe.

Ariſtotle's *definition of nature obſcure and unſatisfactory.*

But, poſſibly, the definition of a philoſopher may exempt us from the perplexities, to which, by the ambiguous expreſſions of common writers, we are expoſed. I therefore conſidered, with more than ordinary attention, the famous definition of nature, that is left us by *Ariſtotle*; which I ſhall recite rather in *Latin* than *Engliſh*, becauſe 'tis very familiarly known, among ſcholars in that language; and becauſe there is ſomewhat in it, that ſeems difficult to be, without circumlocution, render'd intelligibly in *Engliſh*: *Natura* (ſays he) *eſt principium & cauſa motûs & quietis ejus, in quo ineſt, primò per ſe, & non ſecundum accidens.* Now as, according to *Ariſtotle*, the whole world is but a ſyſtem of the works of nature; we might well expect, that the definition of a thing, the moſt important in natural philoſophy, ſhould be clearly and accurately deliver'd; yet, to me, this celebrated definition ſeems ſo dark, that I receiv'd no aſſiſtance from it, towards framing a clear and ſatisfactory notion of nature. For I dare not hope, that what, as to me, is not itſelf intelligible, ſhould make me underſtand what is to be explained by it. And, conſulting ſome of *Ariſtotle*'s interpreters upon the ſenſe of this definition, I found the more conſiderate of them ſo puzzled with it, that their diſcourſes about it ſeem'd to tend, rather to free the definer from tautology and ſelf-contradiction, than to manifeſt the definition itſelf to be good and inſtructive. And indeed, tho' the immoderate veneration they have for their maſter, engages them to make the beſt they can of his definition, even when they cannot juſtify it without ſtrain'd interpretations; yet what every one ſeems to defend in groſs, almoſt every one of them cenſures in parcels; this man attacking one part of the definition, and that another, with objections ſo weighty, that if I had no other arguments to urge againſt it, I might borrow enow from the commentators on it, to juſtify my diſlike thereof.

Several things are commonly received as belonging to the idea of nature, that are not manifeſtly or at all comprehended in this *Ariſtotelian* definition, which never declares whether the principle, or cauſe here mentioned, is a ſubſtance, or an accident; and if a ſubſtance, whether corporeal or immaterial: nor is it clearly contain'd in this definition, that nature does all things moſt wiſely, and ſtill acts by the ſhorteſt ways, without ever miſſing of her end; and that ſhe watches againſt a *Vacuum*, for the welfare of the univerſe.

SECT. II.

THE beſt way to diſcover the common opinion of nature, is, to conſider what axioms paſs for current about her, what titles and epithets are unanimouſly given her by philoſophers, other writers, and by the generality of men who have occaſion to diſcourſe of her, and her actions.

The receiv'd notion of nature; what?

Of ſuch axioms and epithets, the principal ſeem to be theſe.

Nature is exceeding wiſe, and all her works are perform'd with underſtanding. Nature does nothing in vain. Nature never fails of her purpoſe. Nature always does what is beſt. Nature always acts in the ſhorteſt manner. Nature is neither too laviſh, nor too ſparing in neceſſary things. Nature always preſerves itſelf. Nature cures diſeaſes. Nature always watches to preſerve the univerſe. Nature dreads a Vacuum.

From which particulars put together, it appears, that the vulgar notion of nature may be expreſſed by ſome ſuch deſcription as this.

Nature is a moſt wiſe being, that acts nothing in vain, never miſſes of her ends, but always does what is beſt, and that by the moſt direct way, neither employing any things ſuperfluous, nor being wanting in things neceſſary; teaching and inclining every one of her works to preſerve itſelf: and as, in the human frame, ſhe cures diſeaſes; ſo, in the world, for the conſervation of the univerſe, ſhe abhors a *Vacuum*; making particular bodies act contrary to their own inclinations and intereſts to prevent it.

If I were to propoſe a notion, as leſs unfit than any I have met with, to paſs for the principal one of nature; with regard to which, many axioms and expreſſions, relating to that word, may be conveniently underſtood; I ſhould firſt diſtinguiſh between the univerſal, and the particular nature of things. And of univerſal nature, the notion I would offer, ſhould be ſomething like this. *Nature is the aggregate of the bodies that make up the world, in its preſent ſtate, conſidered as a principle, by virtue whereof they act and ſuffer, according to the laws of motion, preſcribed by the author of things.* And this makes way for the other ſubordinate notion; ſince the particular nature of an individual conſiſts in the general nature, apply'd to a diſtinct portion of the univerſe; or, ſuppoſing that placed as it is, in a world, framed by God, like ours, it muſt be a convention of the mechanical properties (ſuch as magnitude, figure, order, ſituation, and local motion) of parts, convenient and ſufficient to conſtitute of, or entitle to its particular ſpecies or denominations, the particular body they make up; the concourſe of all theſe being conſidered as the principle of motion, reſt, and changes in that body.

A new notion of nature, general and particular, advanced.

The vulgar notion of nature, as it has had, doubtleſs, may have an ill effect on religion. The looking upon merely corporeal, and often inanimate things, as endow'd with life, ſenſe, and underſtanding; and aſcribing to nature, and

Ill effects of the vulgar notion of nature upon religion.

Physics.

and ſome other Beings, things that belong to God alone, have been grand cauſes of the polytheiſm and idolatry of the *Gentiles*.

The moſt ancient idolatry, probably, was the worſhip of the celeſtial lights, eſpecially the ſun and moon; as appears both from ſacred and profane hiſtory. The great *Hippocrates* attempts to account for the origin of the world from his immortal heat, which he eſteems as a deity. And *Galen* himſelf, who was not unacquainted with *Moſes*'s writings, and with chriſtianity, fancy'd the earth had a certain ſoul or mind imparted to it by the ſuperior bodies. And an inquiſitive perſon, who, having liv'd many years in *China*, and ſeveral of the neighbouring kingdoms, aſſured me, that, in a ſolemn conference he had with ſome of the more eminent doctors of the *Chineſe* religion, they frankly profeſſed, they believed the heavenly bodies to be truly divine, and deſerving to be worſhipped, becauſe they imparted to men ſuch good things, as light, heat, rain, *&c.* And this belief, they declared, they thought more rational than that of the *Europeans*, who worſhip a deity, whoſe ſhape, colour, and motion, and whoſe efficacy on ſublunary things, were inviſible.

Many of the ancient philoſophers held the world to be animated, and believed, that the mundane ſoul was not barely a living, but a moſt intelligent and wiſe active Being; the *Stoics* held the world to be an animal; and the notion of the ſoul of the world, and of nature, ſeem ſo near allied, that ſome of the old ſages appear to have confounded them, and to have made no other univerſal nature, than the ſoul of the world. However, the great and pernicious errors they were led into, by believing that the univerſe itſelf, and many of its nobler parts, beſides men, were endowed with life, underſtanding, and providence; may make us chriſtians jealous of admitting ſuch a Being, as that which men venerate under the name of nature: ſince they aſcribe to it as many wonderful powers and prerogatives, as the idolaters did to their adored mundane ſoul. They, alſo, ſacrilegiouſly abuſed this Being, as well under the very name of nature, as under that of the ſoul of the world: ſometimes making it the ſame with the world, at others the ſame with God; and *Orpheus* has a hymn addreſs'd to nature as a goddeſs.

Ariſtotle, indeed, and his commentators, do not ſo directly idolize nature, as did *Orpheus*; yet, I doubt, they go further than they can juſtify, when they ſo freely and often aſſert many extraordinary things of her, particularly when they call the works of God, the works of nature, and mention him and her together, not as a creator and a creature, but as two coordinate governors.

Thoſe who thought the ſun endowed, not only with a living ſoul, but with underſtanding and a will, muſt, if they had duly conſider'd, have been much more puzzled, to find food for ſo vaſt a body, and organs in him neceſſary to prepare and digeſt it, and to perform the other functions that belong to animal nutrition, than the philoſophers who maintain'd him to be fire. 'Tis without proof preſumed and aſſerted, that the celeſtial bodies are endow'd with underſtanding and prudence, eſpecially ſo as to be able to know the particular conditions and tranſactions of men, and hear and grant the prayers of their worſhippers. And the moon, which was anciently a principal deity, is ſo rude and mountainous a body, that 'tis a wonder ſpeculative men, who conſider'd how many, how various, and how noble functions belong to a ſenſitive ſoul, could think a maſs of matter, ſo very remote from being fitly organiz'd, ſhould be animated and govern'd by a true, living and ſenſitive ſoul. Indeed, theſe deifiers of the celeſtial globes, and the heathen diſciples of *Ariſtotle*, beſides ſeveral of the ſame mind among the chriſtians, ſay great and lofty things of the quinteſſential nature of the heavenly bodies, and their conſequent incorruptibility; of the regularity of their motions, and of their divine quality of light that makes them refulgent. But the perſuaſion they had of this quinteſſential nature of the ſuperior part of the world, ſeems not grounded upon any ſolid phyſical reaſon, but entertain'd by them for being agreeable to the opinion they had of the divinity of the celeſtial bodies; of which, *Ariſtotle* himſelf ſpeaks in a way that hath greatly contributed to ſuch an exceſſive veneration for thoſe bodies, as is neither agreeable to true philoſophy, nor true religion. He himſelf takes notice, that the *Pythagoreans* held our earth to be one of the planets; and that it moved about the ſun, which they placed in the middle of the world. And ſince this hypotheſis of the earth's motion was in the laſt age reviv'd by *Copernicus*, not only *Kepler*, *Galileo*, and *Gaſſendus*, but moſt of the eminent modern aſtronomers, have embraced it: which, indeed, is far more agreeable to the phenomena, than the doctrine of *Ariſtotle* (who was plainly miſtaken about the order and conſiſtence of the heavens) and the ancient and generally received *Ptolemaic* ſyſtem. Now, ſuppoſing the terraqueous globe to be a planet; who can conſider it as a round maſs of very heterogeneous ſubſtances, whoſe ſurface is very rude and uneven, and its body opake, (unleſs as it happens to be enlightned by the ſun, moon, and ſtars) and ſo very inorganical and unfit for ſo much as nutrition, that it ſeems wholly unfit to be an animal, much leſs a rational one, and aſcribe underſtanding and providence to it? The like may be ſaid of the celeſtial bodies.

As for the boaſted immutability of the heavenly bodies; it may be very probably call'd in queſtion from the phenomena of ſome of the comets, that by their parallax were found to be above the moon, and conſequently in the celeſtial region of the world. But the incorruptibility and immutability of the heavenly bodies are more diſproveable by the ſudden and irregular generation, changes, and deſtruction of the ſpots of the ſun; which ſometimes ſo ſuddenly diſappear, that in the year 1660, on *May* 8. having left, in the morning, a ſpot, whoſe motions we had long obſerv'd thro' an excellent teleſcope, with an expectation that it would continue for many days viſible to us; we were ſurpriz'd to find, that when we came to obſerve it again in the evening, it was quite diſſipated: and by comparing it to the ſun, we eſtimated the extent of its ſurface to be equal to that of all *Europe*. As to the conſtancy of the motions of the ſtars; if the earth, which we know to be inanimate, is a planet, it moves as conſtantly and regularly about the ſun, as the other planets do, or as the moon doth about the earth. And ſuppoſing our globe was not a planet, yet there would manifeſtly be a conſtant regular motion of a great part of it: ſince there is a regular ebbing and flowing twice a day, and ſpring-tides twice a month, of that vaſt aggregate of waters, the ocean; which perhaps is not inferior in bulk to the whole body of the moon.

And laſtly, as a great proof of the divinity of the ſtars is drawn from their light; ſo, tho' I grant it to be the nobleſt of ſenſible qualities, yet I cannot think it a good proof of the divine nature of bodies endow'd with it. For tho' the *Zabians* and *Chaldeans* conſidered and adored the planets as the chief gods, our teleſcopes diſcover to us, that they ſhine but by a borrow'd light; ſo that *Venus*, as vivid as it appears to the naked eye, is ſometimes horn'd like the moon. Thus alſo the earth, whether a planet or no, is enlighten'd by the ſun; and poſſibly, as a body forty times bigger, communicates more light to the moon, than it receives from her; as ſeems probable from the light on the ſurface of the moon in ſome of her eclipſes. And tho' in the night, when the darkneſs hath widened the *Pupillæ* of our eyes, and the moon ſhines with an unrival'd luſtre, ſhe ſeems exceeding bright; yet ſhe may be, for ought I know, more opake than the ſolid part of the terreſtrial globe. For I have more than once obſerv'd a ſmall cloud in the weſt, where the moon then was, about ſun-ſet; and comparing them together, the cloud reflected the light as ſtrongly to my eye, as did the moon, that ſeem'd to be not far from it; both of them appearing like little whitiſh clouds: tho' afterwards, as the ſun deſcended lower beneath the horizon, the moon grew more luminous. And, ſpeaking of light indefinitely, 'tis ſo far from arguing a divine nature in the bodies endow'd with it, (whether, as the planets, by participation from an external illuminant; or, as the ſun, from an internal principle) that a calcined ſtone, witneſs that of *Bolonia*, will afford, in proportion to its bulk, incomparably more borrow'd light than one of the planets. And a light, from internal conſtitution, may be found, not only in ſuch abject creatures as inſects, whether winged, as the *Cucupias* of *Hiſpaniola*, or creeping, as the glow-worm; but alſo in bodies inanimate, and corrupted; as in rotten wood, putrefy'd fiſh, *&c.*

Reaſons againſt admitting the vulgar notion of nature.

The reaſons that have made me backward to entertain ſuch a notion of nature, as I have hitherto diſcourſed of, may be comprized under the following.

1. Such a nature ſeems to be either aſſerted, or aſſumed, without ſufficient proof; and this ſingle reaſon, if well made out, is alone ſufficient to diſcredit it. For, in matters of philoſophy, where we ought not to take any thing upon truſt, or allow it without proof; 'tis enough to keep us from believing a thing, that we have no poſitive argument to induce us to aſſent to it, tho' we have no particular arguments againſt it. Now I have yet met with no phyſical arguments, either demonſtrative, or conſiderably probable, to evince the exiſtence of the nature we examine; yet, ſurely, ſo univerſal an agent, ſuppoſed to act immediately, in abundance

of phenomena, ſhould, if it really exiſted, give ſome manifeſt proofs of itſelf.

2. Such a nature is unneceſſary. For, ſince a great part of the work of true philoſophers has been to reduce the principles of things to the ſmalleſt number they can, without making them inſufficient; why ſhould we take in a principle, of which we have no need? Suppoſing the common matter of all bodies to have been at firſt divided into innumerable minute parts by the wiſe author of things, and theſe parts to have been ſo diſpos'd, as to form the world as it now is; and, ſuppoſing the univerſal laws of motion, among the parts of matter, to have been eſtabliſh'd, and ſeveral conventions of particles contrived into the ſeminal principles of various things; all which may be effected by the mere motion of matter, ſkilfully guided at the beginning of the world: ſuppoſing all this, together with God's ordinary and general concourſe, which we very reaſonably may; I ſee not, why the ſame phenomena, that we now obſerve in the world, ſhould not be produc'd, without taking in any ſuch powerful and intelligent being, diſtinct from God, as nature is repreſented to be. And, 'till ſome inſtance is produc'd to the contrary, I ſhall think that the phenomena we obſerve, will genuinely follow from the mere fabric and conſtitution of the world. Thus, ſuppoſing the ſun and moon to have been put, at firſt, into ſuch motions about the earth, as experience ſhews they have; the determinate celerity of theſe motions, and the lines wherein they are performed, will make it neceſſary, that the moon ſhould exhibit ſuch ſeveral phaſes as every month ſhe doth; and that at ſome times ſhe and the ſun ſhould have a trine, or a quartile aſpect, *&c.* and that now one, and now the other of them, ſhould, at ſet times, ſuffer an eclipſe: tho' theſe eclipſes were by the *Romans*, and others of old, and are by many unlearn'd nations at this day, look'd upon as ſupernatural things. And, indeed, the difficulty we find to conceive how ſo great a fabric as the world can be preſerved in order, and kept from falling again to a chaos, ſeems to ariſe from hence, that men do not ſufficiently conſider the unſearchable wiſdom of the divine architect, whoſe piercing eyes were able to look at once quite thro' the univerſe, and take into his view, both the beginning and end of time: ſo that, perfectly knowing what would be the conſequences of all the poſſible conjunctures of circumſtances, into which matter, divided and mov'd according to particular laws, could, in an engine ſo conſtituted as the preſent world is, happen to be put; there can nothing fall out, unleſs when a miracle is wrought, that ſhall be able to alter the courſe of things, or prejudice the conſtitution of them, any further than he from the beginning foreſaw, and allow'd.

3. The nature I queſtion, is ſo dark and odd a thing, that 'tis hard to know what to make of it; being ſcarce intelligibly propos'd by thoſe who lay the moſt ſtreſs upon it. For it appears not clearly, whether they will have it to be a corporeal, or an immaterial ſubſtance; or ſome ſuch thing, as may ſeem to be betwixt both; ſuch as many *Peripatetics* repreſent ſubſtantial forms, and what they call real qualities, which ſeveral ſchoolmen

men hold to be ſeparable from all matter whatſoever. If it be merely corporeal, I confeſs, I underſtand not, how it can be ſo wiſe an agent as they would have it paſs for: if it be a body, I would gladly know what kind of body it is; and how, ſince, among bodies, there can be no penetration of dimenſions, this ſo intimately pervades, as they pretend, all the other bodies of the world? If it be ſaid, that nature is a *Semi-ſubſtantia*, as ſome modern ſchool-men call ſubſtantial forms, and real qualities; I anſwer, that I acknowledge no ſuch chymerical and unintelligible Beings. It remains therefore, that this nature we ſpeak of, if poſitive, is an immaterial ſubſtance. But, to have recourſe to ſuch an one, as a phyſical agent, and the grand author of the motion of bodies; and that, eſpecially, in ſuch familiar phenomena, as the aſcent of water in pumps, the ſuſpenſion of it in watering-pots, the running of it thro' ſyphons, *&c.* and thence to aſſign the cauſe hereof, will, I think, prove exceeding difficult.

4. Since many naturaliſts are chriſtians, it may not be improper to add, that another thing, for which I diſlike the vulgar notion of nature, is, its being dangerous to religion, in general, and, conſequently, to the chriſtian.

For this erroneous conceit defrauds the true God of ſeveral acts of veneration and gratitude, that are due to him from men, upon account of the viſible world; and diverts them to that imaginary being they call nature, which has no title to them: for, whilſt nature is ſuppos'd to be an intelligent thing, that wiſely and kindly adminiſters all that is done among bodies, 'tis no wonder that the generality of men ſhould admire and praiſe her, for the wonderful and uſeful things they obſerve in the world. And, in effect, tho' nature, in our preſent ſenſe of the word, be never found in the ſacred writings; yet nothing is more frequently met with in the books of philoſophers, than nature and her effects. And, whatever has been ſaid by ſome in excuſe of *Ariſtotle*, yet the generality of the *Peripatetics*, from whom the vulgar notion of nature is chiefly received, make the world eternal; and refer all the tranſactions among the bodies it contains, to what they call nature. Whence 'twill not be difficult to perceive, that, if they do not quite exclude God; yet, as they leave him no intereſt in the firſt formation of the univerſe, ſo they leave him but very little in the adminiſtration of the parts it conſiſts of, eſpecially in ſuch as are ſublunary. Inſtead, therefore, of the true God, they have ſubſtituted, for us, a kind of a goddeſs, with the title of nature; upon whom they look as the immediate agent and director in all excellent productions, and aſcribe to her the praiſe and glory of them.

This great error, in a point of ſuch importance, ſeems to undermine the foundation of religion. For ſince the moſt general and effectual argument which has perſuaded men that there is a God and a providence, is afforded by the conſideration of the viſible world, wherein ſo many operations and other things are obſerv'd to be managed with ſuch conduct and benignity, as cannot juſtly be aſcribed but to the wiſdom and goodneſs of a deity; they who aſcribe theſe things to mere nature, much weaken the force of that argument, if they do not quite take away the neceſſity of acknowledging a deity; by ſhewing, that, without having recourſe to him, an account may be given of the adminiſtration of the world, and of what is perform'd among things corporeal. Indeed, when men are put upon conſidering the matter, and preſs'd to declare themſelves more clearly; they fear to affirm, that God and nature are the ſame thing, and will confeſs, that ſhe is but his vicegerent; yet, in practice, their admiration, and their praiſes, are frequently given to nature, not to God.

In ſhort, as nature is ſo frequently recurr'd to, and ſo magnify'd in the writings of philoſophers, that the exceſſive veneration men have for her, cauſed ſome (as the *Epicureans*) to deny God; ſo, 'tis to be fear'd, that it makes many forget him. And, perhaps, a ſuſpicious perſon would add, that, were it not for other principles, this erroneous idea of nature would, too often, have a ſtrong tendency to ſhake, if not to ſubvert, the very foundations of all religion; by miſleading thoſe who are inclined to be its enemies, from overlooking the neceſſity of a God, to the denial of his exiſtence.

5. There are many phenomena, which do not agree with that notion or repreſentation of nature, I call into queſtion. For, if there were ſuch an intelligent, powerful, and vigilant being, as philoſophers deſcribe nature to be, ſeveral things would not be done, which experience aſſures us are done.

The great vigilance of nature, for the common good of the univerſe, is pretended to be demonſtrated from the care ſhe takes to prevent, or repleniſh a *Vacuum*; which would be very prejudicial to the fabric of the world. 'Tis alledged, that nature, in pumps, and the like caſes, raiſes the heavy body of water, in ſpight of its tendency towards the centre of the earth, to obviate, or fill up a vacuity; and that out of a gardener's watering-pot, or an inverted tube, ſtopped at one end, neither water, nor quick-ſilver will fall down, leſt it ſhould leave a *Vacuum* behind it: but I demand how it comes to paſs, that if a glaſs-tube be but a foot longer than 34 or 35 feet; or an inverted tube, fill'd with quick-ſilver, but a finger's breadth longer than 30 inches, the water in the one, and the quick-ſilver in the other, will ſubſide? Is it poſſible, that nature, which, in pumps, is ſaid to raiſe up every day ſo many hundred ton of water; and, if we believe the ſchools, would raiſe it to any height; ſhould not have the diſcretion, or the power, to lift up, or ſuſtain, as much water as would ſerve to fill one foot in a glaſs tube, or as much quick-ſilver as an inch of a ſlender pipe will contain; to obviate, or repleniſh, the *Vacuum*, ſhe is ſaid to abhor? Sure, at this rate, ſhe muſt either have very little power, or very little knowledge of the power ſhe has. So, likewiſe, when a glaſs-bubble is blown very thin at the flame of a lamp, and hermetically ſeal'd whilſt 'tis very hot, the reaſon aſſign'd for its being apt to break, when it grows cold, is, that the inward air, which was before rarify'd by the heat, coming to be condenſed by the cold, leſt the ſpace deſerted by the air, that thus contracts itſelf, ſhould be left void, nature, with violence, breaks the glaſs; but if the glaſs be blown a little ſtronger than ordinary, tho' at the flame of a lamp, the bubble, as I have often try'd, will continue unbroken, in ſpight of nature's abhorrence of a *Vacuum*. But if, tho' nature did not hinder the water, or the quick-ſilver, from falling down, there would no ſuch *Vacuum* enſue as ſhe is ſaid to abhor; why does ſhe ſeem ſo ſollicitous to hinder it; why keep three or four and thirty feet of water, in perpendicular height, contrary to the nature of all heavy bodies, ſuſpended in the tube; and why ſo furiouſly break to pieces a thin ſeal'd bubble, to hinder a *Vacuum*, if, in caſe ſhe did not break it, no *Vacuum* would enſue? On the other ſide, if we admit her endeavours, to hinder a *Vacuum*, not to have been ſuperfluous, we muſt confeſs, that, where theſe endeavours ſucceed not, there is really produced ſuch a *Vacuum*, as ſhe is ſaid to abhor: ſo that, either ſhe muſt be very indiſcreet to trouble herſelf, and tranſgreſs her own ordinary laws, to prevent a danger ſhe need not fear; or elſe her ſtrength muſt be very ſmall, that is not able to fill a ſmall vacuity, or to break a little glaſs-bubble.

Another grand inſtance given of the wiſdom of nature, and her watchfulneſs for the good of the whole world, is, the appetite ſhe has implanted in all heavy bodies, to deſcend to the centre of the earth, and in all light ones, to aſcend. But, for poſitive levity, till I ſee it better prov'd than it hath hitherto been, I allow no ſuch thing implanted in ſublunary bodies.

A ball being let fall upon the ground, will rebound, perhaps, ſeveral times before it reſts. If you aſk why the ball being dropt out of the hand, does not move on this or that ſide, or upwards, but fall directly toward the centre of the earth, in that ſhorteſt line, the diameter of the earth produced to the centre of gravity of the ball? 'Twill be readily anſwer'd, that this proceeds from the ball's gravity, an innate appetite, whereby it tends to the centre of the earth the neareſt way. But then I demand, whence comes this rebound, this motion upwards? for, 'tis plainly the genuine conſequence of the motion downwards, and is, therefore, increas'd as that motion in the ball was increas'd, by falling from a greater height: ſo that it ſeems, that nature, in ſuch caſes, plays a very odd game, ſince ſhe forces a ball, againſt the laws of heavy bodies, to aſcend ſeveral times upwards, upon account of that very gravity whoſe office it is to carry it downwards, the directeſt way: at leaſt, ſhe ſeems, in ſpite of the wiſdom aſcribed to her, to take her meaſures very ill, in making the ball move downwards with ſo much violence, as occaſions it ſeveral times to fly back from the place ſhe intended it ſhould go to. As if nature could not manage a ball without letting it be hurried on with far greater violence, than her deſign requir'd it.

The ſame may be ſaid of a pendulum: for, ſince 'tis unanimouſly affirm'd, that this falls to the perpendicular, upon account of its gravity, 'tis certainly a motion proceeding from the ſame gravity, that the ſwinging weight paſſes beyond the perpendicular, conſequently aſcends, and often makes a multitude of vibrations; and, therefore, very frequently aſcends, before it comes to reſt in the perpendicular.

There are ſeveral inſtances of perſons choak'd with a hair. The reaſon of this fatal accident, is probably, the irritation made, by the ſtay of ſo unuſual a thing in the throat; whence ariſe very violent and diſorderly motions to expel it, in the organs of reſpiration; by which means, the continual circulation of the blood, neceſſary to life, is hinder'd, the conſequence whereof is ſpeedy death. But this agrees very ill with the vulgar ſuppoſition of ſuch a kind and provident Being, as nature, who is repreſented as always at hand to preſerve the life of animals, and ſuccour them in their dangers and diſtreſſes: for ſince a hair is ſo ſlender a body, that it cannot ſtop the throat, ſo as to hinder, either the free paſſage of aliment into the ſtomach, or that of air to or from the lungs; were it not much better for nature, to let the hair ſtay there, 'till the juices of the body have reſolv'd or conſum'd it, or ſome favourable accident remov'd it, than like a paſſionate and tranſported thing, oppoſe it with ſuch blind violence, that inſtead of ejecting the hair, ſhe expels the life of the perſon?

How the care and wiſdom of nature will be reconcil'd to ſo improper and diſorderly a procedure, I leave her admirers to conſider. But it will appear very reconcileable to providence: for, in regard of the uſe and neceſſity of deglutition, and, in many caſes of coughing and vomiting, it was, in the general, moſt convenient, that the parts ſubſervient to theſe motions, ſhould be irritated by the ſudden ſenſe of things that are unuſual, tho', perhaps, they would not be otherwiſe dangerous or offenſive; becauſe it was fit that the providence of God ſhould, in providing for the welfare of animals, have more regard to that which uſually and regularly befals them, than to extraordinary caſes, or unfrequent accidents; and the like conſiderations will ſufficiently keep the divine providence from appearing to act irrationally or injuriouſly in other caſes.

It has been frequently obſerv'd, that pregnant women have been made to miſcarry by the ſmell of an extinguiſh'd candle, which would before have indeed diſpleas'd, but not endanger'd the ſame perſons: ſo that nature, in theſe caſes, ſeems very far from being ſo prudent and careful as men uſually fancy her, ſince by an odour ſhe is put into ſuch unruly tranſports: and inſtead of watching for the welfare of the woman, whoſe condition needed an extraordinary meaſure of her care and tenderneſs, ſhe violently precipitates her charge into a danger, that often proves fatal, not only to the mother, but alſo to the child.

How the groſs aberrations of nature, in the productions of monſters, will agree with that great uniformity, and excellent skill, aſcribed to her in ſeminal productions, I leave the naturiſts to conſider. Some of them, I know, lay the fault upon the ſtubbornneſs of the matter, that would not obey the plaſtic power of nature; but 'tis ſtrange it ſhould be pretended, that nature, which they make a kind of ſemi-deity, cannot mould and faſhion ſo ſmall, ſoft, and tractable a portion of matter, as that wherein the firſt model and efformation of the embryo is made; when, at the ſame time, they tell

Physics. tell us, ſhe is able, in ſucking-pumps, to raiſe and ſuſtain whole tons of water, to prevent a *Vacuum*; and can toſs up into the air, houſes, walls, caſtles, and the rocks they are built on, to let kindled gun-powder have the expanſion its new ſtate requires.

It may be ſaid, that unleſs we admit ſuch a Being as nature, to contrive and manage corporeal things, and, in a regular, and methodical way, direct them to their reſpective ends, there will appear no viſible footſteps or proof of a divine wiſdom in the world. This argument, I confeſs, is ſo ſpecious, that it made me long heſitate what to think of the receiv'd notion of nature. But having further conſider'd the matter, I ſaw it might be anſwer'd, that the curious contrivance of the univerſe, and many of its parts, and the orderly courſe of things, with a manifeſt tendency to determinate ends, are matters of fact, and depend not upon the ſuppoſition of ſuch a Being as they call nature; but, ſetting aſide this or that hypotheſis, may be known by inſpection, if thoſe who make it be attentive and impartial. Thus when a man ſees a human body skilfully diſſected, he cannot, if he be intelligent and unprejudic'd, but acknowledge, that there is a moſt curious and exquiſite contrivance in that incomparable engine, and in the various parts of it, which are admirably fitted for diſtinct and determinate uſes. So that I cannot ſuppreſs the manifeſt tokens of wiſdom and deſign, that are to be obſerved in the wonderful conſtruction and orderly operations of the world and its parts; but endeavour to refer theſe indications of wiſdom to the true and proper cauſe. And, as in the hypotheſis of the objectors, there may be three cauſes aſſign'd of theſe foot-ſteps of wiſdom, *God*, *Nature* and *Chance*; if, according to our doctrine, *Nature* be laid aſide, the competition will remain only between *God* and *Chance*: and ſure he muſt be very dull, or very ſtrongly prejudic'd, who ſhall think it reaſonable to attribute ſuch admirable contrivances, and ſuch regular conducts, as are obſervable in the corporeal world, rather to chance (which is no proper cauſe at all) than to a moſt intelligent Being, from which the moſt curious productions may well be expected. But if ſuch a celebrated thing, as nature is commonly thought, be admitted, 'twill not be near ſo eaſy to prove the wiſdom of God by his works, ſince theſe may have another cauſe; that moſt watchful and provident Being, which men call nature. And this will be more particularly difficult in the *Peripatetic* hypotheſis of the eternity of the world: for, according thereto, there appears no neceſſity, that God ſhould have any thing to do with it, ſince he did not make this *Automaton*, but it was always ſelf-exiſtent, not only as to matter, but as to form too: and as for the government or adminiſtration of the bodies it conſiſts of, that is the proper buſineſs of nature. If it be objected, that this Being is by its aſſertors acknowledg'd to be ſubordinate to God; I anſwer, that, as it may juſtly be queſtion'd, whether many philoſophers, and, perhaps, ſome ſects of them, who are adorers of nature, confeſs'd her to be but the ſubſtitute of a ſuperior and divine Being: ſo, this diſtinction and ſubordination is not eaſy to be prov'd againſt thoſe that ſide with the other ancient philoſophers, who either acknowledg'd no ſuch thing, or expreſly deny'd it. Beſides, this objection ſuppoſes the exiſtence and ſuperiority of a Deity, which therefore needs to be prov'd by other ways; whereas, on my hypotheſis, the ſame phenomena that diſcover admirable wiſdom, and manifeſt deſigns in the corporeal world, of themſelves afford a ſolid argument, both of the exiſtence, and of ſome of the grand attributes of God, with which the reſt, that properly belong to him, have a neceſſary connexion.

The reaſons whereon the vulgar notion of nature depends examined.

The reaſons which I conceive may have induced philoſophers to take up, and rely on the receiv'd notion of nature, are ſuch as theſe.

1. One of the moſt obvious may be taken from the general belief, or, as men ſuppoſe, obſervation, that ſeveral bodies, as particularly the earth, water, and other elements, have each of them its natural place aſſign'd it in the univerſe; from which place, if any portion of the element, or a mix'd body, wherein that element predominates, happens to be remov'd, it has a ſtrong inceſſant appetite to return to it; becauſe, when there, it ceaſes to gravitate, and is then in a place which nature has qualified to preſerve it.

Now, I readily grant, that there being ſuch a quantity of very bulky bodies in the world, 'twas neceſſary they ſhould have places adequate to their bigneſs; and 'twas thought fit by the wiſe architect of the univerſe, that they ſhould not be all blended together, but that a great portion of each ſhould, at the beginning of things, be diſpos'd of and lodg'd in a diſtinct and convenient place. But I ſee no neceſſity of granting what is aſſerted in the argument: for inanimate bodies having no ſenſe or perception, it muſt be all one to them in what place they are; becauſe they cannot be concern'd to be in one place rather than in another, ſince ſuch a preference would require a knowledge that inanimate things have not. And, for the ſame reaſon, a portion of an element, remov'd by force or chance, from what they call its proper place, can have no real appetite to return thither; for, who tells it that 'tis an undue place, and that it may better its condition by removing into another? And, who informs it, whether that place lies on this ſide of it, or on that, above it, or beneath it? Some philoſophers, indeed, have been aware of the weakneſs of the argument, drawn from the vulgar inſtance of terreſtrial bodies; which being let fall from an eminence, or thrown into the air, fall of themſelves, in a direct line, towards the centre of the earth; and therefore they have ſtrengthen'd it, by pretending, that theſe bodies have not (as the ancients pretended) an appetite to deſcend to the centre of the earth, but to the great maſs of their connatural bodies. I will not, therefore, accuſe theſe philoſophers, of the inconſiderate opinion of their predeceſſors, who would have nature make all heavy things affect to lodge themſelves in the center of the earth, which being but a point, cannot contain any one of them; yet the hypotheſis of theſe is liable to other weighty objections.

For, firſt, it is not conceivable, how an inanimate body ſhould have an appetite to re-unite with homogeneous bodies, whoſe ſituation, and diſtance are unknown to it.

(2.) It does not appear that all bodies have ſuch an appetite, as is preſum'd, of joining themſelves to greater maſſes of connatural bodies. If you file the end of an ingot of ſilver or of gold, the filings will not ſtick to their own maſs, placed ever ſo near, or made to touch them, much leſs will they leap to it, when 'tis at a diſtance from them. The like may be ſaid of almoſt all the conſiſtent bodies we are acquainted with, except the load-ſtone and iron, and bodies that participate of one or the other of them.

(3.) 'Tis obvious, that what makes terreſtrial matter fall thro' the air to the earth, is ſome general agent, which, according to the wiſe diſpoſition of the author of the univerſe, determines the motion of thoſe bodies we call heavy, by the ſhorteſt ways permitted them, towards the central part of the terraqueous globe; whether the body, put in motion downwards, be of the ſame, a like, or a quite different nature, from the greater maſs of matter, to which, when 'tis aſſociated, it reſts. If, from the ſide of a ſhip, you let fall a chip of wood, when your arm is ſo ſtretch'd out, that the perpendicular, or ſhorteſt line, between that and the water, lies ever ſo little without the ſhip; that chip will fall into the ſea, which is a fluid body, and quite of another nature than itſelf, rather than ſwerve in the leaſt from its line of direction, to rejoin itſelf to the great bulk of wood, whereof the ſhip conſiſts. On the other ſide, if a man, ſtanding upon the ſhore juſt by the ſea, ſhall pour out a glaſs of water, holding the glaſs juſt over his feet; that water will fall into the ſand, where 'twill be immediately ſoaked up, and diſperſed, rather than deviate a little, to join itſelf to ſo great a maſs of connatural body, as is the ocean.

To the general belief, uſually made part of the preceding argument, that water does not gravitate in water, its own natural place; I reply, that water does gravitate in water, as well as out of it, and I elſewhere prove it.

2. Another argument, in favour of the receiv'd opinion of nature, may be drawn from the ſtrong appetite that bodies have to recover their uſual ſtate, when, by any means, they are put out of it, and thereby forc'd into a ſtate that is called preternatural; as air, violently compreſſed in a blown bladder, will, as ſoon as the force is remov'd, return to its firſt dimenſions: as the blade of a ſword bent, reſtores itſelf by its innate power, as ſoon as the force ceaſes; and as water, made hot by the fire, haſtens, when 'tis removed thence, to recover its former coldneſs.

But this argument, is grounded on the affections of inanimate bodies; and ſince an inanimate portion of matter is confeſſedly deſtitute of knowledge and ſenſe; it is uncapable of concern to be in one ſtate, or conſtitution, rather than another; for, it has no knowledge of that wherein it is at preſent, nor remembrance of that from which it was forc'd; and, conſequently, no appetite to forſake the former, and return to the latter. But every inanimate body, being, of itſelf, indifferent to all places and ſtates, continues in that place, or ſtate, to which the action and reſiſtance of other bodies, and eſpecially contiguous ones, effectually determine it.

And, before it be aſſerted, that water heated, returns, of itſelf, to its natural coldneſs; it were fit, that the aſſertors ſhould determine, what degree, or meaſure of coldneſs is natural to that liquor ; and this, if I miſtake not, will prove no eaſy taſk. And I think it yet more difficult to determine, what degree of coldneſs is natural to water, ſince this liquor perpetually varies its temperature, as to cold and heat, according to that of the contiguous, or neighbouring bodies, eſpecially the air. And therefore the water of an unſhaded pond, for inſtance, tho' it reſts in its proper and natural place, as they ſpeak ; yet, in autumn, if the weather be fair, the temper of it will much vary in the compaſs of the ſame day ; and the liquor be much hotter at noon, than early in the morning, or at midnight ; tho' this great diverſity be the effect only of a natural agent, the ſun, acting according to its regular courſe. And, in the depth of winter, 'tis generally confeſſed, that water is much colder than in the heat of ſummer ; which ſeems to be the reaſon of what is obſerv'd by watermen, that, on rivers, boats equally laden, will not ſink ſo deep in winter as in ſummer ; the cold condenſing the water, and, conſequently, making it ſpecifically heavier in the former ſeaſon than in the latter.

As to the motion of reſtitution, obſervable upon the removal, or ceaſing, of the force in air violently compreſſed, and in the blade of a ſword, forcibly bent ; I confeſs, it ſeems to me very difficult to aſſign the true mechanical cauſe of it : yet, I think it far more likely, that the cauſe ſhould be mechanical, than that the effect proceeds from ſuch a watchfulneſs of nature, as is pretended. For I queſtion whether we have any air here below, that is in other than a violent ſtate ; the lower parts of our atmoſphere being conſtantly compreſſed by the weight of the upper.

A long narrow plate of ſilver, that has not been hammer'd, compreſſed, or made red-hot in the fire, and ſuffer'd to cool leiſurely, may be bent any way, and will conſtantly retain the laſt curve figure, that you gave it. But if, having again ſtraiten'd this plate, you give it a few ſmart ſtrokes with a hammer, it will, by that mere mechanical change, become a ſpringy body. The like may be obſerv'd in copper, but not ſo remarkably, and ſcarce at all, in lead. From theſe phenomena, I demand why, if nature be ſo careful to reſtore bodies to their former ſtate, ſhe does not reſtore the ſilver-plate to its ſtraitneſs, when it is bent this way, or that way, before it be hammer'd? and why a few ſtrokes of a hammer ſhould entitle the plate to nature's peculiar care, and make her ſollicitous to reſtore it, when bent? And why, if the ſpringy plate be again heated and cooled of itſelf, nature abandons her former care, and ſuffers it quietly to continue in what crooked poſture one pleaſes to put it? And what is the reaſon of nature's greater partiality to ſilver, copper, and iron, than to lead and gold, with regard to the motion of reſtitution? However, even in ſword-blades, it has been often obſerved, that tho' if, ſoon after they are bent, the force that bent them be withdrawn, they will ſuddenly return to their former ſtraitneſs ; yet, if they be kept too long curved, they will loſe their elaſtic power, and continue in that crooked poſture, tho' the force that put them into it, ceaſes to act : ſo that, it ſeems, nature eaſily forgets the care ſhe was here preſumed at firſt to take.

PHYSICS.

3. It paſſes for current, and ſeems much to favour the opinion of the naturiſts, that what is violent, is contrary to nature, and therefore cannot laſt long. This trite ſentence is, by the ſchools, ſo particularly apply'd to local motion, that ſome of them have made it the characteriſtic, whereby to diſtinguiſh natural motions from others; that the former are perpetual, or at leaſt very durable, but the latter, being continually check'd by the contrariety of nature, continually decay, and are, within no long time, ſuppreſſed.

But it may be juſtly queſtioned, whether there is any motion among inanimate bodies, that deſerves to be called violent, in contradiſtinction to natural; ſince, among ſuch, all motions, where no intelligent agent intervenes, are made according to univerſal, and mechanical laws.

And methinks, the *Peripatetics*, who are moſt forward to employ this axiom, ſhould find but little reaſon for doing ſo, if they conſider'd how unſuitable it is to their doctrine, that the vaſt body of the firmament, and all the planetary orbs are, by the *Primum Mobile*, with a ſtupendous ſwiftneſs, whirl'd about from eaſt to weſt, in four and twenty hours, contrary to their natural tendency; and, that this violent and rapid motion, of the incomparably greater part of the univerſe, has laſted as long as the world itſelf, that is, according to *Ariſtole*, for innumerable ages.

We may alſo obſerve here below, that the ebbing and flowing of the ſea, which is generally ſuppos'd to proceed either from the motion of the moon, or that of the terreſtrial globe, or ſome other external cauſe, has laſted for ſome thouſands of years, and probably will laſt as long as the preſent ſyſtem of our vortex continues. That other great ocean too, the atmoſphere, conſiſts of numberleſs myriads of corpuſcles, that are here below continually kept in a violent ſtate; ſince they are elaſtical bodies, whereof the lower are ſtill compreſs'd by the weight of the upper. And, to make a ſpring of a body, it is requiſite that it be forcibly bent or ſtretch'd, and have ſuch a perpetual endeavour to fly open, or to ſhrink in, that it will not fail to do either, as ſoon as the force, that hinder'd it, is remov'd. And, as for the ſtates of inanimate bodies, I ſee not, that their being or not being natural, is, with any certainty, manifeſt from their being or not being very durable. For leaves that wither in a few months, and bloſſoms that often fade in a few days, are as well natural bodies, as the ſolid and durable trees that bear them. And 'tis obvious, that, whether we make the ſtate of fluidity, or of congelation, to be natural to water, and the other violent; its change from one of thoſe ſtates into the other, and even its return to its former ſtate, is often, at ſome ſeaſons, and in ſome places, made perhaps in an hour or leſs, by cauſes acknowledg'd to be natural. Miſts, hail, whirlwinds, lightning, falling-ſtars, *&c.* tho' natural bodies, are far from being laſting; eſpecially in compariſon of glaſs, wherein the ingredients, ſand and fix'd ſalt, are brought together by great violence of fire. And the motion that a thin plate of this glaſs can exerciſe, to reſtore itſelf to its former poſition, when forcibly bent, is in great part a laſting effect of the ſame violence of the fire: and ſo is the moſt durable perſeverance of the alkaline ſalt, one of the two ingredients of glaſs, tho' very eaſily ſoluble in water and other liquors, and even in the moiſt air itſelf.

4. There is a diſtinction of local motion, into natural and violent, ſo generally received and uſed, both by philoſophers and phyſicians, that it deſerves our notice here; ſince it implicitly contains an argument for the exiſtence of the thing call'd nature, by ſuppoſing it ſo manifeſt, that an important diſtinction may juſtly be grounded on it.

This objection, I confeſs, is difficult to clear; not for any force in it, but the ambiguity of the terms, wherein the diſtinction is employ'd: for moſt men ſpeak of this diſtinction or motion, in ſo obſcure, or ſo uncertain a way, that 'tis not eaſy to know what they mean by either member thereof. Yet ſome there are, who endeavour to ſpeak intelligibly, and define natural motion to be that, whoſe principle is within the moving body; and violent motion, that which bodies are put into by an external agent. But, even according to this explication, I am not ſatisfied with the diſtinction: for, as 'tis a principle received, and frequently employ'd, by *Ariſtotle* and his followers, *That whatever moves, is put in motion by ſomewhat elſe*; it ſeems, that, according to this axiom, all motion may be called violent, ſince it proceeds from an external agent. And indeed, according to the ſchool-philoſophers, the motion of far the greateſt part of the viſible world, tho' this motion be moſt regular and laſting, muſt hence be reputed violent; ſince they aſſert, that the immenſe firmament itſelf, with all the planetary orbs, is perpetually, and againſt its native tendency, hurry'd about the centre of the world, once in twenty-four hours, by an external, inviſible agent, which they therefore call the *Primum mobile.* And as for the criterion of natural motion, that *its principle is within the moving body*, it may be ſaid, that all bodies, once in the ſtate of actual motion, whatever cauſe firſt brought them to it, are moved by an internal principle: for inſtance, an arrow, that actually flies in the air towards a mark, moves by ſome principle or other reſiding within itſelf; for it does not depend on the bow 'twas ſhot out of, ſince 'twould continue, tho' that were broken, or annihilated; nor does it depend upon the medium, which more reſiſts than aſſiſts its progreſs. And if we ſhould ſuppoſe the air either to be annihilated, or render'd uncapable of furthering or hindring its progreſs, I ſee not why the motion of the arrow muſt neceſſarily ceaſe, ſince in this caſe there remains no medium to oppoſe its progreſs. When in a watch that is wound up, the ſpring endeavours to unbend itſelf; and when the ſtring of a drawn bow is broken or let go; the ſpring of the former, and the woody part of the latter, both return to a leſs curved line. And tho' theſe motions are occaſioned by the forcible acts of external agents, yet the watch, the ſpring, and the bow, have in themſelves, for ought appears to thoſe I reaſon with, an inward principle, by which they are mov'd, till they have attain'd their poſition. Some would add, that a ſquib, or a rocket, tho' an artificial body, ſeems, as well as a falling-ſtar, to move from an internal principle. On the other ſide, external agents are requiſite

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to many motions, acknowledg'd to be natural; as the heat of the ſun to the germination and flouriſhing of ſeveral plants, hung up in the air in the ſpring. And if in the air-pump you place ſeveral inſects, and withdraw the common air from the receiver, they will lie moveleſs, as if they were dead, tho' it be for ſeveral hours, whilſt they are kept from enjoying the air: but when that is permitted again to return upon them, they will preſently revive, and be brought to move again; as if a fly, for inſtance, reſembled a little wind-mill, in being moveleſs of itſelf, and requiring the action of the air to put its wings, and other parts into motion. But, ſince motion is not eſſential to matter, as diviſibility and impenetrability are believ'd to be; the motions of all bodies, at leaſt at the beginning of things, and of moſt, the cauſes of whoſe motions we can diſcern, were impreſſed on them; either by an external immaterial agent, God, or by external portions of matter acting on them.

And tho' motion be deſervedly made a principal part of *Ariſtotle*'s definition of nature; yet men uſually call ſuch motions natural, as are very hard to diſtinguiſh from thoſe they call violent. Thus, when water falls to the ground, they tell us, this motion is natural to that liquor, as 'tis a heavy body; but when a man ſpurts water into the air, they pronounce the motion, becauſe of its tendency upwards, to be contrary to nature: yet when he draws water into his mouth thro' a pipe held perpendicularly, they will have this motion, tho' directly upwards, to be not violent, but natural. So when a blown bladder, let fall upon a floor, rebounds, the deſcent and aſcent are both ſaid to be natural motions; tho' the former tends towards the centre of the earth, and the latter from it. And ſo, if, from a conſiderable height, you let fall a ſphere of ſome cloſe wood, not too heavy, into a deep veſſel of water, it will deſcend far in that liquor, by a natural motion; yet its contrary motion, upwards, ought not to be eſteem'd violent, ſince, according to the ſchools, being lighter *in ſpecie* than water, 'tis natural to it to affect its proper place; for which purpoſe, it muſt aſcend to the top of the liquor, and float. 'Tis, however, from theſe tendencies to oppoſite points, that men judge many motions of bodies to be natural, or violent.

And, indeed, ſince it muſt be indifferent to a lifeleſs and inſenſible body, to what place 'tis made to move, all its motions may, in one reſpect, be ſaid to be natural; and, in another, violent: for as very many bodies, of a viſible bulk, are ſet in motion by external impellents, upon which account, their motions may be ſaid to be violent; ſo the generality of impell'd bodies, move upwards, downwards, *&c.* toward any part of the world, in what line ſoever they find their motion leaſt reſiſted; which impulſe and tendency being given by virtue of what they call the general laws of nature, the motion may be ſaid to be natural.

The vulgar diſtinction of motion, therefore, into natural and violent, is not ſo clear and well-grounded, as to oblige us to admit that there is ſuch a Being as the naturiſts aſſert.

The vulgar notion of a criſis examin'd.

5. I come now to conſider the argument that may be drawn in favour of the receiv'd notion of nature, from the critical evacuations, which happen in diſeaſes; and the ſtrange ſtratagems that nature is ſaid to make uſe of, to free herſelf from the noxious humours that oppreſs her.

This argument I willingly acknowledge to be very conſiderable; for we really ſee, that in continual fevers, eſpecially in hot climates, there uſually happen, at certain times of the diſeaſe, notable commotions, or conflicts, after which the morbific matter is diſpoſed of, and diſcharged by ways ſtrange and ſurprizing; to the great and ſudden relief of the patient. Upon this account, I take the argument drawn from criſes to be much the ſtrongeſt that can be urged for the opinion from which I diſſent.

But the body of a man is an incomparable engine, which the moſt wiſe author of things has ſo skilfully framed, for laſting many years, that, if there were in it an intelligent principle of ſelf-preſervation, (as the naturiſts ſuppoſe there is) things would not, in moſt caſes, be better, or otherwiſe managed, for the preſervation of the animal, than they generally are. So that the queſtion is not, whether there be a great deal of providence and wiſdom exerciſed in the criſes of diſeaſes; but upon what account it is, that theſe appoſite things are perform'd? 'Tis the univerſal opinion of phyſicians, that 'tis the intelligent principle they call nature; which, being ſollicitous for the welfare of the patient, and diſtreſſed by the quantity or hurtfulneſs of the morbific matter, watches her opportunity to expel it haſtily out of the body, by the moſt ſafe and convenient ways, which, in the preſent condition of the patient, can be taken. I, on the other ſide, attribute criſes to the wiſdom and ordinary providence of God, exerting itſelf by the mechaniſm of that great machine, the world; and of that ſmaller engine, the human body, as conſtituted in the patient's circumſtances. The reaſons that hinder me from acquieſcing in the general opinion of phyſicians, about criſes, are principally theſe.

Criſes, properly ſo call'd, very ſeldom happen, except in fevers, and the like acute diſeaſes; where, according to the common courſe of things, the diſorder is ſoon terminated, either by recovery, death, or a change into ſome other diſeaſe: but chronical ſickneſſes, unleſs they happen to be accompany'd with feveriſh diſtempers, uſually have no criſes. This argues, that nature doth not make critical evacuations, upon account of ſuch care and watchfulneſs, as phyſicians aſcribe them to; ſince ſhe neglects to employ ſo wholeſome an expedient in diſeaſes, that are often no leſs dangerous and mortal than acute ones, whilſt ſhe attempts to cure by criſes.

Again, critical evacuations may be procured by the bare mechaniſm of the body; for, by virtue of that, the fibres, or motive organs of the ſtomach, *Viſcera*, and other parts, being diſtended, or vellicated, by the abundance, or acrimony, of the peccant matter, will, ſometimes, by ſuch an irritation, be made to contract themſelves vigorouſly, and to throw out the matter that offends the part, either by the emunctories of the body, or by whatever paſſages it can, with moſt eaſe, be diſcharged. Thus, when ſome men find their ſtomachs burden'd with aliment, they provoke it to unload itſelf with a feather, or the like, without being beholden to nature's watchfulneſs for a criſis, which, probably, ſhe would not attempt. And as 'tis uſual for criſes to be made in fevers by large hemorrhagies at the noſe, and other parts; which is aſcribed to nature's care for the patient's recovery; I muſt add, that it hath been often obſerved, that even, after death, large bleedings have happen'd at ſeveral other parts of the body; which ſhews, that ſuch excretions may be made by virtue of the ſtructure of the machine, or the turgeſcence and acrimony of the humours, without any deſign of nature to ſave the life of the patient.

Indeed, if it appear'd, by experience, that all criſes of diſeaſes either expell'd the morbific matter, or notably relieved the patient; the critical attempts of nature would much favour the opinion men have conceiv'd of her vigilance and conduct: but unwelcome inſtances daily ſhew, that, as ſome criſes are ſalutary, ſo others prove mortal. And, among thoſe that do not directly kill the patient, there are ſeveral which leave him in a worſe condition than he was before. I therefore wonder not, that phyſicians have thought themſelves obliged to lay down ſeveral circumſtances, as neceſſary requiſites of a laudable criſis; if any of which be wanting, 'tis not thought of the beſt kind: and if the contrary to ſome of them happen, 'tis to be judged either pernicious, or hurtful. For as there are two general ways ſuppoſed to be employ'd by nature, in making criſes, the one by expulſion of the peccant matter out of the body; and the other, by the ſettling of the matter ſomewhere within it; neither of theſe is conſtantly ſucceſsful.

Whence experience hath obliged phyſicians to divide criſes not only into perfect, that fully determine the event of the diſeaſe; and imperfect, that only alter it for the better or the worſe; but into ſalutary, that quite relieve the patient; and mortal, which deſtroy him. And, to a perfect and ſalutary criſis, ſome require no leſs than ſix conditions; that it be preceded by ſigns of coction; that it be made by a manifeſt and ſufficient excretion, or tranſlation; that it happen upon a critical day; that it leave no relicks behind, to endanger a relapſe; that it be without dangerous ſymptoms; and, laſtly, ſuitable to the nature of the diſeaſe, and the patient.

Hence it may appear no common thing to meet with a perfect and ſalutary criſis, ſince ſo many laudable conditions muſt concur in it. And, indeed, nature uſually takes up but with imperfect ones; and it were happy if ſhe made no better, provided ſhe made no worſe. But 'tis found, by ſad experience, that ſhe rouzes herſelf up to make a criſis, not only upon improper days, but alſo when there appear no ſigns of coction; and, by theſe unſeaſonable attempts, weakens the patient and increaſes the malady, or, perhaps, makes it ſpeedily mortal. Nor will it juſtify nature, to ſay, that theſe attempts are accidentally brought on, by the acrimony, or importunity of the morbific matter, by which ſhe is provoked, before the time, to endeavour an expulſion of it. For if nature be ſo prudent and watchful a guardian, as is thought, ſhe ſhould not ſuffer herſelf to be provoked to act prepoſterouſly, and make furious attempts, that laviſh the little ſtrength the patient hath ſo much need of. And, therefore, phyſicians often do very well, when, agreeably to the dictates of prudence, they forget how much wiſdom they are apt to aſcribe to nature, and employ their beſt skill and remedies to ſuppreſs, or moderate, the inordinate motions, or the improper and profuſe evacuations, that ſhe, when irritated, raſhly begins to make. And tho' the criſes that are made by a tranſlation of the peccant matter, or by lodging it in ſome particular part of the body, be often, when they are not ſalutary, ſomewhat leſs hurtful than thoſe made by excretion; yet theſe, frequently, prove dangerous; ſometimes producing inward impoſthumes, or external tumors, in parts either noble by their functions, ſituation, connexion, or ſympathy with others, that are not to be without hazard, or great inconvenience, oppreſs'd.

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I know that phyſicians make it a great argument of nature's providence and skill, that ſhe watches for the concoction of the peccant matter, before ſhe attempts to expel it by a criſis. But what is to be meant by this coction of humours, they do not clearly declare. As I underſtand it, when they ſay, that a portion of peccant matter is brought to coction, they mean, that it has acquir'd ſuch a diſpoſition, as makes it more fit than before, to be ſeparated from the ſounder portion of the maſs of blood, or from the conſiſtent parts to which it, perhaps, formerly adhered; and to be afterwards expell'd out of the body. As where the lungs are affected by a freſh taken cold, we ſee, that, after a few days, the phlegm is made more fluid, and that lodg'd in them, is eaſily brought up by the coughing, which could not diſlodge it before.

And, in fevers, that ſeparation in the urine, which phyſicians look upon as a good ſign of coction, ſeems to be produc'd by ſome part of the peccant matter, that, beginning to be ſeparated from the blood, mixes with the urine, and is not uſually diſtinguiſh'd from it, whilſt this liquor is warm; but when grown cold, on account of its weight, or texture, appears in a diſtinct form; as of a cloud, ſediment, *&c.*

But, whatever they mean by coction, 'tis plain that, on many occaſions, nature waits not for it, but unreaſonably, and often with danger, attempts to throw off the matter that offends her, before it be duly prepar'd for expulſion.

'Tis a circumſtance of criſes, thought the moſt wonderful, that nature often, by very unuſual ways, and at unexpected places, diſcharges the matter that offends her, and thereby either cures, or notably relieves the patient.

But, ſetting aſide extraordinary caſes, it ſeems probable, that the performances of nature, in common criſes, are, in ſome meaſure, referable to the particular condition of the matter to be expell'd, but principally to a peculiar diſpoſition in the primitive fabric of ſome parts of the patient's body; or ſome unuſual change made in the conſtruction of theſe parts by the diſeaſe itſelf, or

other

other accidents: which original, or adventitious diſpoſition being inviſible to us, at leaſt whilſt the patient lives, we are apt to aſcribe the unexpected accidents of a criſis, if it prove ſalutary, to the wonderful providence of nature; and if it happen otherwiſe, to overlook them. Plentiful evacuations, procured by medicines, are a kind of artificial criſes. Some bodies, we ſee, are ſo conſtituted, that tho' the peccant humour, wrought on by the medicine, ought, as the phyſician thinks, to be expell'd by ſtool, in the generality of thoſe who take rhubarb or ſena; yet the peculiar diſpoſition of the patient's ſtomach will make that an emetic, which was intended for a cathartic. Nor does this conſtitution of the ſtomach equally regard all purging medicines: for the ſame ſtomach that will reject them in the form of a potion, will quietly retain them, in that of pills. Thus mercury, which tho', when duly prepared, is uſually given to procure ſalivation, eſpecially to ſucculent bodies; yet there are ſome patients, wherein, inſtead of ſalivating, 'twill work violently downwards, like a purge, or make ſome other unexpected evacuation. And I have ſeen a patient, who, tho' young and very fat, could not be brought to ſalivate, either by the gentler ways; by turbith-mineral, or other harſher medicines, tho' adminiſtred by very ſkilful phyſicians and chirurgeons.

And this peculiarity may be as well contracted, as native: for ſome perſons, eſpecially after ſurfeits, having been ruffly dealt with, or, at leaſt, tired out with a medicine of this or that kind of form, will afterwards nauſeate and vomit up the like, tho' in other bodies it be very far from emetic.

We ſee, alſo, that ſometimes ſudorific medicines, inſtead of procuring ſweat, prove briskly diuretic, and ſometimes either purging or vomitive.

From all this we may argue, that the qualities of the irritating matter, and much more the particular diſpoſition of the patient's body, may procure evacuations at unexpected places.

There are alſo inſtances of periodical and critical evacuations, at very inconvenient, and unuſual vents. Thus ſome women are recorded to have had their menſes, ſometimes at the eyes, ſometimes at the navel, ſometimes at the mouth, *&c.* of which there ſeems no cauſe ſo probable, as ſome peculiar ſtructure, whether native or adventitious, of the internal parts concern'd in that diſcharge: and of ſuch unuſual ſtructures, anatomiſts muſt have ſeen many, ſince I myſelf have obſerv'd more than one or two. Now if theſe uncommon ways of diſpoſing of the morbific matter, were always ſalutary to the patient, the argument grounded on them would have more weight: but tho' moſt men take notice of this ſort of criſes, only when they are lucky, yet an impartial obſerver ſhall often find, that ill-condition'd and hurtful ones, may be made by unuſual and unexpected ways. And, in ſome tranſlations of the morbific matter to diſtant and nobler parts, perhaps it will be as difficult to ſhew, by what channels or known ways the matter paſs'd from one to another, as 'tis to determine, how it was conducted to the parts that moſt happily vented it.

'Tis my ſettled opinion, that divine prudence is, at leaſt, often concern'd, in a peculiar manner, about the actions of men, and the things that happen to them, or has a neceſſary connexion with the one, the other, or both. And tho' I think it probable, that in the conduct of that far greateſt part of the univerſe, which is merely corporeal, the wiſe author of it ſeldom manifeſtly procures a receſs from the ſettled courſe of the univerſe, and eſpecially from the moſt general laws of motion; yet, where men, who are creatures, that he is pleas'd to endow with free-will (at leaſt with regard to things not ſpiritual) are nearly and highly concern'd; he may, not only ſometimes by thoſe ſignal and manifeſt interpoſitions we call miracles, acted in a ſupernatural way; but, as the ſovereign lord and governor of the world, frequently give, by the intervention of rational minds, as well united, as not united to human bodies, ſeveral ſuch determinations to the motion of parts in thoſe bodies, and of others, which may be affected by them, as by laws merely mechanical, thoſe parts of matter would not have had: and by motions ſo determin'd, either ſalutary or fatal criſes, and many other things, conducive to the welfare or detriment of men, may be produced.

The interpoſition of divine providence, in caſes of life and death, might be eaſily ſhewn to chriſtians, from ſeveral paſſages of ſcripture. And, I think, it becomes a chriſtian philoſopher, to admit in general, that God ſometimes in a peculiar, tho' ſecret way, interpoſes in the ordinary phenomena and events of criſes; but that this is done ſo ſeldom, at leaſt in a way that we can certainly diſcern, that we are not haſtily to have recourſe to an extraordinary providence, and much leſs to the ſtrange care and skill of that queſtion'd Being call'd nature, in a particular caſe, if it may be probably accounted for by mechanical laws, and the ordinary courſe of things.

When we conſider the world, and the phyſical changes that happen in it, with regard to the divine wiſdom and providence; the arguments for the affirmative ought, in their kind, to have more force than thoſe for the negative. For it ſeems more allowable, to argue a providence from the exquiſite ſtructure and ſymmetry of the mundane bodies, and the apt ſubordination and train of cauſes, than to infer from ſome phyſical irregularities, that things are not fram'd and adminiſtred by a wiſe author and governor. For the characters and impreſſions of wiſdom, conſpicuous in the curious fabric, and orderly train of things, can, with no probability, be referr'd to blind chance, but to a moſt intelligent and deſigning agent. Whereas, on the other hand, the irregularities we ſpeak of, are incomparably fewer than thoſe things which are regular, and produc'd in an orderly way; and the divine maker of the univerſe being a moſt free agent, and having an intellect infinitely ſuperior to ours, may, in the production of ſeemingly irregular phenomena, have ends unknown to us, which even theſe irregularities may be very fit to compaſs.

SECT. III.

Axioms about nature, how far, and in what ſenſe to be admitted.

TO proceed, the moſt general and current axioms concerning nature, employ'd in the writings of philoſophers, may have a fair account given of them, agreeably to the doctrine hitherto propos'd; tho' ſome of theſe axioms ſuppoſe, and others ſeem ſtrongly to ſupport the receiv'd notion of nature.

But before I conſider the particular maxims receiv'd concerning nature's actions, let us ſee in what ſenſe nature may, or may not, be ſaid to act at all, or to do this or that.

For ought I can clearly diſcern, whatſoever is perform'd in the merely material world, is really done by particular bodies, acting according to the laws of motion, &c. ſettled and maintain'd by God, among things corporeal. In which hypotheſis, nature ſeems rather a notional thing, than a true phyſical, diſtinct efficient; ſuch as would be, in caſe *Ariſtotle*'s doctrine were true, one of thoſe intelligences he preſum'd to be the movers of the celeſtial orbs. But men often expreſs themſelves ſo very ambiguouſly, or intricately, when they ſay, that nature does this, or acts thus, that 'tis ſcarce poſſible intelligibly to tranſlate their expreſſions into any forms of ſpeech, adequate to the originals.

1. In this axiom, every nature preſerves itſelf; by the word nature, I ſuppoſe, is underſtood a natural body, otherwiſe I know not what it means. Thus, then, it may either ſignify, that no one body tends to its own deſtruction, or that, in every body, there is a principle call'd nature, upon account whereof the body is vigilant and induſtrious to preſerve its ſtate, and to defend itſelf from the violence and attempts of all other bodies that oppoſe, or endeavour to deſtroy or harm it.

In the former ſenſe, the axiom may be admitted, without any prejudice to our doctrine: according to which, inanimate bodies can have neither appetites, hatreds, nor deſigns; ſo that we may eaſily grant, they have none to deſtroy themſelves.

But, according to the other ſenſe of the axiom, 'twill import, that every body has within itſelf a principle, whereby it deſires, and with all its power endeavours to compaſs its own preſervation; and both to do thoſe things, that tend thereto, and oppoſe all endeavours, that outward agents, or internal diſtempers, may uſe to compaſs the deſtruction of it.

And, as this is the moſt vulgar ſenſe of the axiom, ſo 'tis chiefly in this, that I am concern'd to examine it.

I conceive, then, that the moſt wiſe creator of things at firſt ſo framed the world, and ſettled ſuch laws of motion between the bodies, which, as parts, compoſe it; that by the aſſiſtance of his general concourſe, the portions of the univerſe, are lodg'd in ſuch places, and furniſh'd with ſuch powers, that, by the help of his general providence, they may have their beings continued and maintained, as long as the courſe he thought fit to eſtabliſh amongſt things corporeal, requires.

Upon this ſuppoſition, which is but reaſonable, there will appear no neceſſity to have any recourſe, for the preſervation of particular bodies, to ſuch an internal appetite and in-bred knowledge in each of them, as the naturiſts preſume: ſince, by virtue of the original frame of things, and eſtabliſhed laws of motion, bodies are neceſſarily determined to act on ſuch occaſions, after the manner they would, if they had really an aim at ſelf-preſervation.

Thus, 'tis all one to a lump of dough, whether you make it into a round loaf, a long roll, or a flat cake: for in whatever figure the hands leave it, that it retains without having any appetite to return to the former. The mariner's needle, before it is excited, may have no particular propenſity to one part of the heavens more than another; but when it has been duly touch'd upon a load-ſtone, the flower-de-luce will be determin'd to the north, and the oppoſite extreme to the ſouth. So that, if the lily be drawn aſide, towards the eaſt or the weſt, as ſoon as the force that detain'd it, is remov'd, it will return to its former poſition, and never reſt, till it regard the north. But, in ſpite of this ſeeming affection of the lily to that point of the horizon, yet, if the needle be duly touch'd upon the contrary pole of a vigorous load-ſtone, the lily will preſently loſe its former inclination, and regard the ſouth part of the heavens.

2. Another received axiom concerning nature, is, that ſhe never fails of her end. This propoſition is very ambiguous. However, if by nature we here underſtand the Being, that the ſchool-men ſtyle *Natura naturans*; I allow that nature never miſſeth its end. For the omniſcient and almighty author of things having once framed the world, and eſtabliſh'd in it the laws of motion, which he conſtantly maintains, there can no irregularity happen, eſpecially among the greater mundane bodies, that he did not from the beginning foreſee and permit. And tho', on ſome ſpecial occaſions, this inſtituted order, either ſeemingly or really, has been violated; yet theſe things happening but rarely, and for weighty ends and purpoſes, by the peculiar intervention of the firſt cauſe, either guiding or over-ruling the propenſities and motions of ſecondary agents; it cannot be ſaid, that God is fruſtrated of his ends by theſe exorbitances, whereby he moſt wiſely and effectually accompliſhes them. But, if by nature be meant ſuch a ſubordinate principle, as men uſually underſtand by that name, I doubt the axiom is in many caſes falſe: for tho' the material world is ſo conſtituted, that, for the moſt part, things are brought to paſs by corporeal agents, as regularly, as if they deſigned the effects they produce; yet there are ſeveral inſtances wherein things happen quite otherwiſe.

Thus when a woman is pregnant, the aim of nature is, to produce a perfect human fœtus; yet we often ſee, nature widely miſſing her mark, inſtead of that, produces a monſter. Thus the ſap, that nature raiſes with intent to feed the fruit of a white-thorn, for inſtance, is by graffing brought to nouriſh a fruit of quite another kind. So, when the malſter makes barly to ſprout, whence nature intends to produce ſtalks and ears, 'tis perverted to a very different purpoſe.

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3. Another celebrated axiom concerning nature, is, that ſhe always acts by the ſhorteſt ways. But this rule, as well as ſeveral others, requires to be explained and limited, before it be admitted. 'Tis true, the omniſcient author of the univerſe has ſo framed it, that moſt of its parts act as regularly in order to the ends propoſed, as if they did it with deſign. But ſince inanimate bodies have no knowledge, it cannot reaſonably be ſuppoſed, that they moderate and vary their own actions, according to the exigency of particular circumſtances, wherewith they muſt of neceſſity be unacquainted; it were therefore ſtrange, if various occurrences did not determine them to act by other than the ſhorteſt ways, that lead to particular ends, if thoſe other ways be more agreeable to the general laws or cuſtoms eſtabliſhed among things corporeal. This I prove by inſtances taken from gravity itſelf, a quality perhaps very probably referr'd to an innate power and propenſion. For if a heavy body be let fall into the free air, 'twill take its courſe directly towards the centre of the earth; and, if it meet with an inclining plane, which puts it out of its way, it will not loſe its tendency towards the centre, but run along that plane, by which means its tendency downwards is proſecuted, tho' not, as before, in a perpendicular line, yet in the ſhorteſt way it is permitted to take. Theſe obvious phenomena, I confeſs, agree very well with the vulgar axiom, and poſſibly were the chief things that induced men to frame it. But now let us ſuppoſe, that a little ſphere of marble or ſteel, after having long fallen thro' the air, lights upon a pavement of hard ſtone, that lies horizontal; in this caſe, experience ſhews, that the falling globe will rebound to a conſiderable height, and falling down again, rebound again, and ſo for ſeveral times ſucceſſively, before it approaches as near as is permitted it, to the centre of heavy bodies. But if nature acted in all caſes by the ſhorteſt ways, this ſphere ought not to rebound at all. And having taken a good ſea compaſs, and ſuffer'd the magnetic needle to reſt north and ſouth; if I held the proper pole of a good load-ſtone at a convenient diſtance, on the right or left hand of the lily, this would be drawn aſide from the north point towards the eaſt or weſt, as I pleaſed; and then the load-ſtone being quite remov'd, the lily of the needle would indeed return northward, tho' not ſtop in the magnetic meridian, but paſſing on ſeveral degrees beyond it, 'twould thence return without ſtopping at the meridian line; and ſo, by its vibrations, deſcribe many arches ſtill ſhorter and ſhorter, till at length it came to ſettle on it, and recover that poſition, which, if nature always acted by the moſt compendious ways, it ſhould have reſted at the firſt time it had regain'd it. The truth is, that, at leaſt, inanimate bodies, acting without knowledge or deſign of their own, cannot ſtop or moderate their own actions, but muſt neceſſarily move as they are determined by the general laws of motion; according to which, in one caſe, the impetus, that a body acquires by falling, is more powerful to carry it on beyond the line of direction, than the action of the cauſes of gravity is to ſtop it, as ſoon as it comes to the neareſt place they can give it to the centre of the earth. And ſomething like this happens in levity, as well as

gravity; for if you take an oblong and conveniently ſhaped piece of light wood, and having ſunk it to the bottom of deep ſtagnant water, give it liberty to aſcend, it will not only regain the ſurface of the water, where, by the laws of gravity, it ought to reſt, and did reſt before it was forc'd down; but riſe far beyond that ſurface, and in part, as it were, ſhoot itſelf up into the incumbent air, then fall down again, and riſe a ſecond time, and perhaps much oftner, and fall again, before it ſettles in its due place, wherein it is in an equilibrium with the water, that endeavours to preſs it upwards.

4. Another maxim, generally receiv'd concerning nature, is, that ſhe always does what is beſt. But of this it will not be ſafe to deliver an opinion, till I have remov'd the ambiguity of the words; for they eaſily admit of two different ſenſes. They may ſignify, that nature, in the whole univerſe, does always that which is beſt for the preſervation of it in its preſent ſtate; or that, with regard to each body in particular, nature ſtill does what moſt conduces to the preſervation and welfare of that body. In the firſt of theſe ſenſes, the axiom will be leſs liable to exception; but then, I fear, it will be difficult to be poſitively made out by ſuch inſtances as prove that nature acts otherwiſe than neceſſarily according to mechanical laws. And therefore till I meet with ſuch proofs, I ſhall proceed to the other ſenſe, which, tho' the moſt uſual, I cannot admit, without it be both explain'd and limited. I readily grant, as I have often occaſion to repeat, that the all-wiſe author of things corporeal has ſo framed the world, that moſt things happen in it as if the particular bodies that compoſe it were watchful both for their own welfare and that of the univerſe. But I think, withal, that particular bodies, at leaſt ſuch as are inanimate, acting without either knowledge or deſign, their actions tend not to what is beſt for them in their private capacities, any further than ſuits with the general laws of motion, and the important cuſtoms eſtabliſh'd among things corporeal: ſo that, in conformity hereto, ſeveral things are done, that are neither the beſt, nor ſo much as good, with regard to the welfare of particular bodies.

We often ſee, that fruit-trees, eſpecially when they grow old, will, for one ſeaſon, be ſo overcharg'd with fruit, that they decay and die ſoon after; and even whilſt they flouriſh the exceſſive weight upon them ſometimes breaks off the branches, and thereby both hinders the maturity of the fruit, and haſtens the death of the tree. Now this fatal profuſion would have been prevented, if a wiſe nature, harbour'd in the plant, did, as is preſumed, ſollicitouſly watch for its welfare.

We ſee alſo, in ſeveral diſeaſes, and in the unſeaſonable and hurtful criſes of fevers, how far what men call nature often is from doing that which is beſt for the patient's preſervation. In many diſeaſes, a great part of the phyſician's work is to appeaſe the fury, and to correct the errors of this pretended nature, which being, as 'twere, tranſported with a blind and impetuous paſſion, unſeaſonably produces thoſe dangerous difdiſorders in the body, that, if ſhe were wiſe and watchful of its welfare, ſhe would have been as careful to prevent, as the phyſician to remedy.

And if nature be ſo provident and watchful for the good of men, and other animals, and of that part of the world wherein they live; how comes ſhe, from time to time, to deſtroy ſuch multitudes of men and beaſts, by earth-quakes, peſtilences, famine, *&c.*

5. The word *Vacuum* being ambiguous, and uſed in differing ſenſes, 'tis requiſite, before I declare my opinion about the receiv'd axiom of the ſchools, that nature abhors a *Vacuum*, to premiſe the chief acceptations, in which I have obſerv'd the term *Vacuum* to be uſed; for it has ſometimes a vulgar, and ſometimes a philoſophical, or ſtrict ſignification. In common ſpeech, to be empty, uſually denotes, not to be deſtitute of all body whatſoever; but of that body men ſuppoſe ſhould be in the thing ſpoken of, or of that which it was framed, or deſign'd to contain. Thus, we ſay, a purſe is empty, if there be no money in it; or a bladder empty, when the air is ſqueez'd out, *&c.* The word *Vacuum* is, alſo, taken in another ſenſe, by philoſophers who ſpeak ſtrictly, when they mean by it a ſpace within the world, wherein there is not contain'd any body whatſoever.

Now the chief, if not the only reaſon that moves the generality of philoſophers to believe, nature abhors a *Vacuum*, is, that, in ſome caſes, they obſerve an unuſual endeavour, and, perhaps, a forcible motion in water, and other bodies, to oppoſe a *Vacuum*. But I am not apt, without abſolute neceſſity, to aſcribe to inanimate, and ſenſeleſs bodies, the appetites and hatreds that belong to rational or ſenſitive Beings; and, therefore, think it a ſufficient reaſon to decline employing ſuch improper cauſes, if, without them, the motions, aſcribed thereto, can be accounted for.

If the *Carteſian* notion of the eſſence of a body, conſiſting in three dimenſions, be admitted, it can ſcarce be deny'd, that nature does not produce theſe great, and irregular efforts to hinder a *Vacuum*; ſince, it being impoſſible there ſhould be any, 'twere a fond thing to ſuppoſe, that nature, who is repreſented to us as a moſt wiſe agent, ſhould take pains, and do extravagant things, to prevent an impoſſible miſchief.

If the atomical hypotheſis be admitted, it muſt be granted, that nature is ſo far from abhorring a *Vacuum*, that a great part of the things ſhe does require it, ſince they are brought to paſs by local motion; and there are very many caſes, wherein, according to theſe philoſophers, the neceſſary motions of bodies cannot be perform'd, unleſs the corpuſcles, that lie in their way, have little empty ſpaces to retire, or be impell'd into; when the body, that impels them, endeavours to diſplace them. Whence the axiom, that nature abhors a *Vacuum*, agrees with neither of the two great ſects of the modern philoſophers.

But, for ought appears by the phenomena employ'd to demonſtrate nature's abhorrence of a *Vacuum*; either nature does not abhor a *Vacuum*, even when ſhe ſeems ſollicitous to hinder it; or, ſhe has but a very moderate hatred of it, in that ſenſe wherein the vulgar philoſophers take the word *Vacuum*.

For, in almoſt all viſible bodies here below, and even in the atmoſphere itſelf, there is more or leſs of gravity, or tendency towards the centre of our terraqueous globe; whence nature need not diſquiet herſelf, and act irregularly to hinder a *Vacuum*, ſince, without her abhorrence of it, it may be prevented or repleniſhed, by her affecting to place all heavy bodies as near the centre of the earth, as heavier than they will permit. And even without any deſign of her's a vacuity will be as much oppos'd, as we really find it, by the gravity of moſt, if not of all bodies here below, and the fluxility of liquors. For, by virtue of their gravity, and the minuteneſs of their parts, they will be determin'd to inſinuate themſelves into, and fill all the ſpaces that they find not already poſſeſſed by other bodies, either more ponderous, *in ſpecie*, than themſelves, or, by reaſon of their firmneſs of ſtructure, capable of reſiſting or hindering their deſcent. Accordingly, we obſerve, that, where there is no danger of a *Vacuum*, bodies may move, as they do, when they are ſaid to endeavour its prevention. Thus if you would thruſt your hand deep into a veſſel of ſand, and afterwards draw it out again, there will need nothing but the gravity of the ſand to make it fill up the greateſt part of the ſpace deſerted by the hand; and if the veſſel be repleniſhed, inſtead of ſand, with an aggregate of corpuſcles more minute and ſmooth than the grains of that, as, for inſtance, with quick-ſilver, or with water; then the ſpace deſerted by the hand will be, to ſenſe, compleatly fill'd by the corpuſcles of the fluid. And if you take a glaſs pipe, whoſe cavity is too narrow to let water and quick-ſilver paſs by one another in it, and having lodg'd a ſmall cylinder of mercury, of about half an inch long, in the lower part, you carefully ſtop the upper orifice with your finger, the quick-ſilver will remain ſuſpended in the pipe; and if, then, you thruſt the tube directly downwards, into a deep glaſs of water, till the quick-ſilver be depreſſed about a foot beneath the ſurface of the water, and then take off your finger from the orifice of the pipe; the quick-ſilver will immediately aſcend ſwiftly for five or ſix inches, and remain ſuſpended at this new ſtation. Here, therefore, we have a ſudden aſcent of ſo heavy a body as is quick-ſilver, and a ſuſpenſion of it in the glaſs, not produced to prevent or fill a *Vacuum*, for the pipe was open at both ends; the phenomena being but genuine conſequences of the laws of the equilibrium of liquors.

Conſidering how great a power the ſchool-philoſophers aſcribe to nature, I am the leſs inclined to think her abhorrence of a *Vacuum* ſo great as they imagin'd. I have ſhewn, that her averſion to, and watchfulneſs againſt it, are not ſo vehement, but that, in the ſenſe of the *Peripatetics*, ſhe can quietly admit it, in ſome caſes, where, with a very ſmall endeavour, ſhe might prevent or repleniſh it. When the *Torricellian* experiment is made, tho' it cannot, perhaps, be fully prov'd, either againſt the *Carteſians*, or ſome other *Pleniſts*, that in the upper part of the tube, deſerted by the quick-ſilver, there is a *Vacuum*, in the ſtrict philoſophical ſenſe of the word; yet, as the *Peripatetics* declare their ſenſe, by their reaſonings

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againſt a *Vacuum*, 'twill appear very hard for them to ſhew, there is not one in that tube. And, as by the ſcholaſtic way of arguing, nature's hatred of a *Vacuum*, from the ſuſpenſion of water, and other liquors, in tubes and conical watering-pots, it appears, they thought that any ſpace here below, deſerted by a viſible body, not ſucceeded by another viſible body, or at leaſt by common air, may be reputed empty; ſo, the ſpace deſerted by the quick-ſilver, at the top of a barometer, thirty-one inches long, will invite one to doubt, whether a *Vacuum* ought to be thought ſo formidable a thing to nature, as they imagine ſhe thinks it? For, what miſchief would enſue to the univerſe, upon the production or continuance of ſuch a *Vacuum*, tho' the deſerted ſpace were much more than an inch, and continued many years, as has ſeveral times happen'd in the taller ſort of mercurial barometers? And thoſe *Peripatetics* who tell us, if there were a *Vacuum*, the influences of the celeſtial bodies, that are abſolutely neceſſary to the preſervation of the ſublunary ones, would be intercepted, ſince motion cannot be made *in vacuo*, would do well to prove ſuch a neceſſity; and to conſider, that in our caſe the top of the quick-ſilver, to which the *Vacuum* reaches, uſually appears protuberant; which ſhews, that the beams of light are able to paſs thro' that *Vacuum*, being, in ſpite of it, reflected from the *Mercury* to the eye. And in ſuch a *Vacuum*, as to common air, I have try'd, that a load-ſtone will emit its effluvia, and move iron or ſteel placed therein.

In ſhort, it is not evident, that, here below, nature ſo much endeavours to hinder or fill up a *Vacuum*, as to manifeſt an abhorrence of it: and, without much peculiar ſolicitude, a *Vacuum*, at leaſt a philoſophical one, is as much provided againſt, as the welfare of the univerſe requires, by gravity and the fluxility of the liquors, and other bodies.

6. I come now to the celebrated ſaying, that nature cures diſeaſes, taken from *Hippocrates*, who expreſſes it in the plural, νουσῶν φυσιὲς ἰητροὶ. And becauſe this axiom is generally receiv'd among phyſicians and philoſophers, and ſeems to be one of the principal things, that has made them introduce ſuch a Being as they call nature; I ſhall attentively conſider, in what ſenſe, and how far, this famous axiom may, or ſhould not, be admitted.

Firſt, then, I conceive it may be taken in a negative ſenſe, ſo as to import, that diſeaſes cannot be cured in ſuch perſons, in whom the aggregate of the vital powers, or faculties of the body, is ſo far weaken'd or depraved, as to be utterly unable to perform the functions neceſſary to life; or at leaſt to actuate and aſſiſt the remedies employ'd by the phyſician, to preſerve or recover the patient.

This I take to be the meaning of ſuch uſual phraſes as theſe, *Phyſic comes too late. Nature is quite ſpent*, &c. And in this ſenſe I readily acknowledge the axiom to be true. For, where the engine has ſome neceſſary parts, whether fluid or ſolid, ſo far deprav'd or weakned, as to render it altogether unable to co-operate with the medicine, it cannot be rationally expected, that the adminiſtration of that medicine, ſhould prove effectual. But there is a poſitive ſenſe of our axiom, wherein it is the moſt uſually employ'd; for men commonly believe there reſides in the body of a ſick perſon, a certain provident or watchful Being, that induſtriouſly employs itſelf, by its own endeavours, as well as by any occaſional aſſiſtance, to rectify whatever is amiſs, and reſtore the diſtemper'd body to its priſtine ſtate of health.

Now, I conceive that the wiſe and beneficent maker of the world, intending that men ſhould, for the moſt part, live a conſiderable number of years, in a condition to act the parts aſſign'd them; he was pleas'd to frame human bodies ſo, that, with the ordinary ſuccours of reaſon, *&c.* they may in many caſes recover a ſtate of health, if they chance to be put out of it by leſs accidents than thoſe, that God, in compliance with the great ends of his general providence, did not think fit to ſecure them from, or enable them to ſurmount. Many things, therefore, that are commonly aſcrib'd to nature, may be better aſcribed to the mechaniſm of the univerſe, and the human body.

The cauſes that diſorder the human frame, are often tranſient, but the ſtructure of the body itſelf, and the cauſes that conduce to the preſervation of its ſtructure, are more ſtable and durable; and, on that account, may enable the engine to out-laſt many things that are unfriendly to it, as ſlight hurts, and diſeaſes.

Several phenomena of diſeaſes may be illuſtrated, by ſuppoſing dirt thrown into a vial of fair water, and then the vial to be well ſhaken; whereby the water will loſe its tranſparency, upon a double account; that of the dirt, whoſe opake particles are confounded with it; and that of the bubbles, which ſwim at the top of it: yet to purify this water, and make it recover its former tranſparency, there needs no particular care or deſign of nature; but, according to the common courſe of things, after ſome time the bubbles will break and vaniſh at the top, and the earthy particles, that compoſe the mud, will, by their gravity, ſubſide to the bottom, and ſettle there, whence the water becomes clear again.

There is indeed one thing, to which the maxim, *Nature cures diſeaſes*, may be very ſpeciouſly applied; and that is the healing wounds; which, if but in the fleſh, may often be cured without medicines. However, this healing ſeems to be only an effect or conſequent of that fabric of the body, on which nutrition depends. For the alimental juice being, by the circulation of the blood and chyle, carried to all parts of the body; if it meets any where, either with preternatural concretions, or with a gap made by a cut or wound, its particles there concrete into a kind of fleſh, or other body, which that juice, in the place and other circumſtances 'tis in, is fitted to conſtitute. Thus not only wens and ſcrophulous tumors are nouriſh'd in the body, but miſ-ſhapen mola's grow in the womb, as well as embryo's. And we ſee, in wounds, that fungus's are as well produced and nouriſh'd by the aliment brought to the wounded part, as the true and genuine fleſh; ſo that either nature ſeems much miſtaken, if ſhe deſigns the production and maintenance of ſuch ſuperfluous and inconvenient bodies;

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bodies; or the chirurgeon is much to blame, who induſtriouſly deſtroys them. But, for ought appears, nature is not ſo ſhy and reſerv'd in her bounty, but that ſhe ſends nouriſhment, to repair as well things that do not genuinely belong to the body, as to reſtore fleſh to wounded parts. This appears by warts and corns, that grow again after they are cut. And I have ſeen a woman, in whoſe forehead nature was careful to nouriſh a horn, above an inch in length; which I fully examin'd, whilſt it was yet growing upon her head, to avoid being impoſed upon.

But there are many diſeaſes, as well acute as chronical, wherein, 'tis confeſs'd, that nature alone does not work the cure; ſo that the aphoriſm, which makes nature the curer of theſe, is not true, but in a limited ſenſe. 'Tis, indeed, pretended, that even here nature is the principal agent, by whoſe direction the phyſician acts in ſubſervience to her deſigns; and phyſicians themſelves acknowledge, that they are but nature's miniſters. Let us, therefore, ſee in what ſenſe it is fit, according to our doctrine, to admit theſe aſſertions.

One great cauſe of the common miſtakes about this matter, is, that the body of a man hath been look'd upon rather as a ſyſtem of parts, whereof moſt are groſs and conſiſtent, and not a few hard and ſolid too, than as, what indeed it is, a very compounded engine; that, beſides theſe parts, conſiſt of the blood, chyle, gall, and other liquors; and of more ſubtile fluids, as ſpirits and air: all which are inceſſantly and variouſly moved, and thereby put ſeveral of the ſolid parts into frequent and differing motions. When, therefore, the conſtitution, or the motions, that in a ſound body regularly belong to the fluids, happen, the former to be deprav'd, or the latter to grow irregular; the engine is immediately out of order, tho' the groſs ſolid parts were not primarily affected: ſo, when by proper remedies the vitiated texture of the blood, or other juices, is corrected, and the inordinate motions of them and the ſpirits are calm'd and rectify'd, the groſſer and more ſolid parts of the body, and ſo the whole animal œconomy, will be reſtored to a more convenient ſtate.

The phyſician, in my opinion, is to look upon his patient's body as an engine out of order, yet ſo conſtituted, that by concurring with the endeavours or tendencies of the parts of the machine, it may be brought to a better ſtate. If, therefore, he find, that, in the preſent diſpoſition of the body, there is a propenſity to throw off the matter that offends it, in a convenient way, and at commodious places; he will then act ſo as to comply with and further that way of diſcharge, rather than another. As if there be a great appearance that a diſeaſe will quickly have a criſis by ſweat, he will rather further it by covering the patient with warm clothes, and giving ſudorific medicines; than, by endeavouring to carry off the peccant matter by purging or vomiting, unſeaſonably hinder a diſcharge, that probably will be beneficial. And in this ſenſe men may ſay, if they pleaſe, that the phyſicians are miniſters or ſervants of nature, as ſea-men, when the ſhip ſails before a good wind, will not ſhift their ſails, nor alter the ſhip's motion, becauſe they need not. But to ſhew, that 'tis, as 'twere, by accident, that the phyſician, in this caſe, obeys nature, (to ſpeak in the language of the naturiſts;) there are many other caſes, wherein the phyſician, if ſkilful, will be ſo far from taking nature for his guide, to direct him by her example; that a great part of his care and skill is employ'd, to hinder her from doing what ſhe ſeems to deſign; and to bring to paſs other things very differing from, if not contrary to, what ſhe endeavours.

Thus, tho' nature, as 'tis call'd, importunately craves for drink in dropſies, the phyſician thinks himſelf obliged to deny it; as he does what his patients greedily deſire in the *Pica*, *&c.* Thus alſo the chirurgeon often hinders nature from cloſing up the lips of a wound, as ſhe would unskilfully do, before it be well and ſecurely heal'd at the bottom. So the phyſician, by purging or phlebotomy, carries off the matter, that nature would more dangerouſly throw upon the lungs.

In ſhort, I look upon a good phyſician, not properly as a ſervant to nature, but a counſellor and a friendly aſſiſtant; who, in his patient's body, furthers theſe motions, and other things, that he judges conducive to the welfare and recovery of it: but as to thoſe, that he perceives likely to be hurtful, either by increaſing the diſeaſe, or otherwiſe endangering the patient, he thinks it his part to oppoſe or hinder, tho' nature manifeſtly ſeems to endeavour to exerciſe or carry on thoſe hurtful motions.

Hence, I fear, the commendations which *Hippocrates* gives of nature, in the cure of diſeaſes, make many phyſicians leſs courageous and careful than they ſhould be, to employ their own skill, on ſeveral occaſions that greatly require it.

As, in ſome caſes, the phyſician relieves his patient in a negative way, by oppoſing nature in her unſeaſonable or diſorderly attempts; ſo, in others, he may do it in a poſitive one, by employing medicines that either ſtrengthen the parts, or make ſenſible evacuations of matter, neceſſary to be proſcribed by them; or, by uſing remedies, that by their manifeſt qualities oppoſe thoſe of the morbific cauſe. And, perhaps, in ſome caſes, the phyſician may, in a poſitive way, contribute more to the cure even of an inward diſeaſe, than nature herſelf ſeems able to do: for, if any ſuch medicine may be prepared by art, as *Helmont* affirms to be attainable from *Paracelſus*'s *Ludus*, by the alkaheſt; or, as *Cardan* relates, that an empiric had in his time, who travell'd up and down *Italy*, curing ſuch where-ever he came, as were tormented with the ſtone of the bladder; the phyſician might, by ſuch inſtruments, perform that which, for ought appears, is not to be done by nature herſelf; ſince we never find that ſhe diſſolves a confirm'd ſtone in the bladder. Nay, ſometimes the phyſician does, even without the help of a medicine, control and over-rule this nature, to the great and ſudden advantage of the patient, as in the caſe of ſwooning.

It appears from the whole, that as there are ſome phenomena, which ſeem to favour the doctrine of the naturiſts, about the cure of diſeaſes; ſo there are others, that more manifeſtly agree with our hypotheſis. And both

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theſe ſorts of phenomena being conſider'd together, may well ſuggeſt a ſuſpicion that the moſt wiſe and free author of things having framed the firſt individuals of mankind ſo as to be fit to laſt for many years, and endow'd thoſe originals with the power of propagating their ſpecies; it thence comes to paſs, that in the ſubſequent engines we call human bodies, when neither particular providence, nor the rational ſoul, nor over-ruling impediments interpoſe, things are generally perform'd according to mechanical laws and courſes; whether the effects and events of theſe prove conducive to the welfare of the engine itſelf, or elſe cheriſh and foment extraneous bodies or cauſes, whoſe preſervation and increaſe are hurtful to it. Hence the happy things referr'd to nature's prudent care for the recovery and welfare of ſick perſons, are uſually genuine conſequences of the mechaniſm of the world, and the patient's body; which effects luckily happen to be co-incident with his recovery, rather than to have been purpoſely and wiſely produced in order to it; ſince what is called nature, ſeems careful to produce, preſerve, and cheriſh things hurtful to the body, as well as thoſe beneficial to it.

Thus not only worms, but frogs and toads, taken in their ſpawn, with corrupted water, have been cheriſh'd in the ſtomach, till the eggs being grown compleat animals, have produced horrid ſymptoms in the body. And if, according to the receiv'd opinions of phyſicians, ſtubborn quartans are produced by a melancholy humour ſeated in the ſpleen; it may be ſaid that nature ſeems to buſy herſelf to convert ſome parts of the fluid chyle into ſo tenacious a juice, that, in many patients, notwithſtanding the neighbourhood of the ſpleen and ſtomach, neither ſtrong emetics, purges, nor other uſual remedies, are able, in a long time, to diſlodge, reſolve, or correct it.

But the poiſon of a mad dog nature ſometimes ſeems induſtriouſly to preſerve; for we have inſtances of a little foam convey'd into the blood by a ſlight hurt, which, notwithſtanding the conſtant heat and perſpirable make of the human body, and the diſſipable texture of the foam, ſo preſerved, and ſometimes too for many years, that at the end of that time it has broke out, and diſplay'd its fatal efficacy with as much vigor and fury as if it had but newly been receiv'd into the body.

Thus tho' the quantity of poiſon in the bite of the *Tarantula* is ſcarce viſible, being communicated by the tooth of ſo ſmall an animal as a ſpider; yet, in many patients, 'tis preſerv'd during a great part of their lives, and manifeſts its continuance in the body by annual paroxyſms. And a perſon of great quality complain'd to me, that being in the *Eaſt*, the biting or ſtinging of a creature, whoſe offenſive arms were ſo ſmall, that the eye could very hardly diſcern the hurt, had ſo laſting an effect upon him, that for about twelve years after he was reminded of his miſchance, by a pain he felt in the hurt place, about the ſame time of the year that the miſchief was firſt done him. And in ſome hereditary diſeaſes, as the gout, the falling-ſickneſs, *&c.* nature ſeems to act as if ſhe did, with care and skill, tranſmit to the unhappy child ſuch morbific ſeeds, or impreſſions of the parents, that, in ſpite of all the various alterations the younger body paſſes thro' during the courſe of many years, this protected enemy is able to exert its power and malice, after forty or fifty years concealment.

In the ſame manner, but with light variations, other axioms about nature might be eaſily explain'd.

But tho' we could not intelligibly explain all the particular axioms about her, and the phenomena of inanimate bodies, that are thought to favour them by mechanical principles, it would not follow that we muſt therefore yield up the whole cauſe to the naturiſts. For we have already ſhewn, that the ſuppoſition of ſuch a Being as they call nature is far from enabling her patrons to give intelligible accounts of theſe and other phenomena of the univerſe. And tho' our doctrine ſhould be inſufficient to give a good account of things corporeal; yet a leſs degree of probability may ſerve in arguments employ'd but to juſtify a doubt, than is requir'd in thoſe that are to demonſtrate an aſſertion.

'Tis true, the naturiſts tell us, that the nature they aſſert is the principle of all motions and operations in bodies; which infers, that in explaining them we muſt have recourſe to her.

But before we acquieſce in, or confidently employ this principle, it were very fit we knew what it is: I therefore demand of thoſe who aſſert ſuch a nature as is vulgarly deſcribed, whether it be a ſubſtance, or an accident? If it be the latter, it ſhould be declar'd what kind of accident it is; how a ſolitary accident can have right to all thoſe attributes, and produce thoſe numerous and wonderful effects that they aſcribe to nature; and why a complex of ſuch accidents as are the mechanical affections of matter, may not altogether as probably as that accident they call nature, be conceiv'd to have been inſtituted by the perfectly wiſe author of the univerſe, to produce thoſe changes among bodies which are intelligibly referable to them. And if things be not brought to paſs by their intervention, 'twere very fit we ſhould be inform'd by what other particular and intelligible means nature can effect them better.

But if it be ſaid, as by moſt it is, that the principle call'd nature is a ſubſtance; I demand whether it be corporeal or immaterial? If it be immaterial, I further ask, whether it be created, or not? If not, we have God under another name, and our diſpute is at an end, by the removal of its ſubject, which is ſaid by the ſchools to be God's vicegerent, not God himſelf. But if nature be affirm'd (as ſhe is by all chriſtian philoſophers) to be a created Being; I then demand whether ſhe be endow'd with underſtanding, ſo as to know what ſhe does, for what ends, and by what laws ſhe ought to act? If the anſwer be negative, the ſuppoſition of nature will be of very little uſe to afford an intelligible account of things. And if it ſhould be ſaid that nature is endowed with underſtanding, and performs ſuch functions as many of the ancients aſcribe to the ſoul of the world; this hypotheſis is near of kin to heatheniſm; and I do not think that they who ſuppoſe a kind of ſoul of the univerſe, will

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find this principle ſufficient to explain the phenomena of it. For though nature be admitted to have reaſon, yet a multitude of phenomena may be mechanically produced, without her immediate intervention; as, we ſee, that in man, though the rational ſoul has ſo narrow a province to take care of, as the human body, and is ſuppoſed to be intimately united to all the parts of it; yet abundance of things are done in the body, by the mechaniſm of it, without being produced by the ſoul. In ſleep, the circulation of the blood, the regular pulſation of the heart, digeſtion, nutrition, reſpiration, *&c.* are perform'd without the immediate agency, or ſo much as the actual knowledge of the mind. And, when a man is awake, many things are done in his body, not only without the direction, but againſt the bent of his mind; as often happens in cramps, and other convulſions, coughing, yawning, *&c.* Nay, tho' ſome brutes, as, particularly, apes, have the ſtructure of many parts of their bodies very like that of the correſpondent ones in men; yet that admirable work of the formation and organization of the fœtus in them, is granted, by philoſophers, to be made by the ſoul of the brute, which is neither an incorporeal, nor a rational ſubſtance. And even the human fœtus, if we admit the general opinion of philoſophers, phyſicians, divines, and lawyers, is form'd without the intervention of the rational ſoul, that is not infuſed, till this hath obtain'd an organization, fitting to receive ſuch a gueſt; which it is reputed to do, about the end of the ſixth week, or before that of the ſeventh.

If it be urged, that nature, being the principle of motion in bodies; their various motions, which amount to a conſiderable part of their phenomena, muſt be explained, by having recourſe to her: I anſwer, 'tis very difficult to conceive how a created, immaterial ſubſtance, can, by a phyſical power, move a body; the agent having no impenetrable part, wherewith to impel the corporeal *Mobile.* God, indeed, who is an immaterial ſpirit, ought to be acknowledg'd the primary cauſe of motion in matter; becauſe motion neceſſarily belongs not to corporeal ſubſtances. But, then, there is that infinite diſtance between the incomprehenſible creator, and the leaſt imperfect order of his creatures, that we ought to be very cautious how we make parallels between them; and draw inferences from his power, and manner of acting, to theirs: ſince he, for inſtance, can immediately act upon human ſouls, as having created them; whilſt they are not able ſo to act upon one another. And it ſeems the more difficult to conceive, that, if nature be an incorporeal ſubſtance, ſhe ſhould be the great mover of the mundane matter; becauſe we ſee, that, in a human body, the rational ſoul, tho' vitally united to it, can only determine the motion of ſome of the parts; not give motion to any, or ſo much as regulate it in moſt. And, if nature be ſaid to move bodies, in another than a phyſical way; I doubt, whether the ſuppoſition of ſuch a principle will be of much uſe to philoſophers, in explaining phenomena. I ſhould ſcarce think him a judicious phyſician, who imagines, that he gives an intelligible and particular account of the ſurprizing ſymptoms of thoſe ſtrange diſeaſes that many impute to witchcraft, when he ſays that their phenomena are produced by a wicked immaterial ſpirit, call'd a devil.

But 'tis thought that the greater number of philoſophers, at leaſt among the moderns, take nature to be corporeal: I therefore demand whether theſe philoſophers believe nature, tho' corporeal, to act knowingly, that is, with conſciouſneſs of what ſhe does, and for deſigned ends; or elſe to be blindly and neceſſarily moved and directed by a ſuperior agent, endow'd with an excellent underſtanding?

The *Carteſians* would ask, how, if nature be a corporeal ſubſtance, we can conceive her capable of thinking, and of being a moſt wiſe and provident director of all the motions that are made in the corporeal world?

And a philoſopher may juſtly demand, how a corporeal Being can ſo pervade the univerſe, as to be intimately preſent with all its minute parts, whereof 'tis ſaid to be the principle of motion?

We may alſo demand whence nature, being a material ſubſtance, comes itſelf to have motion, ſince motion does not belong to matter in itſelf? For a body is as truly a body when it reſts, as when it moves. If it be anſwer'd, that God at firſt put it into motion; I reply, that the ſame cauſe may as probably be ſuppos'd to have put the unqueſtion'd mundane matter into motion, without the intervention of another corporeal Being, in the conception whereof as matter, motion is not involv'd.

It may likewiſe be ask'd, how the laws of motion come to be obſerv'd, or maintain'd, by a corporeal Being, which, as merely ſuch, is either uncapable of underſtanding them, or of acting with reſpect to them; or, at leaſt, is not neceſſarily endow'd with any knowledge of them, or power to conform to them, and to make all the parts of the unqueſtion'd mundane matter do ſo too?

And I ſee not how the taking in ſuch an unintelligible and undeſigning principle, will free our underſtandings from great difficulties, when we come to explain the phenomena of bodies. For if nature be a bodily creature, and acts neceſſarily; I ſee no cauſe to look upon it as other than a kind of engine: and the difficulty may be as great to conceive how all the ſeveral parts of this ſuppos'd engine, call'd nature, are themſelves framed and moved by the great author of things, and how they act upon one another, as well as upon the undoubted mundane bodies; as 'tis to conceive how in the world itſelf, which is manifeſtly an admirably contriv'd automaton, the phenomena may by the ſame author be produced, in conſequence of the primitive conſtruction and motions that he gave it, without the concurrence of ſuch a thing as they call nature. For this, as well as the world, being a corporeal creature; we cannot conceive that either of them act otherwiſe than mechanically. And it ſeems very ſuitable to the divine wiſdom to employ in the world, already framed and completed, the feweſt, and moſt ſimple means, by which the phenomena, deſign'd to be exhibited, could be produced. Nor need we be much mov'd to hear ſome naturiſts ſay, that nature, tho' not an incorporeal Being, is of an order ſuperior to mere matter. For who can clearly conceive an order, or kind of Beings, that ſhall be real ſubſtances, and yet neither corporeal nor immaterial? Nor do I ſee how the ſuppoſition of this unintelligible, at leaſt unintelligent Being, tho' we ſhould grant it to have a kind of life or ſoul, will much aſſiſt us to ſolve phenomena.

To draw, at length, towards a concluſion; what we have here deliver'd upon this ſubject, may, perhaps, do ſome ſervice both to natural philoſophy, and to religion.

The uſe and advantages of this inquiry.

We have attempted to diſſuade philoſophers from often employing, and without great need, a term, which, by reaſon of its great ambiguity, and the little care taken by ſuch as uſe it, to diſtinguiſh its different acceptions, occaſions both a great deal of darkneſs and confuſion in what men ſay and write about things corporeal; and a multitude of controverſies, wherein really they wrangle about words. And this diſcourſe may, poſſibly, wean many from the fond conceit they cheriſh, that they underſtand or explain a corporeal ſubject, or a phenomenon, when they aſcribe it to nature. For to do that one need not be a philoſopher, ſince a peaſant may eaſily do the ſame.

A man has never well perform'd the part of a true philoſopher, till he circumſtantially or particularly deduces the phenomenon he conſiders by intelligible ways, from intelligible principles; which he will be conſtantly put in mind of doing, or diſcover that he hath not done, if by forbearing general and ambiguous terms he endeavours to explain things by expreſſions that are clear to all attentive readers. And this perſpicuous way of philoſophizing ſhould be greatly recommended, by the valuable diſcoveries which thoſe who employ'd it have happily made, in hydroſtatics, optics, anatomy, botanics, and many other parts of real learning.

Our doctrine may alſo be ſerviceable to religion in three reſpects.

And firſt, it may keep many, who were inclined to have an exceſſive veneration for what they call nature, from running into thoſe extravagant and ſacrilegious errors, that have been, upon plauſible pretences, embraced, not only by many of the old heathen philoſophers, but by ſeveral modern profeſſors of chriſtianity.

Secondly, it may conduce to juſtify ſome remarkable proceedings of divine providence, againſt thoſe who boldly cenſure it, upon the account of ſome things they judge to be phyſical irregularities; ſuch as monſters, earth-quakes, floods, eruptions of vulcano's, famines, *&c.* For, according to us,

1. God is a moſt free agent, and created the world, not out of neceſſity, but voluntarily, having framed it as he pleaſed and thought fit at the beginning of things, when there was no ſubſtance but himſelf, and conſequently no creature to which he could be obliged, or by which he could be limited.

2. God having an underſtanding infinitely ſuperior to that of man, in extent, clearneſs, and other excellencies; he may rationally be ſuppos'd to have framed ſo great and admirable an automaton as the world, and

the ſubordinate engines comprized in it for ſeveral ends and purpoſes, ſome of them relating chiefly to his corporeal, and others to his rational creatures; of which ends he hath vouchſafed to make ſome diſcoverable by our dim reaſon, whilſt others are probably not to be penetrated by it, but lie conceal'd in the deep abyſs of his unſearchable wiſdom.

3. It ſeems probable that this moſt excellent and glorious Being thought fit to order things ſo, that both his works and actions might bear ſome marks of his attributes, and eſpecially to ſtamp upon his corporeal works ſome tokens or impreſſes of his divine wiſdom, diſcernible by the human mind.

4. Upon this ſuppoſition it became the divine author of the univerſe to give it ſuch a ſtructure, and ſuch powers, and to eſtabliſh among its parts ſuch general and conſtant laws as beſt ſuited his purpoſes in creating the world, and to give theſe univerſal laws, and particular parts, or bodies, ſuch ſubordinations to one another, and ſuch references to the original fabric of the grand ſyſtem of the world, that, on all particular occaſions, the welfare of inferior or private portions of it ſhould be only ſo far provided for, as their welfare is conſiſtent with the general laws by him ſettled in the univerſe; and with ſuch of thoſe ends that he propos'd to himſelf in framing it, as are more conſiderable than the welfare of thoſe particular creatures.

5. The laſt ſervice that, I hope, our doctrine may do religion, is, to induce men to pay their admiration, their praiſes, and their thanks, directly to God himſelf; who is the true and only creator of the ſun, moon, earth, and thoſe other creatures that men call the works of nature. And in this way of expreſſing their veneration of the true God, and their gratitude to him, they are warranted by the examples of the ancient people of God, the *Iſraelites*; and not only by the inſpired perſons of the old teſtament, but by the promulgators of the new, and even by the celeſtial ſpirits, who in the Revelation of St. *John* are introduced praiſing and thanking God himſelf for his works, without taking any notice of his pretended vicegerent, nature.

AN
INQUIRY
INTO THE
FINAL CAUSES
OF
Natural Things.

SECT. I.

I Here propoſe to deliver my thoughts upon theſe four queſtions. 1. Whether, indefinitely ſpeaking, there be any final cauſes of things corporeal, knowable to men? 2. Whether, if that queſtion be reſolv'd in the affirmative, we may conſider final cauſes in all ſorts of bodies, or only in ſuch as are peculiarly qualified? 3. Whether, or in what ſenſe, the acting for ends may be aſcribed to an unintelligent and inanimate body? 4. And laſtly, how far, and with what cautions, arguments may be framed upon the ſuppoſition of final cauſes?

Whether the final cauſes of natural things are knowable to men?

To begin with the firſt queſtion. Thoſe who would exclude final cauſes from the conſideration of the naturaliſt, ſeem to do it, either becauſe, with *Epicurus*, they think the world was produc'd by atoms and chance, without the intervention of a Deity; and, conſequently, that 'tis in vain to ſeek for ſuch cauſes: or becauſe, with *Des Cartes*, they imagine, that God being omniſcient, 'tis raſh and preſumptuous for men to think they know, or can diſcover what ends he propos'd to himſelf, in his creatures. The ſuppoſition on which the *Epicureans* have rejected final cauſes, has been diſallow'd by the philoſophers of almoſt all other ſects; and ſome have written ſufficient confutations of it: I ſhall, however, in the progreſs of our work, make ſuch occaſional obſervations, as may ſerve to diſcredit ſo unreaſonable an opinion. But the *Carteſian* argument having been ſo prevalent among learned and ingenious men, I ſhall beſtow the more pains in the conſideration of that.

One thing, perhaps, that kept ſo great a philoſopher as *Des Cartes*, from allowing the conſideration of final cauſes in phyſics, was, that the ſchool-philoſophers, and others, are apt to propoſe it too unwarily; as if there were no creature in the world, that was not ſolely, or chiefly deſign'd for the ſervice of man. And, indeed, I have ſeen a body of divinity, publiſh'd by a famous author, wherein he urges this argument for the annihilation of the world; "That ſince the world was made for the ſake of "man, in his travelling capacity; when once man is poſſeſs'd of his ever-"laſting ſtate of happineſs, or miſery, there will be no further uſe of the "world." The opinion that gives riſe to ſuch preſumptuous and unwarrantable expreſſions, was, as I conjecture from his objection, very ſhocking to *Des Cartes*; but the indiſcretion of men ought not to prejudice truth, which is not to be thrown away with the groundleſs conceits that ſome people have pinn'd upon it.

Since then, I cannot cloſe in either with the doctrine of the *Epicureans*, or *Carteſians*, I ſhall leave each party to maintain its reſpective opinion, and proceed to declare my own: but, to clear the way, 'tis neceſſary to premiſe a diſtinction.

Final cauſes, what they may ſignify.

In ſpeaking of the ends which the author of nature is ſaid to have in things corporeal, any of theſe four particulars may be ſignified.

Firſt, Some grand and general ends of the univerſe; ſuch as exerciſing and diſplaying the creator's wiſdom; the communication of his goodneſs, and the admiration and thanks due to him from his intelligent creatures, *&c.* And theſe ends, becauſe they regard the whole creation, I call the univerſal ends of God, or nature.

Secondly, In a more reſtrain'd ſenſe, the ends deſign'd in the number, fabric, ſituation, and motion of great maſſes of matter, that make large parts of the world; ſince 'tis very probable, that theſe bodies, ſuch as the ſun, moon, fix'd ſtars, and the terraqueous globe, were ſo framed, and placed, as not only to perſevere in their own preſent ſtate, but alſo to conduce to the univerſal ends of the creation, and the good of the whole, whereof they are conſiderable parts. Upon which accounts, theſe ends may be called coſmical, or ſyſtematical; as they regard the ſymmetry of the great ſyſtem of the world.

Thirdly, ends that more peculiarly concern the parts of animals, and, perhaps, plants too; or thoſe to which the particular parts of animals are deſtined for the welfare of the whole creature, conſider'd as an entire and diſtinct ſyſtem of organiz'd parts, deſign'd to preſerve himſelf, and propagate his ſpecies, upon that ſtage to which his ſtructure, and circumſtances determine him to act his part. And theſe ends, to diſtinguiſh them from others, may be call'd animal ends.

Laſtly, The ſame expreſſion may ſignify another ſort of ends, which, becauſe they relate particularly to man, may be called human ends, and are thoſe aimed at by nature, where ſhe is ſaid to frame animals, vegetables, *&c.* for the uſe of man. And theſe ends may farther be diſtinguiſh'd into mental and corporeal, not only as man is an animal framed, like others, for his own preſervation, and the propagation of his ſpecies, but alſo as he is made to have dominion over other animals, and works of nature, and fitted to make them ſubſervient to his purpoſes. This diſtinction being thus ſettled, I declare my diſſent as well from the vulgar notion of final cauſes, which allows of none but thoſe we have call'd human ones, as from theirs who wholly reject them all.

'Tis an acknowledg'd principle of the *Carteſian* philoſophy, that there is always the ſame quantity of motion in the world* ; becauſe, ſay they, there is no reaſon why God, who is immutable, ſhould, at the beginning of things, have given ſuch a quantity of motion to matter as ſhould need to be afterwards augmented or leſſen'd. But do not thoſe who employ this negative argument take upon them to judge of the ends that God may have propos'd to himſelf in natural things? For without ſuppoſing that they know what God deſign'd in ſetting matter in motion, 'tis hard for them to ſhew that his deſign could not be beſt accompliſh'd by ſometimes adding to, and ſometimes taking from the quantity of motion he originally communicated to matter. And, I think, it may be worth conſidering, whether by this doctrine of theirs, the *Carteſians* do not more take upon them, than other philoſophers, to judge of God's deſigns. For, if a man be known for very wiſe, and to have various ways of compaſſing his ſeveral ends; he who, ſeeing ſome of thoſe ways have a direct tendency to a rational end, ſhall conclude that end to be one of the number intended, thereby expreſſes more reſpect to that man, than he who ſhould conclude, his end cannot be ſuch; and that he has no other deſign knowable, except a certain general one. And, indeed, it ſeems more eaſy to know, that a particular end, for which an engine is proper, may, among others, be intended by the artificer, tho' never ſo skilful; than to know negatively, that he can have no other than ſuch a certain end.

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* Sir *Iſaac Newton* makes it evident, that there is not always the ſame quantity of motion in the world. “ The *vis inertiæ*, ſays that great philoſopher, is a paſſive principle, by which bodies perſiſt in their motion, or reſt; receive motion in proportion to the force impreſſing it, and reſiſt as much as they are reſiſted. By this principle, alone, there could never have been any motion in the world. Some other principle was neceſſary for putting bodies into motion; and now they are in motion, ſome other principle is neceſſary for conſerving the motion. For, from the various compoſition of two motions, 'tis very certain, there is not always the ſame quantity of motion in the world. For, if two globes join'd by a ſlender rod, revolve about their common centre of gravity, with an uniform motion, while that centre moves on uniformly, in a right line, drawn in the plain of their circular motion; the ſum of the motions of the two globes, as often as the globes are in a right line, deſcribed by their common centre of gravity, will be bigger than the ſum of their motions, when they are in a line perpendicular to that right line. By this inſtance it appears, that motion may be got or loſt. But by reaſon of the tenacity of fluids, and attrition of their parts, and the weakneſs of elaſticity in ſolids, motion is much more apt to be loſt, than got; and is always upon the decay. For, bodies which are either abſolutely hard, or ſo ſoft as to be void of elaſticity, will not rebound from one another. Impenetrability makes them only ſtop. If two equal bodies meet directly *in vacuo*, they will, by the laws of motion, ſtop where they meet, and loſe all their motion, and remain at reſt; unleſs they be elaſtic, and receive new motion from their ſpring. If they have ſo much elaſticity as ſuffices to make them rebound with $\frac{1}{4}$, or $\frac{1}{2}$, or $\frac{3}{4}$ of the force with which they come together, they will loſe $\frac{3}{4}$, or $\frac{1}{2}$, or $\frac{1}{4}$ of their motion. And this may be tried, by letting two equal pendulums fall againſt one another from equal heights. If the pendulums be of lead, or ſoft clay, they will loſe all, or almoſt all their motions; if of elaſtic

And how will a *Carteſian* be ſure, that among the many ends he grants God may have propoſed to himſelf, in the production of his mundane creatures, one is not, that we, whom he has made intelligent Beings, and capable of admiring and praiſing him, ſhould find juſt cauſe to do ſo, for the wiſdom and goodneſs he has diſplay'd in the world? Which attributes we could not well diſcern, and celebrate, unleſs we knew that the creatures were made for ſuch uſes, and that they are exceedingly well fitted to them. God's immutability is, indeed, alledg'd to prove, that the quantity

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" ſtic bodies, they will loſe all but what " they recover from their elaſticity. If " it be ſaid, that they can loſe no motion " but what they communicate to other " bodies, the conſequence is, that *in* " *vacuo* they can loſe no motion; but, " when they meet, muſt go on, and penetrate one another's dimenſions. If three " equal round veſſels be fill'd, the one " with water, the other with oil, and " the third with melted pitch; and the " liquors be ſtirred about alike, to give " them a vortical motion; the pitch, by " its tenacity, will loſe its motion quickly; the oil, being leſs tenacious, will " keep it longer; and the water, being " leſs tenacious, will keep it longeſt; but " yet will loſe it in a ſhort time. Whence " it is eaſy to underſtand, that if many " contiguous vortices of melted pitch, " were each of them as large, as ſome " ſuppoſe to revolve about the ſun and " fix'd ſtars; yet theſe, and all their " parts, would, by their tenacity and ſtiffneſs, communicate their motion to one " another, till they all reſted among " themſelves. Vortices of oil, or water, " or ſome fluider matter, might continue longer in motion; but, unleſs that " matter were void of all tenacity, and attrition of parts, and communication of " motion, the motion would conſtantly " decay. Seeing, therefore, the variety " of motion which we find in the world, " is always decreaſing, there is a neceſſity " of conſerving and recruiting it by active " principles; ſuch as are the cauſe of gravity, by which planets and comets keep " their motions in their orbs, and bodies " acquire great motion in falling; and the " cauſe of fermentation, by which the " heart and blood of animals are kept in " perpetual motion, and heat; the inward " parts of the earth are conſtantly warm'd, " and, in ſome places, grow very hot; " bodies burn and ſhine; mountains take " fire; the caverns of the earth are blown " up; and the ſun continues violently hot, " and lucid, and warms all things by his " light. For, we meet with very little " motion in the world, beſides what is " owing to theſe active principles. And, " if it were not for theſe principles, the bodies " of the earth, planets, comets, ſun, and " all things in them, would grow cold, " and freeze, and become inactive maſſes; " and all putrefaction, generation, vegetation, and life, would ceaſe; and the " planets and comets would not remain " in their orbs." *Newton. Optic.* p. 372, —375.

of motion never varies; but to me 'tis not evident why God's having particular ends, tho' ſome of them ſeem to require a change in his way of acting in natural things, muſt be more inconſiſtent with his immutability, than his cauſing many things, decreed from eternity, to be brought to paſs in proceſs of time. And, particularly, it ſeems not clear, why God may not as well be immutable, tho' he ſhould ſometimes vary the quantity of motion in the world; as he is, tho', according to the opinion of moſt *Carteſians*, he daily creates multitudes of actuating rational ſouls, to unite them to lifeleſs human bodies. I ſay not this, as if I abſolutely rejected the *Carteſian* doctrine about the continuance of the ſame quantity of motion in the whole maſs of matter. For whether that be true or not, 'tis no unuſeful, or improbable hypotheſis; and I have not ſo much argued againſt that, as, upon the ſame grounds they argue for it.

To come then to the thing itſelf; M. *Des Cartes* objects, that 'tis a preſumption in man to pretend he is able to diſcover the ends that the Omniſcient propoſed to himſelf in the making of his creatures. Now I conſider, by way of anſwer, that there are two very different ways, wherein a man may pretend to know the ends of God in his viſible works; for he may either pretend to know only ſome of God's ends in ſome of his works, or he may pretend to know all his ends. He who preſumes to diſcover God's ends, in this latter ſenſe, will ſcarce be excuſed from a high preſumption, and no leſs a degree of folly. But to pretend to know God's ends, in the former ſenſe, is no preſumption; and, to take notice of them, rather a duty. For there are ſome things in nature, ſo curiouſly contrived, and ſo exquiſitely fitted for certain operations and uſes, that it ſeems little leſs than blindneſs in him who acknowledges a moſt wiſe author of things, not to conclude, that tho' they may, perhaps, have been deſign'd for other uſes, yet they were alſo deſign'd for theſe. Thus he who conſiders the admirable fabric of the coats, humours, and muſcles of the eye, and how excellently all the parts are adapted to compoſe an organ of viſion, can ſcarce help confeſſing, that the author of nature intended it ſhould ſerve the animal, to which it belongs, to ſee with. The *Epicureans*, indeed, who believe the world to have been produced merely by a caſual concourſe of atoms, may have a kind of excuſe, which other philoſophers, who acknowledge a deity, want. Thus, the very ſuppoſition, for inſtance, that a man's eyes were made by chance, argues, that they need have no relation to a deſigning agent; and the uſe that a man makes of them, may be either caſual too, or at leaſt an effect of his knowledge, not of nature's. But when, upon the anatomical diſſection, and the optical conſideration of this part, we find it to be as exquiſitely fitted for an organ of ſight, as the beſt artificer could have framed a little engine, purpoſely deſign'd for the uſe of ſeeing; it ſounds very harſh to ſay, that an artificer, who is too intelligent to do things by chance, or to make a curious piece of work, without knowing to what purpoſe, ſhould not deſign it for an uſe to which 'tis wholly adapted.

'Tis poſſible, indeed, he may have deſign'd more uſes of it than one; and, perhaps, ſuch as we cannot find out: however, among its ſeveral uſes, this, to which we ſee it ſo admirably adapted, ſhould be thought one. And I ſee not how it magnifies God's wiſdom, or expreſſes our veneration of it, to exclude from the number of his ends, in framing human eyes, that moſt obvious and ready uſe, which, we are ſure, is made of them; and which they could not be better fitted for. Thus, ſuppoſe a countryman, brought into the garden of a mathematician, ſhould there ſee one of thoſe curious gnomonic inſtruments, that ſhew, at once, the place of the ſun in the zodiac, his declination from the equator, the day of the month, the length of the day, *&c.* it would, indeed, be preſumption in him, whilſt unacquainted both with mathematics, and the ſeveral intentions of the artiſt, to pretend himſelf able to diſcover all the ends for which ſo curious and elaborate a piece was framed: but, when he ſees it furniſh'd with a ſtile, with horary lines, with numbers, and, in ſhort, with all the requiſites of a ſun-dial; and manifeſtly perceives the ſhadow to mark, from time to time, the hour of the day; 'twould be no preſumption in him to conclude it a ſun-dial; whatever other uſes it was deſign'd for.

And I demand of thoſe, who will not allow that any natural things are directed to ends knowable by men, whether, if the divine author of them had really deſign'd them for ſuch ends, the things themſelves are not ſo framed and directed, as in that caſe they ought to be? And whether the fabric and management of natural things do really countenance, or contradict our ſuppoſition?

For my part, I confeſs, I think it no diſparagement to the wiſdom of any agent whatſoever, to ſuppoſe his productions deſign'd for ſuch ends, among others, as they are excellently fitted for; unleſs it appear'd that ſuch ends were unworthy the wiſe agent. But that cannot be juſtly ſaid in our caſe; ſince 'tis not injurious to the divine author of things, to believe that ſome of the ends, to which he deſtined ſeveral of his corporeal works, were to exert and communicate his goodneſs; and to receive from his intelligent creatures an ardent love, a high admiration, and an obſequious gratitude for having diſplay'd ſo much wiſdom and beneficence in them.

And, indeed, I can by no means aſſent to the aſſertion of *Des Cartes*, that none of God's ends in his corporeal works are more manifeſt than others, but that all of them lie equally hid in the abyſs of the divine wiſdom; for there are many of his creatures, ſome of whoſe uſes are ſo manifeſt and obvious, that the generality of mankind have in all ages, and almoſt in all countries, acknowledg'd them. And as to what he adds, that, in phyſics, all things ought to be made out by certain and ſolid reaſons; I anſwer, firſt, that I ſee not how God's deſigning ſome of his works for particular uſes, amongſt others, is inconſiſtent with the phyſical accounts of their creation. Thus, a man may give a mechanical reaſon of the ſtructure of every wheel, and other part of a watch, and of their way of acting upon one another, when rightly ſet together; and, in ſhort, of

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of the contrivance and phenomena of the little machine; tho' he ſuppoſes that the artificer deſign'd it to ſhew the hours of the day, and tho' he has that uſe in view, whilſt he explains the fabric and operations of the watch. Secondly, I readily admit, that, in phyſics, we ſhould ground all we deliver upon as ſolid reaſons as poſſible; but there is no neceſſity that thoſe reaſons ſhould be always preciſely phyſical, eſpecially when we are treating not of any particular phenomenon, produced according to the courſe of nature eſtabliſhed in our ſyſtem; but of the firſt and general cauſes of the world itſelf: from which cauſes, I ſee not why the final uſes that appear manifeſtly to have been deſign'd, ſhould be excluded. And to me 'tis not very material, whether in phyſics, or any other ſcience, a thing be prov'd by the peculiar principles of that ſcience; provided it be firmly prov'd by the common grounds of reaſon. And here let me obſerve, that the fundamental tenets of *Des Cartes*'s own philoſophy, are not prov'd by arguments ſtrictly phyſical; but either by metaphyſical ones, the more general dictates of reaſon, or the particular teſtimonies of experience. Thus, when, for inſtance, he truly aſcribes to God all the motion that is found in matter, and conſequently all the phenomena that occur in the world; he proves it not by a preciſe phyſical argument, that God, who is an immaterial agent, is the efficient cauſe of motion in matter; but only hence, that ſince motion does not belong to the eſſence and nature of matter, matter muſt owe its motion to ſome other Being: and then 'tis moſt agreeable to common reaſon to infer, that ſince matter cannot move itſelf, but muſt be mov'd by ſome other Being, that Being is immaterial; ſince, otherwiſe, ſome matter muſt, contrary to the hypotheſis, be able to move itſelf. And when *Des Cartes* attempts to demonſtrate, that there is always the ſame quantity of motion in the univerſe; and, conſequently, that as much motion as one body communicates to another, it loſes itſelf; he proves it by the immutability of God, which is not a ſtrict phyſical argument, but rather a metaphyſical one; as he had before prov'd, that God was the cauſe of all motion in matter, not by principles peculiar to phyſics, but by the common grounds of reaſon.

But tho' *Des Cartes* ſpeaks very dogmatically and univerſally againſt mens pretending to know any final cauſe in natural things; yet I have met with a paſſage in his writings, where he ſeems to ſpeak more cautiouſly, and oppoſes their opinion, who teach, that God had no other end in making the world, than that of being praiſed by men. His words are theſe. "'Tis ſelf-evident, that we cannot know the ends of God, unleſs "he reveals them to us; and ſuppoſing it morally true, that all things "were made to the end that we ſhould praiſe God for them; in which ſenſe "it may be ſaid, that the ſun was made to give us light; yet it would be "very childiſh and abſurd to ſay, metaphyſically, that God, like a proud "man, had no other end in making the world, but to be prais'd by men "for it; and that the ſun, which is vaſtly larger than the earth, was "wholly deſign'd to give us light, who inhabit but a very ſmall part of the "ſurface of the terraqueous globe." But he, at the ſame time, delivers two or three other things, wherein I cannot acquieſce. As, firſt, that 'tis ſelf-evident, we cannot know the ends of God, unleſs he himſelf reveals them to us. For what he here ſays to be ſelf-evident, is not ſo to the generality of mankind, or even of philoſophers; and therefore I think it ought not to be barely aſſerted, but prov'd. In the next place, he does not ſhew how we are obliged to praiſe God for his works, if God had no intentions that we ſhould do ſo, or that we ſhould diſcover none of the ends for which he deſign'd them. If a judicious man ſhould ſee a book written in ſome language which he is an utter ſtranger to, and ſhould know nothing farther of it than that 'twas compoſed by a very intelligent phyſician; he might indeed conclude, that it was not written by chance; but could not, by inſpecting the book itſelf, be convinced that it was form'd with great skill and kindneſs, and deſerv'd his praiſe and thanks, ſince he was unable to know any of the particular ends to which the ſeveral chapters of it were deſign'd, and conſequently could not ſee how well they were fitted to anſwer ſuch ends. What *Des Cartes* ſays, that it is childiſh and abſurd to think God had created the ſun, which is vaſtly bigger than the earth, only to afford light to a ſmall number of men, is ſomewhat invidiouſly propos'd; for there are few eminent writers who confine the utility of the ſun directly to its affording light to man: and the ſmall bulk of mankind ought not to make it ſeem abſurd, that God may have had an eſpecial eye to their welfare in framing that bright globe; ſince that moſt excellent machine, the human body, appears to be a more admirable thing than the ſun: beſides, the rational and immortal ſoul that reſides in it, is incomparably more noble than a thouſand maſſes of brute unorganized matter can be juſtly reputed. And ſince, in this very diſcourſe, our author confeſſes that we may know the ends of God's corporeal works, if he reveal them to us; a chriſtian philoſopher may be allow'd to think the ſun was made, among other purpoſes, to enlighten the earth, and for the uſe of man; ſince the ſcripture teaches, that not only the ſun and moon, but the ſtars of the firmament, which *Des Cartes*, not improbably, thinks to be ſo many ſuns, were made to give light to the earth, and were divided to all the nations that inhabit it. Perhaps it were not raſh to add, that we may know ſome of God's ends in things corporeal, without leſſening our veneration of his wiſdom, as we know ſome of them in other matters, of which the ſcripture furniſhes us with a multitude of inſtances; as particularly *Job*'s ſacrificing for his friends, and the declar'd uſes of the *Urim* and *Thummim*: ſince God may, if he pleaſes, declare truths to men, and inſtruct them by his creatures and actions, as well as by his word. And thus he taught *Noah* by a rain-bow, and *Jonah* by a gourd and a worm, and regulated the encampments of the *Iſraelites* by the guidance of a cloud, and a fiery pillar. Laſtly, *Des Cartes* objects, that thoſe he diſſents from talk as if they look'd upon God as a proud man, who deſign'd his works only to be praiſed for them; but I know not whether he herein ſpeaks ſo cautiouſly and reverently of God, as he ought, and elſewhere uſes to do. For as humility, tho' a virtue in men, is extremely remote from being any of God's perfections; ſo that may be pride in a man, who is but a creature imperfect, dependant, and having nothing of his own, which wou'd be none at all in the creator, who is incapable of vice, and who may, if he pleaſes, juſtly propoſe to himſelf his own glory for one of his ends, and both require and delight to be prais'd by men for his works; ſince he is moſt worthy of all praiſe, which it is their duty and reaſonable ſervice to pay him.

'Tis not without concern that I find myſelf obliged thus to oppoſe ſome ſentiments of M. *Des Cartes*, for whom I have otherwiſe a great eſteem; for I am not at all of their mind who think that author a favourer of atheiſm, which, to my apprehenſion, would ſubvert the very foundation of his own philoſophy. But judging that his doctrine, as generally underſtood, as to the rejection of final cauſes from the conſideration of philoſophers, tends to weaken one of the beſt and moſt ſucceſsful arguments for the being of a God, whom they ought to admire, praiſe, and reverence; I think it my duty to prefer an important truth before my reſpect to any man, how eminent ſoever, who oppoſes it; and to conſider more the glory of the great author of nature, than the reputation of any of her interpreters.

And to confirm what I have hitherto deliver'd, 'tis remarkable that the excellent contrivance of the great ſyſtem of the world, and eſpecially the curious fabric of animal bodies, with the uſes of their ſeveral ſenſes and parts, have been the great motives which in all ages and nations induced philoſophers to acknowledge a Deity; and that the nobleſt and moſt intellectual praiſes have been paid him by the prieſts of nature, as proceeding from the tranſcendent admiration rais'd in them by the attentive contemplation of the fabric of the univerſe, and of the curious ſtructures of living creatures. And therefore it ſeems injurious to God, as well as unwarrantable in itſelf, to baniſh from natural philoſophy the conſideration of final cauſes; from which, chiefly, I cannot but ſuppoſe that God muſt reap the honour due to thoſe glorious attributes, his wiſdom and his goodneſs. And I confeſs I ſomewhat wonder that the *Carteſians*, who have generally, and ſome of them with ſkill, maintain'd the exiſtence of a Deity, ſhould endeavour to make men throw away an argument which the experience of all ages ſhews to have been the moſt ſucceſsful, and in ſome caſes the only prevalent one, to eſtabliſh among philoſophers the belief and veneration of God. I know the *Carteſians* ſay, that their maſter has demonſtrated the exiſtence of a God, by the innate idea men have of a Being infinitely perfect, who left it upon the human mind as the mark of an artiſt impreſs'd upon his work; and that they alſo aſcribe to God the creation of matter out of nothing, and its motion; which ſufficiently argue the immenſity of his power. But tho' I would by no means weaken the argument drawn from the innate notion of God, ſince many learned men have acquieſced in it; yet why may we not reaſonably think that God, who has taken care men ſhould acknowledge him, may have alſo provided for the ſecuring a truth of ſo great conſequence, by ſtamping characters, or leaving impreſſions of his wiſdom and goodneſs, as well externally upon the world, as internally upon the mind? The bare contemplation of the fabric of the world, without conſidering any part of it as deſtin'd to certain uſes, may ſtill leave men unconvinced that there is any intelligent, wiſe, and provident author and diſpoſer of things; ſince we ſee the *Ariſtotelians*, notwithſtanding the extent, ſymmetry, and beauty of the world, generally believed it to have been eternal. And tho' they did not allow it to have been created by God, yet becauſe they aſſerted that animals, plants, *&c.* act for ends; they were obliged to acknowledge a provident and powerful Being, that maintain'd and govern'd the univerſe, which they call'd *Nature*; tho' they often dangerouſly miſtook, by confounding this Being with God himſelf; and at other times ſpeaking of it as co-ordinate with him; as in that famous axiom of *Ariſtotle*, "God and nature do nothing "in vain." I acknowledge, therefore, that as I ſet a juſt value upon the *Carteſian* proof of God's exiſtence; ſo I ſee no reaſon why we ſhould give up any other ſtrong argument, that proves ſo noble and important a truth; eſpecially ſince the *Carteſian* way of conſidering the world, tho' very proper, indeed, to ſhew the greatneſs of God's power, does not, like the way I plead for, manifeſt that of his wiſdom and beneficence. For while a *Carteſian* only ſhews that God is admirably wiſe, upon the ſuppoſition of his exiſtence; the ſame thing is manifeſted in our method, by the effect of a wiſdom as well as power, that cannot reaſonably be aſcribed to any other than a moſt intelligent and potent Being: ſo that by this means men may at once be brought to acknowledge God, to admire him, and to praiſe him.

SECT. II.

Whether final cauſes are to be expected in all, or only in ſome particular bodies?

BEFORE I proceed to the ſecond queſtion, Whether we may conſider final cauſes in all ſorts of bodies, or only in ſuch as are peculiarly qualified? I muſt divide natural bodies into animate and inanimate. The former term I here take in the larger ſenſe, to comprehend not only animals, but vegetables; tho' I ſhall not reject the opinion of thoſe who are unwilling to allow plants ſuch a ſoul or life, as is confeſſedly granted to animals.

Of the inanimate bodies of the univerſe, the nobleſt, and thoſe which chiefly deſerve to be conſider'd on this occaſion, are the ſun, planets, *&c.* For when men ſaw thoſe vaſt luminous globes, and eſpecially the ſun, to move ſo conſtantly and regularly about the earth, diffuſing thereon both light and heat, and by their various revolutions to produce day and night, ſummer and winter, and the viciſſitudes of ſeaſons; they concluded that theſe motions were guided by ſome divine Being, and deſign'd for the benefit of man. Whether this be a demonſtrative proof, I ſhall not now debate; but ſurely it may carry thus much of probability with it, that in caſe a man ſhall think the fabric of the celeſtial bodies was the production of an intelligent and divine agent, the irregular phenomena will not contradict him; ſince there is nothing in that fabric unworthy a divine author: and the motions and effects of the ſun and ſtars may well allow us to think, that, among other purpoſes, they were made to illuminate the terreſtrial globe, and bring heat and other benefits to the inhabitants of it. So that the contemplation of the heavens, which ſo manifeſtly declare the glory of God, may juſtly excite men both to admire his power and wiſdom in them; and to return him thanks and praiſes for the great advantages we thence receive. On the other hand, it may be ſaid, that in bodies inanimate, whether the portions of matter they conſiſt of be greater or leſs, the contrivance is very rarely ſo exquiſite, but that the various motions and occurſions of their parts may, without much improbability, be ſuſpected capable, after many eſſays, to caſt one another into ſeveral of thoſe circumvolutions call'd by *Epicurus* συστροφὰς, and by *Des Cartes*, *Vortices*; which being once made, may continue a long time, after the manner explain'd by the latter. But allowing this hypotheſis to be poſſible; when I conſider that we are not yet ſufficiently acquainted with the true ſyſtem of the world, nor uſually ſenſible how ſmall a part the terreſtrial globe makes of the univerſe; I am apt to ſuſpect, that men often aſſign the ſyſtematical ends and uſes of the celeſtial bodies upon ſlender evidence; concluding them made and moved only for the ſervice of the earth, and its inhabitants. And tho' I will not deny that as we actually receive benefits by the eſtabliſhed order and motion of the heavenly bodies, ſo one of the ſeveral uſes intended by the author of nature, may particularly regard our ſpecies; yet perhaps 'twill not be eaſy to prove, that ſome of thoſe bodies and motions are not rather intended for other purpoſes, than to caſt their beams, or ſhed their influences upon the earth: at leaſt, it ſeems probable to me, that the ſituations of the celeſtial bodies do not afford, by far, ſo clear and cogent arguments of the wiſdom and deſign of the author of the world*, as do the bodies of animals and plants. And for my part, I think I ſee more of admirable contrivance in the muſcles of a human body, than in what we yet know of the aſtronomical world: and the eye of a fly, as far as appears to us, ſeems to be a more curious piece of workmanſhip, than the body of the ſun itſelf.

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* As a noble inſtance of wiſdom and deſign in the ſituation of the planets, take an obſervation of the great Sir *Iſaac Newton*. "Thoſe planets, *ſays he*, have the greater "denſity, *cæteris paribus*, which are pla"ced neareſt the ſun. Thus *Jupiter* is "more denſe than *Saturn*, and the earth "more denſe than *Jupiter*. For it was "neceſſary to place the planets at different "diſtances from the ſun, that each might "receive a greater or a leſs degree of his "heat, according to its denſity. If our "earth were placed in the orbit of *Saturn*, "the water of it would be frozen up; and "if in the orbit of *Mercury*, 'twould pre"ſently exhale in vapour. For the ſun's "light, to which his heat is proportionable, "is ſeven times more denſe in the orbit of "*Mercury*, than with us; and I have "found, by the thermometer, that water "will boil with a heat ſeven times as great "as that of the ſummer's ſun. But the "matter of *Mercury* is, doubtleſs, fitted for "heat; and therefore muſt be denſer than "that of our earth: ſince all denſe matter "requires a greater degree of heat, to per"form the operations of nature." *Newton. Princip. p.* 372.

As for other inanimate bodies, the matter whereof ſeems unorganiz'd, tho' there be no abſurdity in ſuppoſing that theſe were alſo made for diſtinct and particular purpoſes, if not alſo for human uſes; yet moſt of them are of ſuch eaſy and unelaborate textures, as to make it ſeem poſſible that various occurſions and juſtlings of the parts of the univerſal matter might, at one time or other, have produced them: ſince we ſee that in ſome chymical ſublimations and cryſtallizations of mineral and metalline ſolutions, and other phenomena, where the motions appear not to be particularly guided and directed by an intelligent cauſe, bodies of as various textures as thoſe are producible.

If it be ſaid, that ſuppoſing chance, or any thing elſe, without the particular guidance of a wiſe and all-diſpoſing cauſe, can make a finely-ſhaped ſtone, or a metalline ſubſtance, growing, as ſilver ſometimes does, in the form of a plant; ought we not to allow that chance may alſo make vegetables and animals? I can by no means grant the conſequence. For there are ſome effects ſo eaſy, and readily to be produced, that they do not infer any knowledge or intention in their cauſes; whilſt there are others that require ſuch a number and conjunction of conſpiring cauſes, and ſuch a continued ſeries of motions or operations, that 'tis utterly improbable they ſhould be produced without the ſuperintendency of a rational agent, wiſe and powerful enough to range and diſpoſe the ſeveral intervening materials and inſtruments, in a manner neceſſary to produce ſuch a remote effect: it will not therefore follow, that if chance could produce a ſlight contexture in a few parts of matter, we may ſafely conclude it able to produce ſo exquiſite and admirable a contrivance, as that of the body of an animal. What does it argue then, if ſometimes, in ſawing pieces of variegated marble, we happen to meet with the delineations or pictures of towns, woods, and men? For, beſides that the delightfulneſs and rarity of ſuch ſpectacles inclines the imagination to favour them, and to ſupply their defects; would any wiſe man conclude from hence, that a real town or wood, much leſs numbers of men, ſhould be made by ſuch a fortuitous concourſe of matter? What compariſon is there betwixt the workmanſhip that ſeems to be expreſs'd in a few irregular lines, drawn upon a plane, with perhaps two or three colours luckily placed, and the great multitude of nerves, veins, arteries, ligaments, tendons, membranes, bones, glands, *&c.* required to compoſe a human body; every one of the numerous parts whereof muſt have its determinate ſize, figure, conſiſtence, ſituation, connexion, *&c.* and many, or all of them together, conſpire to exerciſe and perform determinate functions and uſes? And indeed I never ſaw any inanimate production of nature, or, as they ſpeak, of chance, whoſe contrivance was comparable to that of the meaneſt limb of the moſt deſpicable animal; nay, there is infinitely more art expreſs'd in the ſtructure of a dog's foot, than in that of the famous clock at *Strasburg*.

Evident marks of deſign in the ſtructure of the eyes, and other parts, of animals.

And tho' the paw of a dog be far inferior in its ſtructure to the hand of a man, yet even this is inconſiderable, if compared to the eye; the ſeveral

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parts whereof, how numerous ſoever, could none of them be ſpared or alter'd, unleſs for the worſe; as may appear from thoſe many diſeaſes obſerv'd in that ſmall admirable organ: for each of thoſe diſeaſes conſiſts in this, that the humours, or other parts of the eye, are brought to a ſtate different from that whereto nature had deſign'd it. 'Twould be tedious to enumerate the ſeveral diſtempers of the eye; wherefore I ſhall only mention two or three particulars, wherein one would ſcarce imagine that a ſmall receſs from the natural ſtate could bring any conſiderable or ſenſible inconvenience. That which we call the pupil, is not a ſubſtantial part of the eye, but only an aperture of the uvea; almoſt perpetually changing its bigneſs, according to the different degrees of light the eye chances to be expoſed to. And therefore, it ſhould ſeem, that whilſt this hole remains open, it performs its office, by giving entrance to the incident rays of light. And yet I lately ſaw a woman, who, after a fever, was not able to dilate the *Pupillæ* of her eyes, as before; and tho' they were but very little narrower than ordinary, yet ſhe complain'd ſhe had thereby almoſt loſt her ſight. The preternatural conſtriction of the pupil is indeed no frequent diſtemper; yet phyſicians have given it a place among the ſtated diſeaſes of the eye. And, on the other ſide, tho' a competent wideneſs of the pupil be requiſite to a clear and diſtinct viſion; yet if its dilatation exceeds the due limits, there is thereby produced a diſtemper worſe than the former; becauſe it often almoſt totally deprives the patient of ſight. It may ſeem alſo but a ſlight circumſtance, that the tranſparent coats of the eye ſhould be devoid of colour; and of as little moment, that the cornea ſhould be very ſmooth, provided it remain tranſparent: yet when either of theſe circumſtances is wanting, the ſight may be greatly vitiated. Thus we ſee, that in the yellow jaundice, the adventitious colour wherewith the eye is ting'd, makes the patient think he ſees many objects yellow, which are of a contrary colour. And I know a gentleman, who having had a ſmall puſtule excited and broken upon the cornea, tho' the eye has long been cicatriz'd; yet a very little inequality or depreſſion, that ſtill remains upon the ſurface of that tranſparent coat, ſo affects him, that when he comes into the open fields, or the ſtreets, he for a pretty while thinks he ſees objects very glaring, and as many others as men uſually do ſtones at the bottom of clear water: which I impute to the want of uniformity in the refraction of thoſe reflected rays of light, that fall upon the unequal ſurface of the cornea. To give a further proof, that the eye was made with deſign, I ſhall here take notice of an obſervation or two, that do not occur in the diſſection of that part, and are therefore often unobſerved by anatomiſts.

I have found in frogs, that, beſides thoſe parts of the eye which they have in common with men, dogs, cats, and moſt other animals, there is a peculiar membrane or cartilage, which is not commonly perceiv'd; wherewith they can at pleaſure cover the eye, without too much hindering the ſight, becauſe the membrane is both tranſparent and ſtrong; ſo that it may paſs for a kind of moveable cornea, or occaſional ſafe-guard to the eye. In furniſhing frogs with this ſtrong membrane, the providence of nature ſeems very conſpicuous; for, they being amphibious creatures, deſign'd to paſs their lives in watery places, which, for the moſt part, abound in plants, endow'd with ſharp edges, or points; and the progreſſive motion of this animal, being not by walking, but by leaping, if his eyes were not provided with ſuch a caſe, he muſt either ſhut them, and ſo leap blind-fold; or, by leaving them open, muſt run the risk of having the cornea cut, prick'd, or otherwiſe offended; whilſt this membrane, as was ſaid, is like a kind of ſpectacle, that covers the eye, without taking away the ſight: and as ſoon as the occaſion for it is over, the animal withdraws it into a little cell, where it reſts till its uſe be again required. This membrane becomes viſible, by applying the point of a pin, or any ſuch ſharp thing, to the eye of a frog, whilſt his head is held ſteady; for to ſcreen his eye, he will preſently cover it therewith, and afterwards withdraw it, upon a removal of the ſuſpected danger. And becauſe many birds are deſtin'd to fly among the branches of trees, and buſhes; leſt, by this means, the prickles, twigs, leaves, &c. ſhould wound or offend their eyes, nature hath alſo given them ſuch another kind of horny membrane, as we find in frogs.

'Tis known that men, and moſt four-footed beaſts, and birds, have ſeveral muſcles belonging to their eyes, by the help of which, they can turn them any way; and ſo obvert the organ of ſenſe to the object. But nature, not having given that mobility to the eyes of flies, ſhe, in recompence, furniſhes them with a multitude of little protuberant parts, finely rang'd upon the convex of their large bulging eyes: ſo that by means of theſe numerous little ſtuds, numberleſs rays of light are reflected from objects placed on either hand, above or beneath the level of the eye, and conſequently fall upon that organ, to render the objects they come from, viſible to the animal; and by the help of a good microſcope, and a clear light, ſome hundreds of theſe little round protuberances may be diſcover'd, curiouſly rang'd, on the convexity of a ſingle eye of an ordinary fleſh-fly.

But ſome may here pretend, that all organs of ſight ought to be conform'd to thoſe of men; theſe being the beſt and moſt perfect. And, indeed, man being juſtly reputed the moſt perfect of animals, it is not ſtrange he ſhould preſume, that his eyes, and other parts of his body, are the beſt contriv'd of any to be found in nature; yet we cannot, from hence, ſafely conclude, that all eyes, which, in other animals, are of different ſtructures from thoſe of man, ſhould, for that reaſon, be defective. For, firſt, the admirable wiſdom diſplay'd by the author of nature, in fitting the eyes, and other organical parts of animals, to their ſeveral uſes, and the reſpective functions we ſee them exerciſe, may juſtly perſuade us, that the things whoſe reaſons and uſes we do not alike diſcern, are, nevertheleſs, moſt wiſely conſtituted: God having too much knowledge to do any thing unskilfully; and we having too much preſumption, if we ſuppoſe he had no ends in framing his creatures beyond the reach of our diſcovery. And, ſecondly, the eye is not to be conſider'd abſtractedly, as an inſtrument of viſion;

PHYSICS. viſion; but as an inſtrument belonging to an animal, that is to make uſe of it in particular circumſtances. And therefore it ought highly to recommend the wiſdom and providence of the great author of things, that he has furniſh'd various ſpecies of animals with organs of ſight, very differently framed and placed; ſince this diverſity nobly manifeſts his great providence and knowledge, in having ſo admirably ſuited the eyes of all animals, both to the reſt of their bodies, and to thoſe parts of the great theatre of the world, on which he deſign'd they ſhall live and act. Thus, tho' ſeveral beaſts, as horſes, oxen, *&c.* have their eyes furniſh'd with a ſeventh muſcle, beſides the ſix they ſhare in common with men; we muſt not conclude that either the organs of viſion are imperfect in men, or that thoſe of theſe beaſts have ſomething ſuperfluous: for they being to feed, for the moſt part, on the graſs of the field; and that they may the better chuſe their food, being obliged to turn their eyes, for a long time together, downwards; the ſeventh muſcle excellently ſerves them for that purpoſe, by enabling them to continue unwearied by ſuch a poſture; whilſt man, who has no ſuch neceſſity of looking aſſiduouſly downwards, would be only incumber'd by a ſeventh muſcle.

On the other hand, the deficiency obſervable in the eyes of ſome animals, compared to thoſe of man, may be aſcribed to the juſt contrivance of nature, that, on moſt occaſions, declines doing what is unneceſſary to the particular ends ſhe aims at in the fabric of a part. Thus moles, being deſtin'd to live, for the moſt part, under ground, have their eyes ſo little, in proportion to their bodies, that 'tis commonly believ'd they have none at all; but tho' I have found the contrary, yet their eyes are very different from thoſe of other four-footed beaſts; which need not be wonder'd at, conſidering that nature deſign'd theſe creatures to live under ground, where ſight is of no uſe, and where large eyes would be more expos'd to danger; beſides, their ſight, as dim as it is, ſerves them to perceive when they are no longer under ground; which ſeems to be the moſt neceſſary intelligence they want from their eyes.

'Tis obſerved, that the organs of viſion in a camelion are of a very uncommon ſtructure, ſince, to omit other conſiderable peculiarities, his eyes often move independently on each other; ſo that, for inſtance, he can look directly forward with the right eye, and at the ſame time directly backward, towards his tail, with the left; or may turn the pupil of the former ſtrait upwards, whilſt the other is turn'd directly downwards. This peculiar power ſeems to have been granted him by providence, that as he is a very low animal, and deſtin'd to live, for the moſt part, in trees and buſhes, and there to feed chiefly on flies; he may perceive them, which way ſoever they chance to come, within the reach of his tongue, which, being of a great length, he ſuddenly darts out, and therewith catches his prey.

Many fiſh have the cryſtalline humour of their eyes almoſt ſpherical, and conſequently much rounder than it is found in man, and other terreſtrial animals. And this difference of figure, tho' it would be inconvenient for us, is very well accommodated to them; ſince they, living in the water, which, as a thicker medium, much more refracts the rays of light than the air, thro' which they paſs to our eyes; 'twas fit their cryſtalline humour ſhould be of that figure, to refract the rays already refracted by the water, and thereby make them converge, ſo as to paint the images of objects at the bottom of the eye.

Should a perſon of curioſity ſurvey and conſider the various ſtructures of the organs of viſion, in different animals, and compare them with the other parts of the reſpective animal; the ſcene he is deſign'd to act on, and the uſes each of them is to make of his eyes, in the moſt ordinary circumſtances; he might, doubtleſs, offer a probable reaſon of the differences in thoſe organs, which, to a common obſerver, would ſeem to be errors, or defects in nature. Thus, tho' the pupil of the eye be oblong in horſes, oxen, and ſome other quadrupeds, as well as in cats; yet, in the former kinds of animals, it lies tranſverſly from the right ſide of the eye to the left, whilſt in cats its ſituation is perpendicular; for horſes and oxen, being uſually obliged to find their food on the ground, they the more conveniently receive the images of the graſs, *&c.* in a horizontal view, by having their *Pupillæ* tranſverſly placed; whilſt cats, being to live chiefly on rats and mice, which are animals that uſually climb, and run upon ſteep places; the moſt commodious ſituation of their pupil, for diſcovering and purſuing theſe objects, is the perpendicular. But, to proceed: the different ſtructures, and ſituations of the eye, in different animals, wonderfully ſhew a great variety in the skill of the divine author. And, indeed, if I might preſume to gueſs at any of God's ends, that are not manifeſt; I ſhould think that the delightful variety, we may obſerve, not only in animals themſelves, conſider'd as entire ſyſtems; but in thoſe parts of them which appear deſtin'd for the ſame function; as particularly, that the organ of viſion was deſign'd, at leaſt, among other ends, to diſplay the great creator's manifold wiſdom, and to ſhew that his skill is not confined either to one ſort of living engines, or in their parts of the ſame kind, to the ſame contrivance; but is able to make a multitude of ſurprizing organs, all of them curious, and exquiſite in their kind, with regard to their different uſes.

To be able to frame clocks and watches, ſhips, mills, *&c.* manifeſts a far greater skill in an artificer, than the power of making but one of thoſe engines, how perfectly ſoever he contriv'd it. And the ſame ſuperiority of knowledge would be diſplay'd, by contriving engines of the ſame kind, or for the ſame purpoſes, after very different manners.

Thus weights are of great uſe and neceſſity in the famous clock of *Straſburg*; and therefore it recommends the contrivers of watches, that they give them a very little and portable bulk, which is wanting in the *Strasburg* machine; and ſtill more, that they can make a clock without weights, and ſubſtitute a ſpring to perform their office.

And thus tho' it ſeems abſolutely neceſſary that an animal ſhould be furniſh'd with feathers in order to fly, yet the wiſe creator hath ſhewn, that he is not confined to apply them for that purpoſe; ſince a flying-fiſh is able to move a great way in the air. And the *Indies* have lately afforded a ſort of flying ſquirrels, one whereof I ſaw, alive, at *Whitehall*. And tho' the flight of theſe is ſmall, yet there is another kind of animal, without feathers, that long continues upon the wing, and that is the bat; ſome whereof I have ſeen but little leſs than hens: and was aſſur'd, by a credible eye-witneſs, that, in the kingdom of *Golconda*, he had ſeen much bigger.

This conſideration is alone ſufficient to juſtify the wiſdom of the creator; who, being a moſt free, as well as a moſt wiſe agent, men ought not to find fault, if he think fit to recommend his wiſdom, by diſplaying it in very different manners: tho' there are many caſes wherein the leſs perfect fabric, or ſituation, of an eye, or other organical part, may be more convenient than the correſpondent organ of man, to obtain the ends for which it was given to an animal deſign'd to act upon its reſpective ſtage, and live by its peculiar proviſion. Beſides, an organical part may, in ſome animals, be intended for more uſes than in others; and, therefore, require a different ſtructure: as, in moles, the feet are differently framed, or ſituated, from thoſe of other quadrupeds; becauſe the chief uſe they are to make of them, is not to walk upon the ground, but to dig themſelves ways under it; provident nature wiſely ſuiting the fabric of the parts to the uſes wherein they were to be employ'd: as, a mechanic employs one contrivance of his wheels, pinions, *&c.* when he makes a mill to be driven by water; and another, when it is to be mov'd by the wind. The camelion has a tongue, both peculiarly ſhaped, and of a length diſproportionate to that of his body; becauſe, as we before obſerved, he is to take his prey, which are flies, by ſhooting out that inſtrument; and could not often, otherwiſe, approach very near them, without frighting them away. And, in many caſes, where this reflection does not ſo properly take place, we may obſerve, there is a wonderful compenſation made for that which ſeems a defect in the parts of an animal, of a particular ſpecies, compar'd with the correſpondent ones of an animal of ſome other ſpecies. Thus birds, which want teeth to chew their food, are not only furniſh'd with hard bills to break it; and, birds of prey, with crooked ones to tear it; but, which is more conſiderable, have crops to prepare and ſoften it, and very ſtrong muſcular ſtomachs, to digeſt and grind it: in which work, they are uſually aſſiſted by gravel, and little ſtones, that they are led, by inſtinct, to ſwallow; great quantities whereof are often found in their ſtomachs.

Chance an imaginary Being.

And let it be here obſerv'd, that chance is really no natural cauſe, or agent, but a creature of man's own making. For the things that are done in the corporeal world, are really done by the parts of the univerſal matter, acting and ſuffering according to the laws of motion, eſtabliſhed by the author of nature. But we men, looking upon ſome of theſe parts as directed in their motions by God, or nature, and as diſpoſed to the attainment of certain ends; if, by the intervention of other cauſes, that we are not aware of, an effect be produced, very different from that which we ſuppoſed was intended; we ſay that effect was produced by chance: ſo that chance is indeed but a notion of ours, and ſignifies no more than that in our apprehenſions the phyſical cauſes of an effect did not tend to the production of what they have, nevertheleſs, produced. And therefore I wonder, that the philoſophers who preceded *Ariſtotle*, never treated of chance among natural cauſes.

And as ſome ſtones of the moſt curious ſhapes, have embolden'd many favourers of *Epicurus* to ſet them in competition with thoſe animals, or parts of animals, from their likeneſs whereto they have receiv'd their names; we ought to conſider, that ſeveral learned men have, of late, made it very probable, that theſe ſtones were once really the animals, or thoſe parts of them which they reſemble, and were afterwards turn'd into ſtones by the ſupervention of ſome petreſcent matter, or petrifying cauſe. And, allowing ſome of theſe ſorts of ſtones to be the production of the mineral kingdom; yet it would not clearly follow, that they owe their ſhapes to chance, ſince 'tis no abſurdity to admit ſeminal principles in the more elaborate ſorts of foſſils. However, I think it very injurious to make theſe productions rival the animals to which they are compared. For the ſhape, in which alone they and the animals agree, being an external thing, is not worthy to be mention'd, in compariſon of that wherein they differ; the rude and ſlight texture of the beſt ſhaped ſtone being immenſely inferior to the internal contrivance of an animal; which muſt conſiſt of a multitude of parts of a determin'd figure, bulk, ſituation, *&c.* as is obvious to thoſe who have ſeen diſſections skilfully made. And 'tis not only in the firm and quieſcent parts that this great internal difference between ſtones, and the animals they reſemble, is to be found: there appears a far greater difference between a living animal, and a ſtone, than any the anatomical knife can ſhew us betwixt a dead one, and a ſtone, tho' ever ſo curiouſly figured. For there are numberleſs liquors, ſpirits, digeſtions, ſecretions, coagulations, motions of the whole body, of the limbs and other parts, which are lodg'd and perform'd in a living body, and are, perhaps, more admirable than the ſtructure of the ſolid and quieſcent parts themſelves: ſo that, tho' a ſtone, in external appearance very like a ſhell-fiſh, were made by chance; yet from thence to conclude, that chance may make a living ſhell-fiſh, would be to argue worſe than he who ſhould pretend, that becauſe an unskilful ſmith may make a hollow piece of metal like a watch-caſe; and fill it with ſome rude ſtuff, he muſt therefore, be able to make a watch; for there is leſs difference betwixt the skill expreſſed in making the caſe of a watch and the movement, than in making a body like a ſhell, and the internal parts of a real fiſh: or, that becauſe putrefaction and winds have ſometimes made trees hollow, and blown them into the water, where they ſwim like boats; therefore the like cauſes may make a regular galley, according to the laws of naval architecture, man it, ſteer it, excite and guide all its motions to the beſt advantage, for the preſervation, and various uſes of the veſſel. In ſhort, if chance ſometimes does ſtrange things, 'tis in regard to what ſhe herſelf, not to what nature uſes to perform.

And now, to give my thoughts upon the ſecond queſtion. 1. I think the naturaliſt may draw arguments from the ends and uſes of the parts of living bodies, provided he proceeds herein with due caution. 2. 'Tis my opinion, that the inanimate bodies here below, proceeding not from ſeminal principles, have but a more ſlight texture; ſuch as earths, liquors, flints, pebbles; and will not ſufficiently warrant reaſonings drawn from their ſuppoſed ends. 3. It ſeems to me, that the celeſtial bodies abundantly declare God's power and greatneſs, by the immenſity of their bulk, and, if the earth ſtand ſtill, the celerity of their motions; and alſo argue his wiſdom, and general providence, with regard to them: becauſe he has, for ſo many ages, kept ſuch a variety of vaſt vortices, or other maſſes of matter in motions immenſely rapid, without permitting them to deſtroy one another, or loſe their regularity. And I ſee no abſurdity in ſuppoſing that, among other uſes of the ſun, and of the ſtars, the ſervice of man might be intended; tho' I doubt whether, from the bare contemplation of the heavens, and their motions, it can be cogently inferr'd, that the chief end of them all is to enlighten the earth, and bring benefits to the creatures that live upon it.

Revelation allows us to ſpeak more poſitively of final cauſes, than natural philoſophy.

Hitherto I ſuppoſe the naturaliſt to diſcourſe merely upon phyſical grounds; but if revelation be admitted, we may rationally believe more, and ſpeak leſs doubtfully of the ends of God, than bare philoſophy will warrant us to do. For if God is pleas'd to declare any thing to us concerning his intentions in the making of his creatures, we ought to believe it, tho' the conſideration of the things themſelves did not give us the leaſt ſuſpicion of it; which yet, in our caſe, they do. The ſcriptures expreſly teach us, that "God made the two great luminaries, the greater to rule "the day, and the leſſer to rule the night;" that "he made the ſtars al"ſo, and ſet them in the firmament, or expanſum of heaven, to give "light upon the earth." And theſe are reckon'd among the uſes of theſe luminaries: "to divide the day from the night; and to be for ſigns, and "for ſeaſons, and for days and years." And *Moſes*, diſſuading the *Iſraelites* from worſhipping the ſun, the moon, and the ſtars, ſays, that "the "Lord had imparted them unto all nations under the heaven." And therefore thoſe *Carteſians* who admit the authority of holy ſcripture, ſhould not reject the conſideration of ſuch final cauſes as revelation diſcovers to us; ſince 'tis certainly no preſumption to think we know God's ends, when he himſelf acquaints us with them: nor to believe that the ſun, tho' generally eſteem'd a nobler body than the terreſtrial globe, was made, among other purpoſes, to enlighten it. 'Tis recorded, in the book of *Geneſis*, that God's deſign in making man, was, that he ſhould "ſub"due the earth, and have dominion over the fiſh of the ſea, and over the "fowl of the air, and over the cattle, and over all the earth, and over "every living thing that moveth upon the earth." And the ſame book informs us, that, after the deluge, God "deliver'd all terreſtrial beaſts, "and fowl, and fiſh, and every moving thing that lives," into the hands of men, and intended that they ſhould eat animals; as, before the flood, he had appointed them all the ſorts of wholeſome vegetables for their food. PHYSICS. And ſince God was pleaſed to appoint that men ſhould live on theſe creatures, it cannot be abſurd to ſay, that, among other purpoſes to which he deſtin'd the ſun, his ſhining upon the earth was one; ſince, without his light and heat, men could not provide for themſelves; and neither thoſe plants that men and cattle muſt feed upon, could grow and ripen, nor, conſequently, thoſe animals that were to be their principal food, and ſerve them for other uſes, could be ſuſtain'd, and provided for. Many other texts might be here alledged to the ſame purpoſe, but I ſhall content myſelf to mention that of the royal prophet, when, ſpeaking of man to his maker, he ſays, "Thou haſt made him a little lower than the angels, "and haſt crown'd him with glory and honour. Thou madeſt him to "have dominion over the works of thine hands, and haſt put all things "under his feet."

Indeed if we conſider only that viſible part in man, his body; the ſmallneſs thereof may make it ſeem improbable that portions of the univerſe, incomparably greater than he, ſhould be intended for his ſervice. But chriſtians will not think this incredible, if they conſider man, as he chiefly conſiſts of a rational mind; which proceeds immediately from God, and is capable of knowing him, loving him, and being eternally happy with him. They who deſpiſe man, conſider'd in this capacity, very little know the worth of a rational ſoul. But God, who is the beſt judge in this caſe, was pleas'd to conſider men ſo much, that it gave *David* cauſe to admire, as we juſt now ſaw; and not only to form them in his image, at their firſt creation; but when they had wilfully loſt and forfeited it, he vouchſafed to redeem them by the ſufferings and death of his own ſon, who is incomparably more excellent than the whole world. And 'tis not incredible that God ſhould have intended many of his other works to be ſerviceable to man; ſince, by miraculous operations, he hath ſometimes ſuſpended the laws of nature, and ſometimes over-ruled them for his ſake; as appears by the flood, by the paſſage of the *Iſraelites* thro' the red-ſea, and the river of *Jordan*; by the ſtanding ſtill of the ſun and moon at *Joſhua*'s command; by the inefficacy of the burning fiery furnace on *Daniel*'s three companions; and, in ſhort, by the ſtupendous eclipſe of the ſun at the crucifixion of the Meſſias.

And we ſhall the leſs ſcruple to admit that ſuch vaſt bodies as the ſun and moon may tend to ſerve mankind, if we conſider, 'tis far from being a conſtant rule, that a thing more excellent cannot be employ'd for the good of a leſs excellent. Thus an angel was ſent to relieve *Hagar* in the wilderneſs; another had regard to the life of a prophet's aſs; and many more were employed on earth, in doing good offices to particular perſons: and, of all the angels in general, the epiſtle to the *Hebrews* informs us, that "they are miniſtring ſpirits, ſent forth to miniſter unto them who "ſhall be heirs of ſalvation."

SECT. III.

How inanimate bodies may act for ends, wherewith they are unacquainted.

PReparatory to the diſcuſſion of the third queſtion, "Whether, and in "what ſenſe, the acting for ends may be aſcribed to an unintelligent, "and inanimate body?" it will be neceſſary to clear the grand difficulty that has before, and ever ſince the time of *Ariſtotle*, perplexed thoſe who allow the conſideration of final cauſes in natural philoſophy. This difficulty is obvious enough; for much the greater part of bodies being ſenſeleſs, and, moſt of them, lifeleſs too; it ſeems inconceivable how they ſhould conſtantly act for ends they are not capable of pre-deſigning, and, appoſitely employ means they have no knowledge to make choice of.

Ariſtotle, who expreſly teaches, that nature does nothing in vain; and, rightly judg'd, that the actions of natural agents had a tendency to certain ends; takes no notice of this difficulty, but ſeems rather to ſhift it off than reſolve it.

But, to conſider the difficulty itſelf, there are two accounts on which the actions of natural agents may be ſaid to tend to a certain end; as, either when the agent has a knowledge of that end, and acts with an intention to obtain it; or, when the action of the proximate agent is directed as it ought, to obtain an end, which, yet, is neither known, nor intended, by that proximate agent; but by a remoter, that is intelligent. In the former of theſe ſenſes, I cannot admit, that any inanimate body acts for ends; ſince that pre-ſuppoſes the agent both to know the end he is to obtain, and to purpoſe to obtain it: things whereof inanimate bodies are incapable. And, to fancy with ſome, that they may have a knowledge, *ſui generis*, as they ſpeak; which, tho' confined to the actions proper to a particular kind of body, ſhall yet ſuffice to determine them to thoſe actions, is, to offend againſt that rational, and receiv'd rule in philoſophy, that Beings are not to be multiply'd, without there appears a neceſſity for them; and to introduce a ſort of knowledge that ſeems unintelligible.

It remains, then, that I embrace the ſecond ſenſe, in which 'twas formerly ſaid, natural things may work for an end; tho', in this caſe too, we muſt ſpeak ſomewhat improperly: for the action may more juſtly be attributed to the remote intelligent, than to the immediate agent, which is but, as it were, the inſtrument of the other. Now, it appears to me, that the moſt wiſe, and powerful author of nature, whoſe piercing ſight is able to penetrate the whole univerſe, and ſurvey all the parts of it at once; did, originally, frame material things into ſuch a ſyſtem, and ſettle among them ſuch laws of motion, as he judged ſuitable to the ends he propoſed to himſelf in making the world. And as, by virtue of his vaſt and boundleſs intellect that he, at firſt, employ'd; he was able, not only to ſee the preſent ſtate of things he had made, but to foreknow all the effects that particular bodies, peculiarly qualify'd, and acting according to the laws of motion, by him eſtabliſhed, would, in particular circumſtances, have on one another; ſo, by the ſame omniſcient power, he was able to contrive the whole fabric, and all its parts, in ſuch a manner, that, whilſt his general concourſe maintain'd the order eſtabliſhed, each part of this great engine ſhould, without either intention, or knowledge, as regularly and conſtantly act towards the attainment of the reſpective ends he deſign'd them for, as if themſelves really underſtood, and induſtriouſly proſecuted thoſe ends. Thus, in a well-made clock, the ſpring, the wheels, the balance, *&c.* tho' each of them act according to the impulſe it receives, and the determination that is given it by the other parts of the engine, without knowing what the neighbouring parts, or what themſelves perform; yet their tendencies are ſo determined, and over-ruled, and their motions ſo regulated, by the ſtructure of the machine, that the whole could not proceed more conveniently, nor better perform the office of a clock, if they knew they ought all to conſpire, and were deſign'd to make the index truly mark the hours. 'Tis not eaſy, indeed, to conceive, how one agent ſhould, by ſo ſimple an inſtrument as local motion, direct ſuch a multitude of others, as make up a world, to act with the ſame regularity, as if each of them went upon its own particular deſign, and yet all conſpire to obey the laws of nature. But if we conſider, that this great work is aſcribed to an omniſcient, and almighty agent, it will not appear incredible; eſpecially ſince 'tis manifeſt, that a multitude of bodies act as we have ſuppoſed: and that, if we will not aſcribe to God the direction, and ſuperintendency of the motions that are manifeſtly fitted for the attainment of ends, we muſt aſcribe them to nature; which will not leſſen, but increaſe the difficulty. And, upon viewing a great engine, wherein the works of many trades, and a great variety of other motions were perform'd by little puppets, that managed the tools of the ſeveral artificers; whilſt all of them were ſet on work by a ſingle ſpring, which communicated motions, regulated and determined by the particular ſtructure of the little ſtatues, *&c.* I could not think it impoſſible, that the great creator ſhould be able, by the motions and ſtructures of matter, to ſet very many partial, and ſubordinate, engines a-going. For 'twill not, I hope, be ſaid, that the multitude of theſe, any thing near ſurpaſſes that of thoſe which I ſaw in the hand of an illiterate tradeſman, ſo far as the narrow knowledge of that artificer is ſurpaſſed by the boundleſs underſtanding of an omniſcient artiſt. And God's wiſdom and skill is more diſplay'd in making ſo many various bodies act according to their particular deſignations, while they all conſpire to the general ends of the univerſe; than barely in cauſing bodies to act, appoſitely, for ends to themſelves unknown. For, if moving bodies be duly diſpoſed, and have a ſufficient connection, 'tis not difficult to direct a few of them to the attainment of an end propoſed by an underſtanding agent, tho' unknown to the immediate agents: as, anciently, among the *Jewiſh* husbandmen, and, at this day, in ſome parts of the eaſt, the ox, by treading the corn, ſeparates the grain from the ſtraw, as well as the labourer who thraſhes it, on purpoſe to make that ſeparation; and a horſe or an aſs, by going round in a mill, may grind the corn as well as the miller himſelf.

This doctrine, however, is not inconſiſtent with the belief of any true miracle; for it ſuppoſes the ordinary and ſettled courſe of nature to be maintain'd; without at all denying, that the moſt free and powerful author of nature is able, whenever he thinks fit, to ſuſpend, alter, or contradict thoſe laws of motion which he alone at firſt eſtabliſh'd, and which requires his perpetual concourſe to uphold.

SECT. IV.

How final cauſes are to be conſider'd.

TO come to our laſt queſtion, "With what cautions, final cauſes are to be conſider'd by the naturaliſt?" I muſt obſerve, that the caſes whereto it may relate, are ſo many and various, that I can only touch upon ſome few of them.

And to make way for what I am to offer, by a diſtinction; there are two ways of reaſoning from the final cauſes of natural things, that ought not to be confounded. For ſometimes men draw arguments from the uſe of bodies that relate to the author of nature, and the general ends he is ſuppoſed to have intended in things corporeal: as when from the manifeſt uſefulneſs of the eye and all its parts for viſion, 'tis inferr'd that the eye was originally framed by a very intelligent Being, with a particular care that animals ſhould be furniſh'd with the fitteſt organ of ſo neceſſary a ſenſe. And ſometimes, alſo, men ground arguments upon the ſuppoſed ends of things, as to the peculiar nature of the things themſelves, and conclude that this affection of a natural body or part ought to be granted, or that deny'd; becauſe by this, and not by that, or by this more than by that, the end deſign'd by nature may be beſt and moſt conveniently obtain'd. The latter ſort of arguments I uſually call purely phyſical, and thoſe of the former may be ſtiled phyſico-theological, or, by a ſhorter name, metaphyſical ones.

But in order to be the more clear upon this ſubject, I ſhall refer my thoughts of it to the five following propoſitions.

PROP. I.

As to the generality of the celeſtial bodies, it ſeems unſafe to draw arguments of their nature, on a ſuppoſition of particular ends, at leaſt of the human ones, deſign'd by God in their formation.

As to the celeſtial bodies.

I am by all means for encouraging the contemplation of the celeſtial part of the world, and the ſhining globes that adorn it, and eſpecially the ſun and moon, in order to raiſe our admiration of the ſtupendous power and wiſdom of him who was able to frame ſuch immenſe bodies; and, notwithſtanding their vaſt bulk, and ſcarce conceivable rapidity, keep them for ſo many ages conſtant both to the lines and degrees of their motion, without interfering with one another. And doubtleſs we ought to return thanks and praiſes to the divine goodneſs for having ſo placed the ſun and moon, and determin'd the former, or elſe the earth, to move in particular lines, for the good of men and other animals. And how diſadvantageous would it have been to the inhabitants of the earth, if the luminaries had moved after a different manner? I dare not, however, affirm, that the ſun, moon, and other celeſtial bodies, were made, ſolely, for the uſe of man; much leſs preſume to prove one ſyſtem of the world to be true, and another falſe; becauſe the former is better fitted to the conveniency of mankind, or the other leſs ſuited, or perhaps altogether uſeleſs to that end. Thus men ſometimes alledge, that the ſun ought to be in perpetual motion, to ſhine upon the earth; becauſe, as they fancy, 'tis more convenient for man, that thoſe diſtant bodies, than that the earth, which is his habitation, ſhould be kept in motion. But conſidering things as mere naturaliſts, it ſeems not very likely that a moſt wiſe agent ſhould have made ſuch vaſt bodies as the ſun and the fix'd ſtars, (eſpecially, if we ſuppoſe them to move with ſuch a prodigious rapidity as vulgar aſtronomers aſſign them) chiefly to illuminate a little globe, that, without an hyperbole, is but a phyſical point in compariſon of the immenſe celeſtial ſpace; while thoſe lights might as well illuminate the earth, if they were a thouſand times leſs than they are; provided they were placed at a proportionable diſtance from it. And 'twill be very hard to ſay what conſiderable uſe the terreſtrial globe or its inhabitants derive from that multitude of celeſtial ſpheres which compoſe the milky way; ſince each of thoſe ſtars is ſo far from being ſingly able to enlighten the earth, that *Ariſtotle*, and the generality of philoſophers for many ages, took the whole number of them for a meteor. And what light, or other known advantage, can the earth, or its inhabitants, receive from thoſe many fix'd ſtars that the teleſcope only can diſcover, among the ſix or ſeven conſpicuous ones of the *Pleiades*, or among thoſe which the naked eye diſcovers in the belt or girdle of *Orion**?

I foreſee, it may be ſaid, that theſe and the like celeſtial bodies are, at leaſt, thus far uſeful to men, as to diſcover to him the power and greatneſs of the divine maker. And, indeed, tho' perhaps his wiſdom appears as great to us men in the ſtructure of a glow-worm, as in the diſpoſition of the ſmall ſtars that make up the galaxy; yet the immenſity of his power could not poſſibly be ſo well declared by leſs productions. Theſe arguments, nevertheleſs, are not purely phyſical, but of that ſort which I call phyſico-theological, whoſe inferences relate to the general deſigns of God in the univerſe, which I therefore ſtile coſmical ends; but do not reach to prove any thing about the determinate nature of particular bodies. And ſince the utmoſt that philoſophy teaches, is, that in general, the good of man was one of the ends deſign'd by God in framing the world as we ſee it; there may, by the ſame omniſcient author of nature, be other ends deſign'd of thoſe teleſcopical, and other ſmall or remote ſtars, whoſe uſes to us are doubtful or inconſiderable; to attain which ends, thoſe celeſtial bodies and motions may be admirably contrived and directed. And we being unable by mere reaſon to diſcover what thoſe ends are, tho' we have nothing near ſo great cauſe to think there may not be ſuch ends, as the infinite wiſdom of God gives us to think there may; 'tis preſumptuous to judge of the ſyſtem of the world, and of the deſign of vaſtly remote fix'd ſtars, by its being greater or leſs advantageous to us: eſpecially ſince tho' it were certain, that, among other uſes, God intended they ſhould in ſome ſort be ſerviceable to us; yet he has no way declared in what capacity, or to what degree, they ſhall be ſo. And therefore, if they prove ſerviceable in any meaſure; that is, ſo far as we know, all he deſign'd they ſhould be: and that itſelf, being an unmerited favour, deſerves our humble thanks. And it ſeems very likely, that God did not deſign equal advantages to all the parts of the earth, from the preſent ſyſtem of the univerſe; ſince the countries inhabited by the *Samoids* and *Nova-Zemblans*, and other nations that lie very near the arctic pole, want many conveniencies and advantages enjoy'd by the inhabitants of the temperate zones.

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* It ſeems difficult to ſay, what advantages the earth can receive from the comets; yet late diſcoveries have led us to conjecture at their uſe: and that moſt ſagacious philoſopher, Sir *Iſaac Newton*, ſuppoſes that one final cauſe of comets, is, "to recruit the ſeas, and the moiſture "in the planets, by a condenſation of "their vapours, and exhalations there"on. For as ſeas are abſolutely neceſſa"ry to the conſtitution of our earth, to "the end that the heat of the ſun may "thence raiſe vapours in great plenty, "which gathering into clouds, fall down "in rain, and thereby water the earth, "and fit it for vegetation; or elſe being "condenſed by the cold tops of the "mountains, diſtil down in ſprings and "rivers; ſo comets ſeem neceſſary to "preſerve the waters in the planets, by "continually repairing, and ſupplying "with their exhalations and condenſed "vapours, all that liquor, which being "waſted in vegetation and putrefaction, "is

But tho' bare philoſophy does not favour this bold opinion, yet I know 'twill be pretended that revelation does. And I readily confeſs, that the terraqueous globe and its productions, eſpecially the plants and animals 'tis furniſh'd with, appear, from the ſcripture, to have been deſign'd for the uſe and benefit of man; who has, therefore, a right to employ any of them he is able to ſubdue; and that the ſun and moon were appointed by God to give light upon the earth, and be uſeful to all the nations that inhabit it: and that therefore the royal prophet had reaſon to exclaim,

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" is turn'd into dry earth upon them. " For all vegetables wholly grow from " liquids, and afterwards, in great meaſure, change into dry earth by putrefaction; and a terreſtrial part perpetually falls from putrefied fluids. " Hence the bulk of dry earth is conſtantly upon the increaſe, and the " fluids, unleſs by ſome means ſupplied, " muſt continually decreaſe, and at length " be exhauſted. But the vapours of comets being continually rarified in the " empty celeſtial ſpace, are diffuſed every " way therein, and ſpread through the " whole heavens; afterwards they are " gradually attracted by the planets, from " their principle of gravity, and probably intermix'd with their atmoſpheres. " I farther ſuſpect," ſays this great philoſopher, " that the ſpirit which is the " leaſt, the moſt ſubtile, and the beſt part " of our air, and which is neceſſary to the " life of all things, comes principally " from the comets." *Newton. Princip.* p. 472, 473.

"how manifold are thy works, O Lord! how wiſely haſt thou made them "all!" For he applies theſe expreſſions to the terraqueous globe and its inhabitants; as he elſewhere juſtly ſays, that "the heavens declare the "glory of God, and the firmament ſheweth his handy-work." But theſe general declarations, tho' they properly excite our wonder and thankfulneſs, yet I fear, are not good topics, from whence to draw ſuch phyſical concluſions in particular caſes, as ſome learned men venture upon. For I do not remember 'tis any where declared in ſcripture, that the ſervice of man was the only, or principal uſe, of all the celeſtial bodies. And this ſingle conſideration ſhould make us very cautious, how we eſtimate the great ſyſtem of the world by our conveniencies. And if it be ſaid, that man alone has a rational faculty, whereby to refer the great works of God to the glory of their maker; I anſwer, that tho' this has been affirm'd by many, yet I have never found it prov'd. And I ſomewhat wonder, that divines ſhould, on this occaſion, overlook that paſſage in *Job*, which they generally interpret of the angels. For the queſtion which God there puts to *Job*, may be juſtly applied to *Adam* himſelf; "Where waſt thou, when "I laid the foundations of the earth? declare, if thou haſt underſtanding. "When the morning-ſtars ſang together, and all the ſons of God ſhouted "for joy?" And, indeed, if we may preſume to conjecture at ſuch things, it ſeems reaſonable to me, that God created the angels before the material world, that he might have intelligent Beings to pay him the juſt tribute of praiſes for ſo admirable a ſpectacle, as that of the riſing world, or the beginning and progreſs of the creation. However, it appears from theſe words in *Job*, that before man was made, God wanted not intelligent ſpectators and applauders of his corporeal works. And ſince the angels are a nobler order of intellectual creatures than men, and no unconcern'd ſpectators at the works of God; how do we know, that in the ſyſtem of that part of the heavens which is inviſible to us without the help of teleſcopes, and in the plants, animals, or other furniture, whatever it be, of thoſe particular ſtars that ſerve us men, barely for declarations of their maker's power; ſuch intelligent ſpirits as angels may not diſcern as wiſe deſigns, and as admirable contrivances, as thoſe manifeſted in forming and furniſhing the earth? And in this caſe, God will loſe none of the glory due to the divine attributes diſplay'd in the fabric of the celeſtial part of the world, tho' the fix'd ſtars ſhould not be principally deſign'd for the ſervice of man.

But, ſecondly, 'tis yet more unſafe to form arguments upon the nature of particular inanimate bodies in the ſublunary world, from the uſes we think them deſign'd for.

And thoſe that are terreſtrial.

This will be made evident, by conſidering how little we know of the particular purpoſes of nature, in thoſe terreſtrial bodies, which being inorganical, cannot, by their curious ſtructure, diſcloſe to us the particular ends to which they were ordain'd. And their motions wanting that conſtancy and regularity of the celeſtial bodies, the caution given about drawing arguments from the aſtronomical ſyſtem, will not, ſure be thought unfit to take place in clays, chalks, ſtones, &c. whoſe textures, compared with

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with thoſe of living creatures, are very ſimple, ſlight, and ſeldom more curious than may be made artificially, by diſſolving ſtones and metals in chymical menſtrua, and afterwards cryſtallizing the ſolutions. 'Tis true revelation ſpeaks rather of God's having deſtin'd animals and vegetables, than other inanimate bodies, to the ſervice of men; yet there is no abſurdity to conceive in the general the ſame to be one of the ends deſign'd by the author of nature in making metals, ſtones, and thoſe other inanimate parts of the terreſtrial globe that man is able to maſter and make uſe of. But 'tis very unlikely that the internal part of the earth, which may, for ought we know, contain great varieties of foſſils and other creatures, ſhould be made chiefly for the ſervice of men, from whoſe ſight they lie hid, and who will, in all probability, never deſcend to a thouſandth part of the depth requiſite to diſcover them, and do not ſo much as know what kind of bodies they are. And tho' it will not hence follow, that the terraqueous globe was made by chance, any more than that the other plants were ſo, becauſe the admirable ſtructure of plants and animals proves the exiſtence and providence of a moſt wiſe and powerful author of things, who may juſtly be ſuppoſed to have made nothing in vain, even among the inanimate portions of our globe; yet that thoſe inanimate portions were made for determinate ends, is more eaſily deduced from the knowledge we have, by other means, of their being produced by a wiſe author, than from the contemplation of thoſe bodies themſelves. And perhaps it is worth inquiry, whether ſome things may not be made, even by a wiſe agent, not out of a primary intention, but as productions that will naturally follow, upon the eſtabliſhment and preſervation of thoſe grand laws and rules of motion that were moſt fit to be ſettled among things corporeal. And 'tis very poſſible, that, according to ſuch a general eſtabliſhment, many parts of the terreſtrial globe are ſo diſpoſed of, as not to be ſerviceable to men; becauſe the whole maſs could not otherwiſe be ſo well ſuited to the general ends of the univerſe. Thus, tho' the eclipſes of the ſun and moon be uſually unwelcome, and, if aſtrologers may be credited, often prejudicial to men; yet "the great former of all things" did not think fit to alter the tracts or lines of motion that he aſſign'd the luminaries, to avoid the eclipſes that muſt yearly enſue upon their moving in ſuch lines. Whence we alſo learn, that ſome phenomena may not belong to the primary intention of nature, but are only the neceſſary conſequences and effects of the primitive conſtitution of the world, and the univerſal laws of motion.

But if it be here demanded to what end the deep and hidden parts of the terraqueous globe, and the teleſcopic ſtars of the firmament, were made, if not for the uſe of man? I freely acknowledge, that I cannot tell: and perhaps ſuch an anſwer may be more expreſſive of the profound reverence we owe the great author of nature, than their opinion, who would have all theſe made for the ſole uſe of man.

PROP. II.

'Tis often allowable for a naturaliſt, from the manifeſt and appoſite uſes of the parts of animal bodies, to collect ſome of the particular ends for which the creator deſign'd them: and in ſome caſes we may, from the known nature and ſtructure of the parts, draw probable conjectures about the particular offices of them.

To obviate miſtakes, it muſt be here obſerv'd, that I ſpeak only of thoſe ends and uſes of the parts of an animal, that relate to the welfare and propagation of the animal itſelf, and which, therefore, I call animal ends; tho' I do not thereby deny any declaration made in the holy ſcriptures, that God deſign'd the entire animals, as well as their parts, to be ſerviceable many ways to man. This premiſed, I come to conſider diſtinctly the two parts of the propoſition.

And, firſt, there is no work of nature known to us, wherein the conſideration of final cauſes may ſo juſtly take place, as in the ſtructure of animal bodies. For my own part, I confeſs, that when I aſſiſt at a skilful diſſection, I cannot but wonder there ſhould be philoſophers, who aſcribe the admirable contrivance of a human body to blind chance. The *Stoic*, who in *Cicero*, ask'd an *Epicurean* why chance did not make palaces, and erect other buildings, ſeems to have propoſed a pertinent queſtion. But the moſt commodious houſes are far leſs curious ſtructures than the human machine: for the materials of a palace are few, in compariſon to the parts of an animal body; and their diſpoſition exceeding ſlight, compared to the curious and elaborate contrivance of the numerous ſolids and fluids of the human ſtructure: the former whereof, alone, are above ſome hundreds; yet in every one of theſe parts, the bulk, figure, conſiſtence, texture, ſituation, connexion, and aptneſs for motion, are the moſt commodious that can poſſibly be deviſed; whilſt all of them are wonderfully ſymmetrical, both to one another, and the whole body. And this number of parts is ſo artificially contrived and ſet together, that tho' no room be loſt, many of them, at the ſame time, exerciſe very different motions, while each moves freely, and rather promotes than hinders the motion of others.

Hurts or diſeaſes may ſhew how excellently all the parts of our bodies are contrived, in order to our welfare. For if even a finger be ſwell'd, diſplaced, kept in a wrong poſture by contractions, have its continuity violated, its tone chang'd by ſtrains or contuſions, its ſenſe or motion taken away, its membranes fretted by ſharp humours, or its motions diſorder'd by convulſions; we quickly find, how commodiouſly the parts affected were framed or diſpoſed; their natural figure, connexion, tone, &c. being now alter'd.

The eye, to ſingle out again that part for an inſtance, is ſo exquiſitely adapted for ſeeing, and ſo little fitted for almoſt any other office in the body; and that uſe is ſo neceſſary for the welfare of the animal, that it may well be doubted, whether any conſidering man can really think it not deſigned for that uſe. The ſix or ſeven muſcles, which move the whole ball of the eye upwards, downwards, to the right-hand, to the left, and to various oblique poſitions; and the ſeveral coats and humours that make up the organ, have not only their magnitude, figure, conſiſtence, ſituation, and connexion, admirably adapted to that end; but the tranſparency of the cornea, and the three humours, the opacity of the uvea, the ſemi-opacity of the retina, and the ſeveral motions of the parts of the eye, which are requiſite to receive, tranſmit, refract, and diſpoſe the viſual rays, that come from the object, after the manner neceſſary to make the livelieſt picture of it in the bottom of the eye, wonderfully conſpire to compleat this matchleſs inſtrument of viſion: whence we may as properly conclude, that an eye, as that a teleſcope, is made to view objects with. But in that admirable perforation of the uvea, which we call the pupil, nature has greatly exceeded art. For tho' we are obliged to employ opake bodies, with ſeveral circular apertures, to the object-glaſſes of teleſcopes, that ſome may let in leſs light, and others more, according as the objects require to be illumin'd; that part of the uvea which is ſuſpended in the aqueous humour, is an aperture that widens and contracts itſelf in an inſtant, according to the exigency of the object.

But for the uſes of the ſeveral parts of the eye, I refer my reader to *Scheiner*'s *Oculus*, and *Des Cartes*'s *Dioptrics*; whence it will appear, that, in forming this part, nature not only acted with deſign, but with ſo great skill in optics, that a more than ordinary acquaintance with that ſcience is neceſſary to underſtand the wiſdom of the ſeveral contrivances, which perhaps no degree of skill whatever in it would enable a man to alter for the better.

'Twere tedious to mention other parts of the body that manifeſtly appear to have been ordain'd to certain uſes. The books of anatomiſts are full of paſſages to this purpoſe, of which I ſhall only ſay in general, that tho' what they deliver is ſufficient to ſhew all the parts of the body to be the effects of an intelligent cauſe, yet unleſs their deſcriptions and reflections be improved by mathematics, mechanics, and chymiſtry, we ſhall have but an imperfect notion how intelligent that cauſe is, or how much wiſdom is diſplay'd in the ſtructure of a human body, and its ſeveral parts.

I know 'tis objected by the *Epicureans*, that the parts of animals were firſt made, and their uſes afterwards diſcover'd, by the ſagacity of men; but this is a ſophiſtical objection: for, firſt, many of the internal parts perform their functions, without our having any knowledge of their ſtructure or ſituation; ſo far are they from being applied to ſuch uſes by our ſagacity. And as for the limbs and other parts, which we move at pleaſure, 'tis true, they cannot be employ'd to their reſpective uſes, till actually form'd; nevertheleſs, they might be originally ſo form'd, as in due time to be fit for ſuch uſes. And, in effect, we ſee that a chick is furniſh'd with compleat eyes and wings, before it be hatch'd, tho' whilſt incloſed in the egg it can make no uſe of them, either to ſee or fly. And why was it, do the *Epicureans* think, that nature provided a whole ſett of temporary parts for pregnant females, and animals in the womb; which, when they are come into a freer ſtate, partly ſhrink away of themſelves, and partly turn to a ligament, fitted no longer for the former, but for a more ſeaſonable uſe? And 'tis to be noted, that theſe umbilical veſſels, and the placenta to which they are faſten'd, is of no neceſſity or uſe to the female before conception; and therefore thoſe temporary parts appear to have been deſign'd by nature for the propagation of the ſpecies.

And tho' the ſagacity of men may have found out ſome uſes of the particular parts of their bodies, which ſeem not to have been primarily intended by the author of nature; yet this does not prove that thoſe uſes were undeſign'd: for the preſcience and goodneſs of God are ſufficient to render it probable that he who gave man both the limbs of his body, and the endowments of his mind, did both foreſee what uſes men might, according to their ſagacities and emergencies, make of theſe parts, and ſo contrive the parts, that they ſhould be applicable to ſuch uſes.

And we may now proceed to the latter part of our propoſition, which aſſerts, that, in ſome caſes, from the known ends of nature, as well as from the ſtructure of the parts, probable conjectures may be form'd about the particular offices of them.

This could not have been ſeaſonably ſpoken to before, becauſe the arguments that were founded on the uſes of the parts of animals ſuppoſe thoſe parts to have been deſtin'd to particular uſes, knowable by us; and that the ſeveral parts of the body were contrived as wiſely and commodiouſly as men are able to deviſe, in order to the ends of nature; which muſt always be underſtood to have united in her deſigns the uſes of the parts, and the welfare of the whole.

And indeed if we conſider how admirable a fitneſs there is in the parts of a human body, to thoſe particular ends we can diſcover them to have been deſign'd for; it ſeems allowable to conjecture, that ſuch a part was not primarily deſign'd to ſuch an uſe, if it is, on the account of its ſtructure, or otherwiſe, leſs fitted for it than the conſtant wiſdom of nature ſeems to require; eſpecially if there be any other parts by which the office may be more commodiouſly performed. And, on the other ſide, it ſeems probable, that ſuch a part was deſtin'd to ſuch an uſe, if the uſe itſelf appear to be neceſſary, and the part better fitted for it than any other.

Thus, tho' anatomical and optical writers, for many ages, unanimouſly concluded the cryſtalline humour to be the principal ſeat of viſion, yet the induſtrious *Scheiner* juſtly rejects that receiv'd opinion, upon ſhewing, that it ſuits not with the skill and providence of nature, that it ſhould be ſo, ſince it wants many requiſite qualifications for that purpoſe; and eſpecially ſince moſt of theſe are to be found in the retina. And I remember, upon asking our famous *Harvey* what induced him to think of a circulation of the blood; he ſaid, that obſerving the valves in the veins of many parts of the body, ſo placed, as to give free paſſage to the blood towards the heart; but to oppoſe the paſſage of the venal blood, the contrary way; he

PHYSICS. imagin'd that ſo provident a cauſe, as nature, had not thus placed ſo many valves without deſign: and as no deſign ſeem'd more probable than that, ſince the blood could not well, becauſe of the interpoſing valves, be ſent by the veins, to the limbs, it ſhould be ſent thro' the arteries, and return through the veins, whoſe valves did not oppoſe its courſe that way.

Thus, tho' the ancient anatomiſts, and phyſicians, believ'd the parts were nouriſhed by the venal blood; the modern writers teach them to be nouriſhed by the blood, in its paſſage thro' the arteries. Not that they think the blood, which runs thro' the veins, altogether unfit to ſupply the parts with that vital liquor; but becauſe they judge the veins to be leſs fit for this purpoſe, than the arteries; into the latter whereof the blood comes immediately from the left ventricle of the heart, agitated, and ſpirituous, and, by a briſk impulſe, better ſuited to anſwer this end.

But the writings of phyſicians, and anatomiſts, being filled with inſtances of this kind, I forbear to mention any more.

PROP. III.

It is rational, from the manifeſt fitneſs of ſome things, to coſmical, or animal ends, to infer, that they were thereto ordained by an intelligent agent.

Care ſeems to have been taken, that the body of an animal ſhould be provided not only with all things that are ordinarily neceſſary, and convenient: but with ſome ſuper-abundant proviſion againſt accidents. Thus, tho' a man may live, and propagate his ſpecies, after the loſs of an eye; yet nature furniſhes us with two, that, in caſe one be deſtroy'd, the other may ſuffice for viſion. And the like may be ſaid of the ears. In ſhort, nature has furniſh'd men with double parts of the ſame kind, where it is highly uſeful, and may be permitted without prejudice, to the reſt of the body. And this is the more conſiderable, becauſe, in other parts, nature appears to husband things ſo, as to avoid doing what is ſuperfluous. Thus, within the skull, ſome veſſels, that would, in other parts of the body, have double coats, are very thin; the skull being ordinarily ſufficient to defend them from external injuries.

Another argument, that nature acts with deſign, about animals, may be drawn from what anatomiſts obſerve of thoſe parts of the womb, or the fœtus, that are to be found but at certain times, when there is need of them; and not at others, when they would be uſeleſs. Thus, when a woman is with child, the *Vaſa umbilicalia* are produced to be canals, either for the blood, or alimental juice, and ſpirits, that then ought to paſs between the womb and the fœtus, by means of the placenta. And tho', as long as the child continues in the womb, theſe temporary parts continue with him; yet, as ſoon as he comes into the world, the umbilical veſſels, particularly the two arteries, and the vein, together with the membranes they are wrapp'd up in, with the chorion, and the amnios, that involve the fœtus, are thrown off, as unneceſſary, and expell'd in the after-birth; there remaining only that part of the umbilical veſſels that lies within the child's abdomen, between the navel and the liver; where its uſe is conſiderable, tho' new; for it ſerves now no longer to convey blood, or an alimental liquor, to and fro, but degenerates into a ligament.

Thus, alſo, the *Foramen ovale*, gives paſſage to the blood from the right ventricle of the heart to the left, that the circulation of it may be maintain'd; for it cannot in the embryo, as in a born child, paſs thro' the veſſels of the lungs, from one of the ventricles to the other: whence this contrivance ſeems to be an expedient that nature employs, till the fœtus is excluded; when that temporary conformation is obliterated. For the child, now breathing the free air, is in a condition to make the blood circulate thro' the pulmonic veſſels, according to the primary intention of nature. From which, and the like inſtances, we may infer, that theſe temporary parts were framed by a fore-knowing, as well as a deſigning agent; who intended they ſhould ſerve for ſuch a purpoſe, and then be laid aſide: for 'tis utterly improbable, that an undeſigning agent ſhould ſo appoſitely, and exquiſitely, frame ſcaffolds for a future building, if he did not, before-hand, deſtine both the one and the other, to concur to the ſame ultimate effect.

Another argument, for our preſent purpoſe, may be drawn from the conſideration of what, in animals, is commonly call'd inſtinct: which, in ſome caſes, more directly regards the welfare of the creature; in others, the propagation of the ſpecies; and, ſometimes again, reſpects both. The writers of voyages, and natural hiſtory, recount ſtrange inſtances of the inſtinct obſervable in certain animals. But we need not lay the ſtreſs of our argument upon dubious, or ſuſpected relations; ſince what I have met with, in authors of good authority, or receiv'd from the mouths of credible travellers, may ſerve my preſent turn; eſpecially, if we may take the word inſtinct in a latitude, ſo as to comprize thoſe untaught methods, and expedients, that are made uſe of by ſome animals, to avoid dangers, provide for their future neceſſities, or to catch their prey.

Surprizing things are related, not only by poets, but by more credible writers, about the ſagacity and government of bees; in point, both of œconomy, and politics. But tho' I ſhall not build any thing upon ſuſpected authorities, yet, having long kept a tranſparent hive, and thereby gained the opportunity to make frequent obſervations of the actions of theſe little animals; I confeſs, I diſcover'd ſome things that I did not believe before: which induced me to look upon them, as very fit inſtances of creatures endow'd with natural inſtinct and providence. For, 'twere hard for a mathematician, in contriving ſo many cells as bees make in the area of one of their combs, to husband ſo little ſpace more skilfully than theſe animals. And they not only carefully, and ſeaſonably, lay up their honey, to ſerve them all the winter; but, curiouſly, cloſe the particular cells with covers of wax, that keep the included liquor from ſpilling, and from external injuries. I do not here mention the prognoſtication of weather that may be made in the morning, by their keeping within their hives, or flying early abroad, to furniſh themſelves with wax, or honey; or, by their unexpected return before a ſtorm: becauſe I ſuſpect that theſe things are not ſo much the effects of inſtinct, as of a tenderneſs, and quickneſs of ſenſe; ſomething analogous whereto may be ſeen in a good weather-glaſs, and is alſo to be found in many wounded and valetudinary perſons, who are affected with ſuch beginning alterations in the air, as other men perceive not. But among the peculiarities to be obſerv'd in the conduct of bees, 'tis very remarkable, that, after a fight, they take up the dead which lay on the ground, and, as I have obſerv'd, fly away with them far from their hive.

Another obvious inſtance of the inſtinct that nature has given to ſome deſpicable inſects, may be taken from ants: for 'tis known, that theſe little creatures do, in the ſummer, hoard up grains of corn againſt the winter. And their ſagacity is the more conſiderable, if it be true what many learned men affirm, that they eat off the germen of the grains they lay up, leſt the moiſture of the earth, expoſed to the rains, ſhould make them ſprout. But whatever become of this tradition, theſe inſects perform ſome other actions greatly reſembling thoſe proceeding from ſagacity and induſtry.

The natural skill of ſpiders in weaving their webs, that are ſo fitly contrived, both to catch their prey, and give them immediate notice of its being caught, is a thing, which, if it were not familiar, would be look'd upon as admirable. And this skill is not, as ſome imagine, an effect of imitating their parents; for if the eggs be taken away, and encloſed in a glaſs, when they come to be hatch'd by the heat of the ſun, the little creatures will immediately fall to ſpinning in the glaſs itſelf, as was related to me by an eminent mathematician, who made the experiment. And I ſaw the leſs reaſon to diſtruſt it, becauſe having, by an external heat, hatch'd many eggs of ſilk-worms, in a place where there had not been any of a long time before, nor probably ever, till then; yet the worms produced by theſe eggs did in autumn, of their own accord, climb up to ſuch convenient places as I had prepared for them, and there weave thoſe curious oval priſons wherein they encloſe themſelves, and which are unravell'd into extremely fine ſilk.

But this provident induſtry is not confined to inſects, for 'tis to be found in many of the greater animals, particularly in the beaver; a creature whereof, indeed, many fabulous ſtories are related: yet ſober and judicious perſons, who were either born, or lived in *New-England*, where theſe animals abound, have aſſured me, that the beavers, with their ſharp teeth, cut pieces of wood, and fit them to their purpoſe; that, by joining their labours, they lay theſe together, ſo as to build themſelves ſtrong winter-houſes; in which there is ſometimes a kind of ſecond ſtory for the inhabitants to retire to, when the water chances to overflow; that, for theſe houſes, they chuſe a very convenient ſituation, juſt by ſome river, or other water, that can furniſh them with fiſh; and that the hole belonging to each houſe is placed juſt by the water, that they may immediately flounce into it, and ſo ſave themſelves, when their houſes are attack'd. And, to facilitate their ſwimming, and the catching of their prey in the water, nature has given them two feet, not made like thoſe of dogs or cats, or in the manner of their other two, but furniſh'd with broad membranes betwixt their toes, like the feet of geeſe, ducks, and other aquatic animals, that are to uſe them as oars, to thruſt away the water, and facilitate their motions.

The various arts employ'd by animals of different kinds, about the materials, the conſtruction, and the ſituation of their neſts, is uſually remarkable, and ſometimes wonderful. Of this skill we have many eminent inſtances, but 'tis particularly remarkable, that in countries abounding with apes and monkeys, creatures very greedy of birds eggs, there is a ſort of bird whoſe eggs they peculiarly affect, that hang their neſts near the end of ſome long flexible branches which reach over the water, and by that means avoid their enemies, who cannot ſwim.

In the neſts of waſps, which they often, for greater ſecurity, make under ground, I have obſerv'd a very curious and artificial ſtructure, to conceal and ſhelter their young ones till they are ready to fly.

But the inſtinct that nature has planted in animals for their own preſervation, is much inferior to that providence ſhe has furniſhed them with for the propagation of their ſpecies.

There are many remarkable things to be met with in the neſts of ſeveral birds, both as to the materials, the ſtructure, and the ſituation of the places wherein they are built. I have ſeen neſts, and particularly ſome made in the *Indies*, which would raiſe a man's wonder how the birds ſhould find ſuch odd, but commodious materials to build with. There are birds in the eaſt, which make their neſts of a white ſubſtance, that looks almoſt like iſing-glaſs, diſſoluble in liquors, and ſo very well taſted, that it makes the chief ſauce uſed in the ſouthern parts of *India*. The ſtructure alſo of the neſts of ſeveral birds, both as to their figure, magnitude, and accommodations, wherewith they are furniſh'd, for warmth and ſoftneſs, may deſerve the applauſe of mathematicians, eſpecially if it be conſider'd that theſe little untaught architects had no tools to make their curious buildings with, except their bills and feet. Much more fore-ſight, however, appears in the ſituation of the place, that ſome birds make choice of to build their neſts in; as may be obſerv'd, not only in the pendulous neſts of ſwallows, and the ſecret ones of ſome *European* birds; but very conſpicuouſly in the hanging neſts that we lately mention'd, to be ſo oddly placed by ſome birds, to ſecure their eggs from apes and monkeys; and by the ſituation of the neſts uſed for ſauce as we juſt now related, to be found only upon high and ſteep rocks; which are ſo faſten'd to the concave parts of them, that look downwards, and commonly hang directly over the ſea; that there is no getting them, without much trouble and danger: upon which account, as well as that of their delicacy, they are very dear, even in the *Eaſt-Indies*, where they are found.

The like care to contrive their neſts advantageouſly, and make them in ſecure places, is viſible in inſects; as may be obſerv'd in the ſubterraneal neſts of the waſps, formerly mention'd, and in the eggs of ſnails, which I have ſometimes found hid under-ground; and have hatch'd in glaſſes furniſhed with the ſame earth wherein they were found.

If I ſhould here ſet to view the ſeveral effects and arguments of the wonderful providence, that the moſt wiſe author of nature exerciſes about the propagation of animals, by diſtinguiſhing them into male and female; by furniſhing both ſexes with mutual appetites and organs exquiſitely adapted to the increaſe of their kind; by the admirable formation of the fœtus in the womb, without the female's knowing how it is perform'd; by the ſtrange ſubtilties and courage, that ſeveral creatures, either oviparous or viviparous, have, to hide and defend their young; by the full proviſion that is made for the nouriſhment of the fœtus, and the welfare of the female after ſhe has brought forth; and the like: I might much enrich and adorn my argument. But inſtead of purſuing a ſpeculation, that would lead me too far, I ſhall look back upon the intimation I lately gave, that even thoſe meaner parts of animals, which ſeem to have been framed with the leaſt care and contrivance, are yet worthy of their author. For tho' the teeth be ſome of the leaſt elaborate parts of the human body, yet even theſe afford numerous phenomena applicable to our preſent purpoſe: but I ſhall only tranſiently conſider a few of them.

And firſt, 'tis remarkable, that tho' when a man comes to his full ſtature, all the other bones of the body ceaſe to grow, the teeth continue to increaſe, in length, during his whole life. This growth of the teeth appears not only by their continuing for many years of the ſame length, but by the unſightly ſhooting of one tooth, when that which was oppoſite to it in the other jaw is wanting; whence it has liberty to ſprout without oppoſition. Of this difference in point of growth betwixt the teeth and other bones of the body, what reaſon can be ſo properly aſſign'd, as its being intended to repair the daily waſte of ſubſtance, proceeding from the frequent attritions of the upper and lower jaw in maſtication?

2. Tho' the bones of the body are generally inveſted with a very thin ſenſible membrane call'd *Perioſteum*; that part of each tooth, which is not cover'd by the gums, wants it; which would be ſubject to very frequent and painful compreſſions and lacerations.

3. To enable the teeth to break and make comminutions of the more ſolid kind of aliment, nature has providentially framed them of a cloſer and harder ſubſtance than almoſt any other bones in the body; tho' theſe are ſo numerous, that anatomiſts reckon above three hundred of them. And I have met with it in authors of good credit, that ſome mens teeth have been ſo hard, as, when ſtruck againſt another fit body, to produce ſparks of fire.

4. That theſe bones, whoſe uſe is ſo great, may themſelves be continually nouriſh'd and ſupplied; tho' they are ſet in bone, the wiſe author of things has admirably contriv'd a cavity on each ſide of the jaw-bone,

wherein are lodg'd an artery, a vein, and a nerve; which, thro' leſſer cavities, ſend their twigs to each particular tooth.

5. As infants are deſign'd by nature to feed for a conſiderable time on milk, they remain long without teeth; whilſt ſeveral animals, which are often early reduced to ſeek aliment that is neither fluid nor ſoft, come into the world with teeth already form'd in their jaws.

6. The bony ſubſtance appointed for the comminution of the aliment, ought not, for ſeveral reaſons, to be in either jaw entire, or all of one piece; and therefore nature has providently made for that uſe a competent number of diſtinct bones in either jaw. And becauſe men may have occaſion to feed often upon very different ſorts of aliment, and becauſe uſually the ſame aliment may require different preparations in the mouth, to facilitate the digeſtion of it in the ſtomach; nature has provided men with two rows of teeth, for the moſt part equal in number, and correſponding to each other, yet of different ſhapes, for different uſes; the fore-teeth are broader, and have a kind of edge, to cut the more yielding ſort of aliment, whence they are call'd *Inciſores*; others being ſtronger, and more fitly ſhaped to tear the tougher food, are term'd *Canini*; there is alſo a third ſort, whoſe principal office it is to grind the aliment cut or torn by the others: and for this purpoſe they are made much broader, and ſomewhat flattiſh, with their upper ſurfaces uneven and rugged, that by their knobs and little cavities they may the better retain, grind, comminute, and mix the aliment; and for this reaſon they are call'd *Molares*.

7. And becauſe the operations to be perform'd by the teeth often require a conſiderable firmneſs and ſtrength, partly in the teeth themſelves, and partly in the inſtruments that move the jaw, wherein the lower ſett of them is fix'd; nature has provided the lower, or moveable jaw, with ſtrong muſcles, that it may bear forcibly againſt the upper; and has not only placed each tooth in a diſtinct cavity of the jaw-bone, as it were in a cloſe, ſtrong, and deep ſocket; but furniſh'd the ſeveral ſorts of teeth with holdfaſts ſuitable to the ſtreſs, that by reaſon of their different offices is to be laid on them. And therefore the *Inciſores* and *Canini* have uſually but one root, whilſt the *Molares*, that on many occaſions are employ'd to break hard bodies before they can be ground, are furniſhed with three roots, and often with four, in the upper jaw, whoſe ſubſtance is ſomewhat ſofter, and whoſe grinders ſerve as ſo many little anvils for thoſe of the lower to ſtrike or preſs againſt.

If it ſhall be ſaid, that allowing tho' man is indeed advantageouſly furniſh'd with teeth, yet there are many other animals, ſome whereof have not near ſo many teeth, nor ſo commodiouſly ſhaped and placed as his; others, that are not furniſh'd with any teeth at all; and that ſeveral animals have ſome of their other parts leſs convenient in their kind, or are not otherwiſe near ſo well provided for as they would be, if they were not rather the productions of chance, than of an intelligent and deſigning agent: I muſt declare, that many particulars might be farther alledg'd, to ſhew the final cauſes of things corporeal, as the conſideration of them leads to a high veneration of their divine author; and to manifeſt, that when his providence is deny'd or condemn'd, 'tis for want of due conſideration.

But I have already, in great meaſure, prevented myſelf in the anſwering this objection, by obviating ſome exceptions relating to the eyes of man and other animals. For the conſiderations that have been alledg'd to that purpoſe, may, *mutatis mutandis*, be applied to the varieties to be found in the teeth, and other parts, of different kinds of creatures.

'Tis known, that oxen, ſheep, *&c.* are not furniſh'd with near ſo many teeth as men, horſes, dogs, *&c.* But this defect, if it be any, is ſupplied partly by the power and inſtinct they have to chew the cud, and thereby make a ſecond attrition of the aliment, that is already greatly ſoften'd; and partly by the ſucceſſive cavities, or ſtomachs, thro' which that aliment is tranſmitted, and by that means elaborated and fitted for further uſes. The beaver, the tortoiſe, the bee, and the humming-bird, which ſucks the exudations of flowers with his little long bill, like the bee, and many other animals, have their mouths, and their ways of preparing their aliment for the ſtomach, very different from what is obſerv'd in men; and yet each reſpectively very convenient; all circumſtances conſider'd.

Theſe, and the like expedients, are in many animals ſuch as afford no cauſe of taxing the author of nature, for not having given ſome of them all the ſame parts wherewith others are furniſh'd; but rather, the providence and wiſdom of God, in the contrivance of his viſible works, may be as well diſcover'd by the ſeeming omiſſion of this or that part, uſeful to other animals, but unneceſſary to thoſe wherein it is not found; as by granting thoſe parts to ſuch animals, whoſe compleatneſs or welfare they are neceſſary or highly conducive to: and therefore 'tis not ſtrange, that he has not given to men, and many ſorts of birds, ſuch tough, tranſparent, and moveable membranes, as frogs are provided with, to defend their eyes from ſuch accidents as the others are not uſually expoſed to.

Bats are eſteem'd a contemptible ſort of creature, yet they may afford us a conſiderable argument to our preſent purpoſe. For we have here an animal that flies like a bird, tho' it wants feathers, and has a fabric quite different from that of birds. And here too, we may obſerve the compenſation that is made for parts that ſeem either deficient, or leſs advantageous, than thoſe of the ſame denomination in other birds; as alſo the regard which the divine artiſt appears to have to the ſymmetry of parts in his animated works, and to their fitneſs to the places they are to frequent. For the bat being to act ſometimes like a bird, and on ſome occaſions like a terreſtrial animal, or mouſe, he ought to be furniſh'd with parts ſuitable to ſuch different deſigns: and, therefore, the want of feathers in his wings is ſupplied with a broad membranous expanſion, and a kind of toes furniſh'd with articulations. And becauſe this animal was to forbear ſettling on the ground, otherwiſe than his occaſions required, each of his wings is furniſh'd with a ſtrong crook, like the claw of a bird's foot; by the help of which, he can faſten to trees, walls, *&c.* and there keep himſelf at what diſtance he pleaſes from the ground. And as he is furniſh'd with teeth, which other birds want, to chew his food, and thereby prepare it for digeſtion; he needs no crop, or ſuch a ſtrong muſcular ſtomach, as is uſually found in birds.

In ſhort, to omit the peculiar ſtructure of other internal parts, wherein the bat differs from other birds; ſince the female was not, like them, to be oviparous; but, like mice, and other quadrupeds, to bring forth her young ones alive; ſhe is not only furniſhed with an uterus, fitted for that purpoſe; but, in regard ſhe does not exclude, together with the fœtus, a competent ſtock of aliment, to nouriſh it, till it can ſhift for it ſelf; the bat is furniſhed with teats to give ſuck: and 'tis obſerv'd, that as ſhe has but two of theſe, ſo ſhe brings forth but two young ones at a time; while mice are much more prolific.

The writers of natural hiſtory, and travellers, afford numerous inſtances of the various, and excellent contrivances, that are to be found in animals differing from man, in the fabric of the mouth, and other parts, ſubſervient to the preparation and digeſtion of their aliment.

But, to be ſilent, as to the mouths of tortoiſes, camelions, *&c.* the hardneſs of whoſe gums, with regard to their aliment, ſupplies the want of teeth, there is an *American* beaſt, which ſerves as a notable inſtance, to manifeſt how the wiſe architect can compenſate the want of teeth, by the rare ſtructure of the mouth and tongue; and their fitneſs to ſeize, and make uſe of that aliment, which, tho' uncommon for a beaſt of his bulk, he ſeems to have been deſtined to live upon. The animals of this kind are, by *Hernandus*, call'd, *Achoas*, and *Tamendoas*; and, of theſe, he ſays, "they are quadrupeds, about the ſize of lambs, whoſe bellies almoſt "touch the ground; that they have no teeth at all, but live upon ants; "whoſe hillocks they turn up with the two large claws they have fixed "to their fore-feet, and then thruſting out their long rough, cylindrical "tongue, they roll it upon as many as they can, and ſo draw them into "their mouth, which is exceeding narrow." *Piſo* adds, that "their "tongue, which is ſometimes two feet in length, lies doubled up in a "cavity, between the ſides of the lower jaws; which, in order to catch "their prey, they thruſt into the trunks of trees, and there ſuffer it to "continue, with a moiſture upon it, till cover'd with ants; and then ſud"denly draw them with it into their mouths."

'Tis alſo to be conſider'd, that many things may be uſeful, in an organical part, beſides thoſe whereby its office is primarily and chiefly perform'd. Thus, tho' the eye-lids, and their motions, together with the little glands that belong to them, are not at all neceſſary to the act of viſion; yet they are to the compleatneſs, and welfare of the eye: as is manifeſt by the pain and prejudice that organ receives, if the eye-lids be conſiderably diſorder'd.

But, beſides thoſe uſes of the parts of a human body, which I call anatomical, becauſe they are ſuch as anatomiſts have diſcover'd, by mere diſſection; there may be ſeveral others, which I term chymical, becauſe theſe parts prepare ſpirits of ſeveral ſorts, and, perhaps, perform other operations, very important, if not neceſſary, to the welfare of a living man.

And there may ſtill be other uſes, very proper to be conſider'd, in ſome parts of a human body; as the mechanical advantages for which the various ſhapes and ſtructures of different muſcles, the ſeeming irregular fabric of the bones, and eſpecially of the proceſſes and protuberances, are admirably fitted; and in ſome parts too there are peculiar ones: as for inſtance, optical ones in the eyes, which muſt be conſider'd, before the deſign of nature therein can, without raſhneſs, be cenſured.

Thus tho' the figure of the cryſtalline humour be much more ſpherical in moſt fiſh than in men, and other terreſtrial animals; yet he who underſtands the doctrine of refractions, and conſiders that fiſh under water are to ſee thro' a far thicker medium than air, will readily acknowledge that this difference between their eyes, and thoſe of men, is not an imperfection in the former, but, whilſt thoſe creatures are in their own element, a great advantage.

And, to be ſhort, I think there are ſo many ſciences, and other parts of knowledge, ſome of them perhaps ſcarce yet diſcover'd, that may be requiſite to warrant a man in cenſuring the ends of God, in the bodies of animals, that very few men are qualified to condemn them juſtly; and thoſe who have knowledge enough to judge right, will not be forward to condemn, but admire them. Thus men may eaſily be too raſh, in thinking a part inartificially framed, upon ſuppoſition that by the anatomical diſſection of it they know all the uſes whereto the skill of the divine artiſt could deſign it.

And it will not neceſſarily follow, that becauſe in ſome particular bird, or beaſt, or fiſh, we are unable to ſay why this or that part is not to be found, or why it is otherwiſe framed or ſituated than that which is analogous thereto in man; it muſt therefore be caſually or undeſignedly framed, or placed; ſince we cannot expect, from brute animals, anſwers to thoſe proper queſtions about their own bodies, which we can receive from men about theirs. Nay, notwithſtanding the great diligence with which the more curious phyſicians are obliged to cultivate anatomy, and the frequent opportunities they have to do it, and to inquire of living men about what they obſerve, and feel, when the natural uſe of their parts is hinder'd or perverted; yet we are to this day at a loſs as to the true uſes of the viſible parts of the body, to ſay nothing of the inviſible ones. So that 'tis no wonder if in animals, whoſe fabric we have much leſs concern to inquire into, and much leſs opportunity to examine, we ſometimes find parts, of whoſe uſes and fitneſs men are hitherto unable to give a ſatisfactory account. For even in man himſelf, tho' there be numerous valves found in his veins, yet for thoſe many ages that the true uſes of them were unknown, an *Epicurean* phyſician might have thought he had reaſon to look upon them as ſuperfluous parts; which, ſince the circulation of the blood is diſcover'd, are acknowledg'd of the greateſt utility.

PHYSICS.

And, ſince God is both a moſt free, and a moſt wiſe agent, it need not ſeem ſtrange that he ſhould adorn ſome animals with parts or qualities that are not neceſſary to their welfare, but that ſeem deſign'd for their beauty; ſuch as the diſpoſition of the camelion to change his colour, and the lovely green, blue, yellow, and other vivid colours, that adorn ſome ſorts of pigeons, parrots, and the ſmaller birds, and eſpecially that admirable little creature the humming-bird.

On the other ſide, God's wiſdom ſometimes ſeems to be provident, and ſollicitous, not to beſtow on an animal, or a part of it, more than is neceſſary for the uſe whereto either is deſign'd. Thus the veins are by anatomiſts obſerv'd to have but one coat, and uſually to lie more expoſed than the arteries that accompany them, which have ſtronger, and double coats, becauſe they are to convey a more important liquor, which liquor, beſides that 'tis more agitated and ſpirituous, is forcibly impell'd into them, by the ſtrong muſcular contraction of the heart. And 'tis obſervable, that tho' the nerves uſually lie deep, to be kept both ſafe and warm, being very liable to be offended both by cold and the contact of external bodies; yet as 'tis neceſſary that the optic nerve ſhould expand itſelf into the eye, the membranes which inveſt that nerve, and other coats of the eye, except the retina, are made vaſtly more firm than the *Dura*, and the *Pia mater*, whence they proceed; and tho' expoſed to the free air, are leſs ſenſible of the cold than moſt parts of the body, and will bear, without danger, ſeveral liquors, and other offenſive things, whoſe pungency would put other nerves of the body into convulſive motions.

This conduct makes it ſeem as if God, like an excellent writing-maſter, did in the great volume of his creatures intend to beſtow on ſome of them things rather ornamental than neceſſary, as flouriſhes on the capital letters of nature; and ſometimes to employ characters of different ſhapes, to ſtand for the ſame letters; and ſometimes, alſo, to employ abbreviations, to expreſs, in ſhort, that which might be very juſtifiable, had it been more fully delineated.

And if we allow, as 'tis very probable, that God deſign'd, by the great variety of his works, to diſplay to his intelligent creatures the great abundance of his wiſdom; 'tis obvious that much of the variety obſervable in the analogous parts of animals, may be very conducive to ſo comprehenſive a view; whereto the beauty of ſome creatures, and parts, as well as their more neceſſary or convenient ſtructure, may be ſubſervient; eſpecially if the innocent delight of man be alſo intended, as it ſeems to be, in the curious colours and ſhapes of ſeveral flowers, in the melodious muſic of birds, and in the vivid and curiouſly variegated colours of the feathers of ſeveral winged animals; particularly thoſe that make up the peacock's train.

'Tis worth our obſerving, that we are not near ſuch competent judges of wiſdom, as of juſtice and veracity, which are eſtimated by eternal and fixed bounds, or rules, very intelligible to a moderate underſtanding. But as for wiſdom, the more profound it is, the leſs we are able to look thro'

thro' it, penetrate to the bottom of it, and judge knowingly of its actions. And therefore, tho' we may ſafely conclude, that God acts wiſely, when he does any thing that has an admirable tendency to thoſe ends, we juſtly ſuppoſe him to have deſign'd; yet we cannot ſafely conclude in the negative, that this, or that is unwiſe, becauſe we do not diſcern a wiſe tendency therein. For ſo wiſe an agent may have other deſigns than we know of, and further aims than we can diſcern, or ſuſpect; and may have at hand, or furniſh himſelf with ſuch means to compaſs his ends, and that even by the co-operation of what we think uſeleſs, or improper, as are far above the reach of our conjectures; and without the knowledge of which, we raſhly cenſure the wiſdom of his proceedings.

In the double horizontal dial, formerly mention'd, it would be raſh of any to condemn, or deſpiſe, the various lines they find traced upon that uſeful inſtrument, becauſe they ſee they are not neceſſary to ſhew the hour of the day; ſince the mathematician, who drew thoſe lines, may be well ſuppoſed to have more ends than one, or two, in making the inſtrument; and not to have drawn them by chance, or unskilfully, tho' the inconſiderate cenſurers do not know for what other, or farther purpoſes, the artiſt may have deſign'd them.

Suppoſe ſome *Indian* fiſher-man, unacquainted with *European* arts and affairs, ſhould come on board a man of war, under ſail; he would quickly perceive, by the uſe made of the tackling, that this floating building was very artificially contrived; yet, if he ſhould fix his eyes upon one of the guns, or the anchors, and perceive that no uſe was made, or likely to be made of them, in ſailing, he would be ſtrongly tempted to think, that thoſe heavy maſſes were uſeleſs clogs, and burdens to the veſſel. But if he were told the neceſſity, and uſefulneſs of the guns, for defence; and of the anchors, to ſtay the ſhip in ſtorms; he would preſently alter his mind, and confeſs, that he had blamed the contrivers for that, which nothing but his ignorance kept him from commending.

I have dwelt the longer on this third propoſition, becauſe I think it a duty, that our reaſon owes to its author, to endeavour to vindicate his manifold wiſdom, in this libertine age; wherein too many, who have more wit than philoſophy, labour upon *Epicurean*, and ſome even upon *Carteſian* principles, to depreciate the wiſdom of God; whilſt others preſume to cenſure his contrivances, in the bodies of animals.

I had alſo a deſire, that the reader ſhould not barely obſerve the wiſdom of God, but actually be convinc'd of it. To which purpoſe, in my opinion, 'tis very conducive, if not neceſſary, beſides general notions, to obſerve, with attention, ſome particular inſtances of the divine skill, wherein it is conſpicuouſly diſplay'd. 'Tis true, that in the idea of a Being infinitely perfect, boundleſs wiſdom is one of the attributes included: but, for my part, I am of opinion, that this general and indefinite idea of the divine wiſdom, will not give us ſo great a veneration for it, as may be produced in our minds, by knowing, and conſidering the admirable contrivances of the particular productions thereof; and exquiſite fitneſs for thoſe ends and uſes, to which they appear to have been deſign'd.

PROP. IV.

We ought not to be haſty in concluding upon the particular uſe of a thing, or the motive which induced the author of nature to frame it in a peculiar manner.

It has been already ſhewn, that ſome parts are ſo excellently, and manifeſtly fitted for a certain uſe, and ſo much better adapted to that, than to any other; as to make it ſeem perverſe, to doubt of its being deſign'd thereto. But the like cannot be ſaid, in general, of all the parts of the body; eſpecially of the internal. There are many uſes, either neceſſary, or highly conducive to the welfare of the animal, that have no part ſo much more conſpicuouſly fitted for them than another, as to leave it eaſy to determine the true, and primary offices thereof; eſpecially, with ſo much certainty, as thereon to ground philoſophical inferences. And, of this difficulty, I conceive, there may be four reaſons; tho' they do not all occur in each particular caſe.

And, firſt, the whole animal itſelf, that we conſider, is a part of the univerſe; and, therefore, cannot well be ſuppoſed to have been framed, and furniſh'd with the parts it conſiſts of, merely for its own ſake. And, when we ſay, that all its parts are contriv'd to the beſt advantage to the animal; I conceive, this is to be ſo underſtood, in a limited ſenſe; that the parts are excellently framed for the welfare of the animal, as far as that welfare is conſiſtent with the general ends of the author of nature, in the conſtitution, and government of the univerſe; which ends, becauſe they relate to the whole world, I call coſmical. It has not, indeed, been prov'd, that none of theſe coſmical ends are diſcoverable by us: however, to diſcover them all, is no eaſy task. Yet, it ſeems preſumptuous to ſuppoſe, that the welfare of particular animals is any farther deſign'd, and provided for, than will conſiſt with the coſmical ends of the univerſe, and the courſe of God's general providence; to which his particular providence ought, in reaſon, to be ſubordinate. And, tho' it ſeems great raſhneſs in men, to determine poſitively, and excluſively of others, what ends the omniſcient creator propoſed to himſelf, in giving to the world its preſent frame; yet, as far as I can hitherto diſcern, I ſee nothing more likely to have been one grand motive of ſo great a variety, as we obſerve in his corporeal works, eſpecially in animals, than that he might, by ſo many very different contrivances as are to be met with in the ſtructure of men, quadrupeds, birds, fiſh, reptils, *&c.* exerciſe and diſplay his manifold wiſdom. As man, even upon account of his body, is acknowledg'd the moſt perfect of animals; if God had barely deſign'd to give every creature the moſt advantageous ſtructure, it ſeems, that he ſhould have made no other animals than men. But, then, there could not have been that diverſity of contrivance among living automata, which ſo greatly recommends

mends the wiſdom of him who could frame ſo many and ſo differing animals, tho' not all equally perfect, yet all admirably furniſh'd for thoſe purpoſes to which he deſign'd them. It does not therefore argue any want of providence, that he has not furniſh'd man with wings, fiſh with feet, and birds with fins and ſcales; becauſe theſe parts would have been either ſuperfluous, or burdenſome, or unſuitable to his deſign of fitting ſome animals to live on the earth, and others in water: and if he deſign'd any to inhabit both, he furniſhes them with parts of a peculiar ſtructure, as was formerly noted of the beaver. Were it not for this conſideration, it would be hard to aſſign the reaſon why vegetables are not the food of all animals, but that ſome ſhould be carnivorous, and furniſh'd with appetites and organs to devour others, and live, as birds and beaſts of prey, upon the weaker. And 'twill be hard to ſhew why even in animals of the ſame kind, the ſafety of ſome ſhould be ſo much better provided for than that of others; as we ſee ſome ants and glow-worms furniſh'd with wings, and others not. And even in our own ſpecies, thoſe of the female ſex are not ſo happily framed, in order to their own welfare, as males; ſince the uterus, *&c.* which are not neceſſary to the good of individuals, but to the propagation of the ſpecies, ſubject that tender ſex to a ſett of diſeaſes peculiar to them. So that men may ſometimes miſtake, when they peremptorily conclude that this or that part of an animal muſt or cannot have been framed for ſuch a particular uſe, without conſidering the coſmical, primary, and over-ruling ends, that may have been deſign'd by nature in the conſtruction of the whole animal.

2*dly*, Men ſometimes erroneouſly conclude, that ſuch an office cannot belong to ſuch a part, becauſe they think it is not ſo commodiouſly framed for it, as might be wiſh'd; without conſidering whether the ſtructure they propoſe would not, in ſome other as conſiderable reſpect, oppoſe the welfare of the animal; or whether it would be conſiſtent with the other uſes deſign'd by nature in that ſort of creature. For in the living works of ſo excellent an architect as nature, it muſt not be expected that any particular end ſhould be proſecuted, to the prejudice of the whole; but it muſt rather be ſuppos'd that ſhe aims not only at particular expedients, but univerſal ſymmetry, while ſhe excellently fits the ſeveral parts for their reſpective offices, tho' only ſo far as a due regard to the deſign, and welfare of the whole, will permit. Thus, as we before obſerv'd, tho' man be allow'd the moſt perfectly framed of any animal in the world, yet his body is not made the model for nature to form the correſpondent parts of other animals by. The lungs of a dog, a bird, a frog, a viper, *&c.* are of a ſtructure very different from thoſe of man. He is not furniſh'd with ſo many ſtomachs as an ox, or a ſheep; becauſe nature intended not that he ſhould ruminate like them. And tho' his gall be lodg'd in a peculiar bag, within the liver; yet 'twere raſh to ſay that the ſecretion of the bile is none of the uſes of thoſe livers, wherein ſuch a cyſtis is not to be met with; ſince, in ſome animals, as in horſes and pigeons, that bitter humour is not uſually, as in man, collected into one bag: and tho' it be ſo in vipers, yet, as far as I have obſerv'd, the containing cyſtis does not at all touch the liver.

3dly, 'Tis difficult to determine the true and primary uſe of a part; becauſe nature often fits one part for ſeveral uſes.

And, *4thly*, It ſometimes increaſes the difficulty, that nature may compaſs the ſame ends by ſeveral means, each of them ſufficiently commodious. I join theſe two obſervations together, becauſe in effect they often concur to render it hard to determine the true uſe of a part. Neither does nature conſtantly employ only one part to perform a particular office; but the deſign'd effect is ſometimes produced by a ſeries of ſucceſſive operations, to which ſeveral parts may differently contribute.

Neither the mechaniſm of a human body, nor of very conſiderable parts thereof, is to be aſſign'd from the bare ſtructure of the viſible matter, whether ſolid or fluid, to be found in the veſſels and cavities of a dead body, skilfully diſſected. For the body of a living man is a very compounded engine, many of whoſe functions are perform'd, not by the blood, and other viſible fluids, barely as they are liquors, but partly by their circulation, and other motions, and partly by a very agile and inviſible ſort of fluids, call'd ſpirits animal and vital; partly, perhaps, by little ſpringy particles, by ſomewhat that may be call'd the vital portion of the air, and by things analogous to local ferments: the important operations of all which, ceaſe with life, and the agents themſelves are not to be diſcern'd in a dead carcaſs. So that, beſides thoſe manifeſt uſes which the viſible fabric of the engine may ſuggeſt to an anatomiſt, there may be chymical uſes of ſome parts that ſerve for the elaboration of ſpirits, and other fluids; which uſes, as we formerly obſerv'd, are not ſuggeſted to the anatomiſt by the inſpection of the ſtructure of the parts, but to diſcern them may require no ordinary skill in chymical principles and operations.

Such conſiderations as theſe lead me to think it exceeding difficult to determine with any certainty the principal uſe of many particular parts, eſpecially if other uſes be excluded. It is not enough to ſecure us, that we underſtand the chief function and end of a part, to know that it is contrived for ſuch a purpoſe; ſince this fitneſs hinders not but that the primary uſe of the part may be another more conducive to the general welfare of the animal, or the coſmical ends of nature. And it ought not to ſeem ſtrange that ſome pieces of workmanſhip, conſiſting of many parts, all of them curiouſly contrived, ſhould, by one learned man, be gueſs'd to be deſign'd for this uſe, and by others for that; while both of them may be worthy of the artificer.

When ſome very politic prince does a great thing, without declaring why; the gueſſes of the ſtateſmen are often very different, tho' none of them aſcribe to him a deſign unbecoming a wiſe man. So when a learned author expreſſes himſelf ambiguouſly, one reader interprets his words to this ſenſe, and another to that; yet both of the ſenſes pitch'd on, may fairly agree with the context, and the main ſcope of the writer. This I ſay, becauſe I would by no means diſparage the wiſdom of nature, by propoſing the difficulties I have hitherto mentioned, tho' I confeſs, that upon the account of them, and ſome others, I look upon many of the arguments that ſeveral authors draw from final cauſes, but as conjectures. And in many caſes I allow what is ſuggeſted to me, upon the ſuppoſition of the intended uſes of particular parts, rather as proper hints to excite and direct a more thorough inquiry, than as ſafe grounds to build phyſical concluſions on.

PROP. V.

The naturaliſt ſhould not ſuffer the ſearch, or diſcovery, of final cauſes, to make him undervalue, or neglect, the inquiry after their efficients.

'Tis true, to inquire for what purpoſe nature would have particular effects produced, is a curioſity worthy of a rational creature. But this is not the proper task of a naturaliſt; whoſe work is not ſo much to diſcover the manner wherein, as for what reaſon particular effects are produced. Thus an ignorant ruſtic in *England* knows ſomething of a watch, if he is able to tell you that 'tis an inſtrument made to meaſure time; which is more than every *American* ſavage would be able to ſay, and more than thoſe civiliz'd *Chineſe* knew, who took the firſt watch they ſaw for a living creature: yet the *Engliſh* ruſtic, who knows no more of a watch, than that 'twas made to ſhew the hour of the day, underſtands but very little of the nature of that machine. And as the two things men aim at in phyſics, are to underſtand after what manner nature produces the phenomenon we contemplate; and, in caſe it be imitable by us, how we may, if occaſion requires, produce the like effect, or come near it; theſe ends cannot be attain'd by the bare knowledge of the final cauſes of things, nor of the general efficient. But, to anſwer thoſe intentions, we muſt know the particular efficients, with the manner and progreſs of their operating, and what diſpoſitions they either find, or produce in the matter they work on. Thus, he who would thoroughly underſtand the nature of a watch, muſt not reſt ſatisfy'd with knowing, in general, that a man made it for ſuch uſes; but he muſt, particularly, know of what materials the ſpring, the wheels, the chain, and the balance are made; he muſt know the number of the wheels, their magnitude, ſhape, ſituation, and connexion, in the engine, and after what manner one part moves another, in the whole ſeries of motions, from the expanſive endeavour of the ſpring, to the revolutions of the index that points the hours. And much more muſt a mechanic know all this, if he means to make a watch himſelf, or to give ſufficient inſtructions for it to be made. In ſhort, the neglect of efficient cauſes would render philoſophy uſeleſs; but the ſtudious ſearch after them will not prejudice the contemplation of final cauſes. For the wiſe author of nature has ſo excellently contriv'd the univerſe, that the more clearly and particularly we diſcern how ſuitable the means are to the ends to be obtain'd by them, the more plainly we diſcern the admirable wiſdom of the author of things; who is "wonderful in counſel, and excellent in working." Nor will the ſufficiency of the intermediate cauſes make it needleſs to admit a firſt and ſupreme cauſe, ſince that order of things, by virtue whereof theſe means become ſufficient to ſuch ends, muſt have been at firſt inſtituted by an intelligent cauſe. And if it be irrational to aſcribe the excellent fabric of the univerſe, and the actions that have manifeſt tendencies to determinate uſeful ends, to ſo blind a cauſe as chance; it will be rather more irrational to aſcribe to chance the firſt formation of the univerſe; of which the preſent ſtate of things is but the natural conſequence, or effect. For it may indeed be plauſibly ſaid, that in the preſent ſtate of things the ſeveral parts of the univerſe are, by the contrivance of the whole, determin'd, and thereby qualify'd to obtain their ends; but it cannot be rationally pretended, that at the firſt formation of the world there was a ſufficiency in the ſenſeleſs materials of it, without any particular guidance of a wiſe ſuperintendent to frame bodies which are ſo excellently contriv'd, and fitted to their reſpective ends.

Upon the whole, it appears, that all conſideration of final cauſes is not to be baniſh'd from natural philoſophy, but that 'tis rather allowable*, and, in ſome caſes, commendable, to argue, from the manifeſt uſes of things, that the author of nature pre-ordain'd them; that the ſun, and moon, with the other celeſtial bodies, excellently declare the power, wiſdom, and glory of God; and were, ſome of them, among other purpoſes, made to be ſerviceable to man; that, from the ſuppos'd ends of inanimate bodies, whether celeſtial or ſublunary, 'tis very unſafe to draw arguments to prove the particular nature of thoſe bodies, or the true ſyſtem of the univerſe; that, as to animals, and the more perfect ſort of vegetables, 'tis warrantable to ſay, particular parts were preordained to particular uſes, relating to the welfare of the animal, or plant itſelf, or to its ſpecies; but that ſuch arguments may eaſily deceive, if thoſe who form them are not very cautious to avoid miſtaking, among the various ends that nature may have in the contrivance of an animal body, and the various ways which ſhe may ſucceſsfully take to compaſs the ſame ends; and laſtly, it appears, that a philoſopher, nevertheleſs, muſt not let the ſearch or knowledge of final cauſes make him neglect a careful inquiry after efficients.

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* Final cauſes are allowable in philoſophy; and Sir *Iſaac Newton* himſelf ſcruples not to aſſign them in the ſtructure and contrivance of the univerſe. And of this we have a remarkable example in his doctrine of comets. "Comets, (ſays that "admirable philoſopher) by reaſon of the "great number of them, the vaſt diſtance "of their aphelia from the ſun, and the "conſiderable ſtay they make there, muſt "neceſſarily receive ſome diſturbance from "their mutual gravitations to each other. "Whence their eccentricities, and times "of revolution, will ſometimes be a little "increas'd, and at others, diminiſh'd. We "are not, therefore, to expect, that the "ſame comet ſhall always exactly return in "the ſame orbit, at the ſame period of "time. —— And hence appears the reaſon why comets move not in the zodiac, "as the planets do; but ſtray from it, in "various motions, to all the parts of the "heavens: for, in their aphelia, where "their motion is ſloweſt, they ought to "be at the greateſt diſtance from one another, that their mutual attractions may "be the leſs." *Newton. Princip. p.* 480.

THINGS ABOVE *REASON*, CONSIDER'D.

SECT. I.

Things above reaſon, of three kinds.

IT appears to me, that, among the objects of our reaſon, there are ſome, whoſe nature we cannot comprehend; others, whoſe attributes, or actions, we cannot reconcile to the ſubject; and, laſtly, others, that we cannot conceive how they ſhould conſiſt with ſome acknowledg'd truth: and theſe may be called things above reaſon. By which I here underſtand, not ſuch things as are falſe, and abſurd; but ſuch as, tho' the underſtanding ſees ſufficient cauſe to aſſent to, yet finds itſelf reduced to this aſſent, with a remarkable, and peculiar, diſadvantage. And this diſadvantage, uſually, proceeds either from the nature of the thing propoſed; which is ſuch, that we cannot, ſufficiently, comprehend it; or, from our being unable to conceive the manner of its exiſtence, and operation; or, becauſe it involves ſome notion, or propoſition, that we ſee not how to reconcile with ſome other, which, we are perſuaded, is true. The firſt of theſe three ſorts of things, may be call'd incomprehenſible; the ſecond, inexplicable; and the third, unſociable.

Incomprehenſible.

The firſt conſiſts of thoſe things, whoſe nature is not diſtinctly, and adequately, comprehenſible by us; to which, perhaps, we may refer all ſuch intellectual Beings, if any be granted, as are, by nature, of an higher order than human ſouls; at leaſt, we may refer to this head the great author of nature.

Inexplicable.

The ſecond ſort of things above reaſon, conſiſts of ſuch as, tho' we do not deny to exiſt, yet we cannot clearly, and ſatisfactorily, conceive how they can be what we acknowledge they are: as, how matter can be infinitely diviſible; and how there ſhould be ſuch an incommenſurability betwixt the ſide, and diagonal, of a ſquare, that the ſmalleſt line cannot, adequately, meaſure them both.

PHYSICS.

Unſociable.

The third ſort, are things incumber'd with difficulties, and objections, that cannot directly, and ſatisfactorily, be removed by thoſe who acquieſce in the receiv'd rules of inferior ſciences, and reaſon but at the common rate; ſuch objects of contemplation having ſomething belonging to them, that appears irreconcilable with ſome very manifeſt, or acknowledg'd truths.

Thus, that man has free will, at leaſt with regard to civil matters, is the general confeſſion of the ſpecies: all the laws that forbid, and puniſh, murder, adultery, theft, and other crimes, being founded upon a ſuppoſition, that men have a power to forbear committing them: and the ſenſe men have of being poſſeſſed of this power over their own actions, is great enough to make malefactors acknowledge their puniſhments juſt; being condemned by their own conſciences, as well as by their judges. Yet the generality of mankind aſcribe to God an infallible preſcience of human actions. But how a certain fore-knowledge can be had of contingent things, and ſuch as depend upon the free-will of man, is that which we are unable clearly to comprehend. And, doubtleſs, 'tis hard to conceive, how an infinitely perfect Being ſhould want preſcience; or, that our will ſhould want the liberty we almoſt perpetually exerciſe.

Again, geometricians teach the diviſibility of quantity *ad infinitum*, to be mathematically demonſtrable. Suppoſe, then, a ſtrait line, of three feet long, divided into two parts, the one double to the other; now a line of two feet is diviſible into infinite parts, or it is not; if you ſay it is not, you contradict a demonſtration; if you ſay it is, you muſt confeſs, either that the line of one foot is diviſible into as many parts as the line of two feet; or elſe, that the infinite parts into which the line of one foot is granted to be diviſible, is exceeded in number by the parts into which the line of two feet is diviſible; and, conſequently, that the line of two feet has a multitude of parts, greater than infinite. Which reaſon may let us ſee, that we may be reduced, either to reject inferences fairly drawn from manifeſted, or granted truths, or to admit concluſions that appear abſurd; if we will have all the common rules, whereby we judge of other things, to be applicable to infinites. Hence it is clear, in the general, that there may be things which ſurpaſs our reaſon; at leaſt, ſo far, as not to be judg'd of by the meaſures and rules uſed in judging of ordinary occurrences: for which reaſon, I ſhall call them privileg'd things.

Privileg'd things what?

The imperfection of the human mind.

But, to manifeſt the imperfections of our reaſon, with regard to privileg'd things, we need not have recourſe to the divine nature: a mathematical demonſtration may ſerve the turn. For, ſuppoſe a great-circle, divided into its 360 degrees; and ſuppoſe any number of ſtrait lines, drawn from the ſeveral aſſignable parts of ſome one of theſe degrees, to the centre; 'tis manifeſt, that the degrees being equal, as many lines may be drawn from any other; and ſo from every one of the reſt.

Then, ſuppoſe a circular arch, equal to the aſſumed degree, to be further bent into the circumference of a little circle, concentrical with the great one; it follows, from the nature of a circle, that the ſemi-diameters of it, how many ſoever they be, can no where touch one another, but in the centre. Whence 'tis evident, that all the lines, drawn from the circumference to the centre of the greater circle, muſt paſs thro' different points of the circumference of the ſmaller; and, conſequently, that as many lines as can, even mentally, be drawn from the ſeveral points of the circumference of the great circle to the common centre of both circles, muſt all paſs thro' different points of the little circle; and thereby divide it into as many parts as the greater circle is divided into. Here, then, the circumference of the leſſer circle preſents us with a curve line, which was not poſſibly diviſible into more parts than an arch of one degree, or the 360th part of the circumference of the greater circle; yet, without being lengthen'd, it becomes diviſible into as many parts as the whole circumference of the ſame greater circle. And, tho' we ſhould ſuppoſe the circumference of the internal circle not to exceed one inch; and that of the exterior circle to exceed the circumference of the terreſtrial globe, or *Orbis magnus* itſelf; ſtill the demonſtration would hold.

And, ſurely, 'tis very agreeable, both to the nature of God, and to that of man, that ſuch things as theſe ſhould prove true; for we miſtake, and flatter human nature too much, when we think our underſtanding ſo unlimited, both in point of capacity, and of extent; and ſo free, and unprepoſſeſs'd, as many philoſophers ſeem to ſuppoſe. For, whatever our ſelf-love may incline us to imagine, we are really but created, and finite Beings, and come into the world but ſuch as it pleas'd the moſt free author of nature to make us. And, from this dependance, and limitation of our natures, it follows, not only that we may be born with certain impreſſions, appetites, or tendencies of mind; but alſo, that the means given us to employ in the ſearch, or judging of truth, are only proportionable to the deſign of our make; and, therefore, may, probably, be ſuppoſed not able to reach all kinds of truths; many of which may be unneceſſary for us to know here: and ſome may be reſerv'd, to render us ſenſible of the imperfections of our natures, and to make us aſpire to that condition, wherein our faculties ſhall be much enlarged, and heighten'd. It ſeems not, therefore, unreaſonable to think, both that God has ſo limited our faculties, that, in our preſent ſtate, there ſhould be ſome objects beyond the comprehenſion of our underſtandings, or, that ſome of his creatures ſhould not be able perfectly to underſtand ſome others; and yet, that he has given us light enough to perceive, that we cannot attain to a clear, and full knowledge of them.

It may here, indeed, be objected, that tho' the Inſtances given, have not been hitherto clear'd by the light of reaſon, yet, 'tis probable, they may be ſo hereafter; conſidering how great a progreſs is, from time to time, made in the diſcoveries of nature. I anſwer, that I imagine, what future diſcoveries are made, will chiefly concern thoſe things which either we are ignorant of, for want of a competent hiſtory of nature; or miſtake, thro' erroneous prepoſſeſſions, or for want of freedom, and attention, in our ſpeculations. But as to all metaphyſical difficulties, wherein neither matters of fact, nor the hypotheſes of ſubordinate parts of learning are of force, we have little to expect. And, however it be as to other abſtruſe objects; I am very apt to think, there are ſome things relating to the infinite Being, which will ſtill remain incomprehenſible, even to philoſophical underſtandings. I can ſcarce hope to ſee thoſe obſtacles ſurmounted, that proceed not from any perſonal infirmity, or evitable faults, but from the limited nature of the human mind. Beſides, as mankind may hereafter explain ſome of thoſe grand difficulties, which have hitherto perplex'd philoſophers; ſo their inquiries may, poſſibly, lead them to diſcover new difficulties more perplexing than the firſt. For, even amongſt the things wherewith we are already acquainted, there are many which we think we know, only becauſe we never, with due attention, try'd whether we can frame ſuch ideas of them, as are clear, and worthy for a rational mind to acquieſce in. This appears from the great intricacy that conſidering men find in the notions commonly receiv'd of ſpace, time, and motion, *&c.* and the difficulties of framing clear, and ſatisfactory apprehenſions, even of ſuch obvious things. We ſee alſo, that the angle of contact, the doctrine of aſymptotes, and that of ſurd numbers, and incommenſurable lines, all which gives no concern to common accomptants and ſurveyors, perplex the greateſt mathematicians. Hence the growing curioſity of mankind is not more likely to ſolve ſome difficulties, than to raiſe others; which may prove more inſuperable than they.

The motion of a coach-wheel is ſo obvious, and ſeems ſo plain a thing, that the coach-man never looks upon it with wonder; yet after *Ariſtotle* had taken notice of the difficulty that occurr'd about it, this trivial phenomenon has perplex'd even famous mathematicians, and continues yet to do ſo: there being ſome circumſtances in the progreſſive motion and rotation of the circumference of a wheel, and its nave; or of two points aſſign'd, the one in the former, and the other in the latter, that have appear'd too ſubtile for modern writers.

After what manner human reaſon acts.

And here we may obſerve, that reaſon operates according to certain ideas, axioms, and propoſitions, wherewith, as by rules and meaſures, it conceives, eſtimates, and judges of things. And, indeed, when we ſay, that a thing is conſonant or repugnant to reaſon, we mean uſually, that it is either immediately or mediately deducible from, or at leaſt conſiſtent with, or contradictory to one or other of theſe ſtandard-rules or notions. But if theſe rules and notions be ſuch as are drawn only from finite things, or are agreeable but to thoſe; they may prove uſeleſs or deceitful, when we go to ſtretch them beyond their meaſure, and apply them to infinites.

To illuſtrate and confirm this notion. All the things that we naturally do or can know, may be divided into ſuch as we may acquire without a medium, and ſuch as we cannot attain to, but by the intervention of a medium, or by a diſcurſive act. To the firſt belong ſuch notions as are ſuppoſed to be connate; as, that "Two contradictories cannot be both true;" "The whole is greater than a part thereof;" "Every whole number is

“ either even or odd ;” *&c.* as alſo, thoſe other truths, that are aſſented to upon their own account, without needing any medium to prove them ; becauſe that, as ſoon as by clear terms, or fit examples, they are plainly propoſed to the underſtanding, they diſcover themſelves to be true ſo manifeſtly by their own light, that they want no propoſition to make the underſtanding acquieſce in them. Of this kind are ſome of *Euclid*'s axioms ; for inſtance, “ If to equal things equals be added, the totals will “ be equal.

To the ſecond ſort of things knowable by us, belong all that we acquire by the act of reaſoning ; wherein, by means of propoſitions or mediums, we deduce one thing from another ; or conclude, affirmatively or negatively, one thing of another. This being ſuppoſed, and we being conſcious to ourſelves that we are not the authors of our own nature ; all the experience we have hitherto had, leads us to think, that the meaſures ſuggeſted to us, either by ſenſations, the reſults of ſenſible obſervation, or the other inſtruments of knowledge, are ſuch as fully reach but to finite things ; and, therefore, are not ſafely applicable to others. And many of thoſe principles that we think very general, may be only gradual notions of truth ; and but limited and reſpective, not abſolute and univerſal.

And tho' perfect ſyllogiſms be counted the beſt and moſt regular forms, that our reaſonings can aſſume ; yet even the laws of theſe are grounded on the doctrine of proportions ; for even between things equal, there may be a proportion. Upon this ground, I ſuppoſe, it is, that mathematical demonſtrations have been publickly propoſed of the grand ſyllogiſtical rules. And, in conſequence hereof, geometricians tell us, there is no proportion between a finite line and an infinite ; becauſe the former can never be ſo often taken, as to exceed the latter ; which, according to *Euclid*'s definition of proportion, it ſhould be able to do. Since then, the underſtanding operates but by the notions and truths 'tis furniſh'd with, and ſince theſe are its inſtruments, by proportion to which, it takes meaſures, and makes judgments of other things ; ſuch inſtruments may be too diſproportionate to ſome objects, to be ſecurely employ'd to determine ſeveral particulars about them. Thus, the eye being an inſtrument which the underſtanding employs to eſtimate diſtances, we cannot by that, ſafely take the breadth of the ocean ; becauſe our ſight reaches not far enough, to diſcover the extent of ſo vaſt an object. And the common inſtruments of ſurveyors, that would ſerve to meaſure the height of a houſe or a ſteeple, or even of a mountain, cannot enable them to take the diſtance of the moon. But when aſtronomers take, by ſuppoſition, a line that reaches from the ſurface to the centre of the earth, tho' by the help of this, and the parallax, they may tolerably meaſure the diſtance of the moon ; yet, with all their great induſtry, they cannot, by the ſame way, with any tolerable accuracy, meaſure the diſtance of the ſtars ; the ſemi-diameter of the earth bearing no ſenſible proportion to that of ſo vaſt a ſphere, as makes their parallax vaniſh : it being all one to ſenſe, whether, at ſo great a remove, a ſtar be obſerv'd from the centre, or from the ſurface of the earth. Thus when I think of a triangle or a ſquare, I find in my imagination an intuitive idea of thoſe figures; that is, a picture clear and diſtinct, as if a figure of three ſides, or of four equal ſides and angles, were placed before my eyes. But if I would fancy a myriagon, or a figure conſiſting of ten thouſand equal ſides, my imagination is overpower'd with ſo great a multitude, and frames but a confuſed idea of a polygon with a very great many ſides. For if a man ſhould endeavour to frame ideas of a myriagon, or a chiliagon, they would be both ſo confuſed, that his imagination would not be able clearly to diſcriminate them, tho' the one has ten times as many ſides as the other. Thus if you would imagine an atom, of which perhaps ten thouſand would ſcarce make up the bulk of one of the light particles of duſt that plays in the ſun-beams, ſo extraordinary a minuteneſs, not having fallen under any of our ſenſes, cannot truly be repreſented in our imagination. So when we ſpeak of God's omnipotence, and other of his infinite attributes and perfections; we have ſome conceptions of the things we ſpeak of, but may very well diſcern them to be inadequate. And tho' ſeveral propoſitions relating to things above reaſon ſeem clear enough to ordinary capacities, yet he who ſhall with a competent attention, curioſity, and skill, conſider and examine them, will find that either their parts are inconſiſtent with one another, or that they involve contradictions to ſome acknowledg'd or manifeſt truth, or are veil'd over with darkneſs, and encumber'd with difficulties, from whence we are not able to reſcue them. Thus, when the ſide and diagonal of a ſquare are propoſed, we have clear and diſtinct ideas of each of them a-part; and when they are compared, we may have a conception of their incommenſurability; yet this negative notion, if it be thoroughly conſider'd, and far enough purſued, clearly contains that of a ſtrait line being diviſible *in infinitum*; and this diviſibility is encumber'd with ſo many difficulties, and is ſo hard to be reconciled to ſome confeſs'd dictates of reaſon, that philoſophers and geometricians, tho' convinc'd of the truth, are to this day labouring to extricate themſelves out of theſe perplexing intricacies.

'Tis evident that ſome ſubſtance or other, whether God, the world, or matter, never had a beginning, that is, has been for ever: but when we ſpeak of an eternity *à parte ante*, as they call it, we do not ſpeak of a thing whereof we have no conception at all; as will appear to a conſidering perſon: yet the general notion we have is ſuch, that when we come attentively to examine it, by the ſame ways whereby we judge of almoſt all other things, the underſtanding is confounded: for we muſt conceive, that the time efflux'd ſince the firſt man began to live, bears no greater proportion to the duration of God, or of matter, than a moment. And as there are ſome things, whoſe nature and conſequences puzzle our faculties; there are others, whereof tho' we have a notion, yet the *Modus operandi* is beyond our comprehenſion. I do not mean only the true and certain *Modus operandi*, but even an intelligible one. Thus tho' many, eſpecially *Carteſians*, and that upon a philoſophical account, aſſert that God created the world, yet how a ſubſtance could be made out of nothing, I fear we cannot conceive.

And tho' all philoſophers, very few excepted, believe that God made the world out of pre-exiſtent matter; yet how he could make it, but by locally moving the parts of the matter it was to conſiſt of, and how an incorporeal ſubſtance can move a body, which it may paſs thro' without reſiſtance, will be found hard to explain. If it be ſaid that the ſoul being an immaterial ſubſtance, can nevertheleſs move the limbs of the human body rightly diſpoſed; I anſwer, it does not appear that the rational ſoul gives any motion to the parts of the body, but only guides or regulates that which ſhe finds in them already.

Thus then by making obſervations of ſuch things as are the proper objects of our faculties, and by drawing juſt conſequences from ſuch obſervations, and from our other knowledge, we may come to be certain that ſome things exiſt, and ſo have general and dark ideas of them; when, at the ſame time, we are at a loſs to conceive how they can be ſuch, or how they can operate and perform what they do; ſuppoſing the truth and ſufficiency of ſome other things we are convinc'd of. To be ſhort, negative apprehenſions we may have of ſome privileged things; and poſitive, but indiſtinct apprehenſions of others; and that is enough, in ſome ſort, to make us underſtand ourſelves, and one another, when we ſpeak of them: tho' when we ſufficiently conſider what we ſay, we may find that our words are not accompany'd with clear, diſtinct, and ſymmetrical conceptions of thoſe abſtruſe and perplexing things we ſpeak of. And ſince we find by experience that we are unable ſufficiently to comprehend things, which by clear and juſt conſequences may be proved to exiſt; this ſtrongly argues, that ſome of our conceptions may be of things whereto ſomewhat belongs, that tranſcends our reaſon, and ſurpaſſes our comprehenſion. So that when natural philoſophy had taught men to believe God to be an infinitely perfect Being, we ought not to ſay that they had no idea of ſuch a Being, becauſe they had not a clear and adequate one. And ſince *Ariſtotle* profeſſedly diſcourſes of infinity, and cites the ancienter philoſophers for having done ſo before him; and ſince *Democritus*, *Epicurus*, *Gaſſendus*, and others, maintain either that the world is boundleſs, or that ſpace is not finite in extent, or that the world conſiſts of atoms, infinite in number; we muſt not ſuppoſe they ſaid they knew not what, as they muſt have done if they wrote without ideas of the things they treated of: tho' we may juſtly ſay, that the ſubject being infinite, the ideas they framed of it could not be comprehenſive and accurate. Thus, when the eye looks into a deep ſea, tho' it may pierce a little way into it, yet when it would go deeper, it only diſcovers ſomewhat dark and indiſtinct, which affects the organ ſo differently from what other more genuine objects do, that we thence eaſily diſcern our ſight fails us in the way, before it arrives at the bottom; and conſequently that there may be many things conceal'd there which our ſight is unable to reach. But if we really find there are things which our reaſon cannot comprehend, then, whether the account I have given why our faculties prove inſufficient for theſe things, be good or not, there muſt be ſome true account or other of that inſufficiency.

ciency. And ſome things muſt appear to us ſo ſublime and abſtruſe, that we are unable to comprehend them, and to diſcern ſo much as the reaſon why they cannot be comprehended by us.

Upon the whole, then, we may reaſonably ſuppoſe, that the great author of nature ſo framed man, as to have furniſh'd his intellective faculty with a light, whereby it can, not only eſtimate the power of a multitude of other things, but alſo judge of its own nature and power, and diſcern ſome of the limits, beyond which it cannot ſafely exerciſe its act of judging and defining. But the rational ſoul does not only paſs judgment of the things without her, but about herſelf, and what paſſes within her: ſhe ſearches out, and contemplates her own ſpirituality, and union with the body. The intellect judges wherein its own nature conſiſts, and whether or no itſelf be a diſtinct faculty from the will. And to come yet cloſer, logic and metaphyſics are the works of the human mind; which, by framing thoſe diſciplines, manifeſts that it not only judges of reaſoning, but of the very principles and laws thereof; teaching what things are neceſſary to obtain evidence and certainty, and what kind of mediums they are, from whence we muſt not expect any demonſtrative arguments relating to a ſubject. Thus, if we compare the bodily eye with the underſtanding, which is the eye of the mind; we muſt allow this difference, that the intellect is as well a looking-glaſs as a ſenſory; ſince it does not only ſee other things, but itſelf too, and can diſcern its own blemiſhes, bad conformation, or whatever other infirmities it labours under. The ſoul, therefore, when duly excited, is furniſh'd with a light that may enable her to judge, even of many of thoſe original notions, by which ſhe judges of other things. In ſhort, the ſoul, upon trial, may find, by an inward ſenſe, that ſome things ſurpaſs her force; as, a blind man being ſet to lift up a rock, would quickly find it too unwieldy to be managed by him: and the utmoſt exerciſe of his ſtrength, would but convince him of the inſufficiency of it to ſurmount ſo great a weight. We do not then pretend, that the eye of the mind ſhould ſee inviſibles; but only, that it ſhall diſcern the limits of that ſphere of activity, within which nature hath bounded it; and conſequently, that ſome objects are diſproportionate to it. *Ariſtotle* himſelf ſays, that the eye ſees both light and darkneſs; which expreſſion, tho' ſomewhat odd, may be defended: ſince, tho' darkneſs is a privation, not a Being, it cannot properly be the object of ſight; yet it may be perceiv'd by means of the eye, from the very different affection which that organ ſuffers, when impreſs'd by luminous objects, and when it is made uſeleſs to us by darkneſs.

Whether men may, with juſtice, diſcourſe of things above reaſon?

But it may be ask'd, how can we juſtify our diſcourſing, at all, of things tranſcending reaſon?

The better to clear this matter, I muſt make ſome diſtinctions of the notions or conceptions of the mind; and, for brevity ſake, give names to thoſe I have now occaſion to employ. I conſider then, that whether the conceptions or ideas we have of things, be ſimple or compounded, they may be diſtinguiſh'd into ſuch as are particular or diſtinct, and ſuch as are only general, dark, and confuſed, or indiſtinct. When a navigator to unknown countries, firſt gets a ſight of land, tho' he may be ſatisfied that it is land, yet he has but a very dark and confuſed picture of it made in his eye, and cannot deſcry whether the ſhore be rocky, or what creeks or harbours it may have in it; much leſs, whether the coaſt be well inhabited; and if it be, what kind of buildings it has: all which he may plainly and diſtinctly ſee, upon going on ſhore. And of ſome things we have an adequate, of others but an inadequate conception: thus, if we ſuppoſe the ſame navigator ſhould look towards the main ſea, tho' he might ſee a great way diſtinctly, yet at length it would appear ſo dark and confuſedly to him, that at the verge of the ſenſible horizon, his ſight would make him judge the ſea and sky came together; he would, however, conclude, that the utmoſt part of the ſea he could deſcry, was but a part of the ocean, which may, for ought he knows, reach to a vaſt extent beyond the viſible horizon. To our confuſed, and often, alſo, to our inadequate conceptions, belong many of thoſe that may be call'd negative; which we employ, when we ſpeak of privations or negations, as blindneſs, ignorance, death, *&c.* We have a poſitive idea of things that are ſquare and round, black and white; and, in ſhort, of other things, whoſe ſhapes and colours make them the objects of our ſight. But when we ſay, for inſtance, that a ſpirit or an atom are inviſible; thoſe words are attended with a negative conception, which is commonly but dark and confuſed, becauſe 'tis indefinite, and removes, or lays aſide thoſe marks, by which we clearly perceive and diſtinguiſh viſible ſubſtances: as, when we ſay, that ſuch a thing is impoſſible, we have ſome kind of conception of what we ſpeak; but 'tis a very obſcure and indiſtinct one, at beſt; exhibiting only a general and very confuſed repreſentation of ſome ways, whereby one might think the thing likely to be effected, if it were at all performable; and accompanied with a perception of the inſufficiency of thoſe ways. There is yet another difference in the notions we have of things, which, tho' not uſually obſerv'd, is too important to be here omitted. Of ſome things we have a knowledge, that for want of a fitter term, may be call'd primary or direct; and of others, the knowledge we have, is acquired but by inferring it from ſome more known or clearer truth; and ſo may be call'd inferr'd or illative knowledge. Thus, when a geometrician defines to me an hyperbola, I quickly gain a clear and diſtinct idea of it: but when he proves to me, that this hyperbola may have ſuch a relation to a ſtrait line, which he calls an aſymptote, that this line being continued, ſtill comes nearer and nearer to the prolong'd ſide of the hyperbola, and yet, how far ſoever both be drawn, will never come to touch it; his ſubtile demonſtrations preſent me with an inferr'd or illative truth, at which we arriv'd not, but by the help of a train of reaſonings; and on which if we exerciſe our imagination, we ſhall find this factitious truth, if we may ſo call it, accompanied but with a very dim and confuſed idea. To theſe diſtinctions we may add, laſtly, what belongs chiefly to the notions we have of true or falſe propoſitions, that of our conceptions of things, ſome are ſymmetrical, or every way conſiſtent; by which I mean, thoſe which have theſe two qualifications, the one, that all the parts are conſiſtent among themſelves; the other, that the entire idea is conſiſtent with all other truths: and ſome are chimerical, or aſymmetrical; by which I underſtand thoſe that are either ſelf-deſtructive, by the contrariety of the parts themſelves, whereof they are made up, as if one ſhould talk of a triangular ſquare, or a ſun-ſhiny night, or, being extravagant, lead to ſome manifeſt abſurdity that may be juſtly inferr'd from them, or into inextricable difficulties, or elſe involve a real repugnancy to ſome acknowledg'd truth, or rule of reaſon.

Now the mind of man is ſo framed, that, when duly inſtructed, it can perceive a want of light in itſelf for ſome purpoſes, or of clearneſs and completeneſs in the beſt ideas it is able to frame of ſome things; and, on this account, can ſo far take notice of the extent and imperfection of its own faculties, as to diſcern that ſome objects are diſproportionate to it. As when we attentively conſider the dimenſions of ſpace, or thoſe of the univerſe, we may, by trial, find, that we cannot conceive them ſo great, but that they may be yet greater, or may exceed the bounds, how remote ſoever, which our former conception preſumed to aſſign them. Thus, when the eye looks upon the main ſea, we eaſily grow ſenſible, that how much ſoever we can diſcover of it, yet our ſight falls far ſhort of the extent of that vaſt object. And 'tis by the ſenſe which the mind has of its own limits and imperfections, on certain occaſions, that we may eſtimate what things ought not, and what ought to be look'd upon as things above reaſon; for by that term I mean not ſuch things as our rational faculty cannot at all reach to, or has no kind of perception of; for of ſuch things we cannot, in particular, either think or ſpeak like men: but my meaning is this, that the rational ſoul, being conſcious of her own actions, and feeling that ſhe knows ſeveral ſorts of things truly and clearly, and thereby juſtly concluding them to be within the compaſs of her faculties; when ſhe contemplates ſome few things, which ſeem to be of another order, ſhe is convinced, that however ſhe ſtrains her power, ſhe has no ſuch ideas or perceptions of them, as ſhe has, or may have, of thoſe objects which are not diſproportionate to her faculties.

Again, the nature of the mind is ſuch, that its faculty of drawing conſequences from known truths, is of greater extent than its power of framing clear and diſtinct ideas of things; ſo that by ſuitable or ſucceſſive inferences it may attain to a clear conviction that ſome things exiſt, of whoſe nature and properties it can frame no clear and ſatisfactory conceptions. And that men ſhould be better able to infer propoſitions about ſeveral things, than to penetrate into their nature, is the leſs ſtrange, becauſe 'tis oftentimes ſufficient for our uſes to know that ſuch things exiſt, tho' that knowledge be not accompany'd with a clear and diſtinct idea, and becauſe the rules, as this, for example, "whatever is produced, muſt "have a cauſe," are often clear, and eaſy, that enable the mind to infer concluſions about things whoſe nature is very abſtruſe.

To apply, then, theſe notions to the three ſorts of things that I have repreſented, as being in ſome ſenſe above reaſon. There are objects of ſo immenſe and peculiar a nature, that by an eaſy view of the mind, without ſubtile and laborious inquiry, the ſoul diſcerns, and, as it were, feels the object to be diſproportionate to her powers; and, accordingly, if ſhe tries, ſhe quickly finds herſelf unable to frame conceptions thereof fit to be acquieſced in: and this ſort of objects, I upon that account call inconceivable, or ſupra-intellectual.

But when by attentively conſidering the attributes and operations of a thing, we ſometimes find that it hath a property belonging to it, or performs ſomewhat, which, by reflecting on the Beings and ways of working that we know already, we cannot diſcern to be reducible to them, or derivable from them; we then conclude this property, or this operation, to be inexplicable; that is, ſuch as cannot ſo much as, in a general way, be intelligibly accounted for: and this makes the ſecond ſort of our things above reaſon. But this is not all; for the rational ſoul, that is always furniſh'd with primitive ideas and rules of true and falſe, when ſhe comes to examine certain things, and make ſucceſſive inferences about them, ſhe finds that ſhe cannot avoid admitting ſome conſequences as true and good, which ſhe is not able to reconcile to ſome other manifeſt truth, or acknowledg'd propoſition. And other truths are ſo harmonious, that there is no diſagreement between any two of them. The heterogeneous truths I ſpeak of, appear not ſymmetrical with the reſt of the body of truths, and we ſee not how we can at once embrace theſe, and the reſt, without admitting the grand abſurdity, which ſubverts the very foundation of our reaſonings, that contradictions may be true. Thus, in the controverſy about the endleſs diviſibility of a ſtrait line, as 'tis manifeſt that a line of three feet, for inſtance, is thrice as long as a line of one foot, ſo that the ſhorter line is but a third part of the longer; it ſhould follow, that a part of a line would contain as many parts as the whole, ſince each of them is diviſible into infinite parts; which ſeems repugnant to common ſenſe, and, to contradict one of thoſe axioms in *Euclid*, whereon geometry itſelf is built. Upon which account, I have ventur'd to call this third ſort of things above reaſon, aſymmetrical, or unſociable; of which eminent inſtances are afforded us by thoſe controverſies, wherein, which ſide ſoever of the queſtion you take, you will be unable directly and truly to anſwer the objections that may be urg'd to ſhew that you contradict ſome primitive or other acknowledg'd truth.

Theſe are ſome of the conſiderations by which I have been induced to diſtinguiſh the things that to me ſeem to over-match our reaſon, into three kinds. Of thoſe I ſtile inconceivable, our ideas are ſuch, that a moderate attention ſuffices to make the mind ſenſible ſhe wants either light, or extent enough to have a clear and full comprehenſion of them; thoſe things I have called inexplicable, are ſuch as we cannot perceive to depend upon the ideas wherewith we are furniſh'd;

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and to reſemble, in their manner of working, any of the agents, whoſe nature we are acquainted with; and, laſtly, thoſe things which I have named unſociable, are ſuch as have notions belonging to them; or have concluſions deducible from them, that appear either diſagreeable to our primitive ideas, or, when driven home, inconſiſtent with the manifeſt rules we are furniſh'd with, to judge of true and falſe.

But, by ſorting things above reaſon, into three kinds, I do not deny that 'tis poſſible one object may, in different regards, be referr'd to more than one of theſe ſorts; as may ſufficiently appear in that nobleſt of objects, God; who, when he made the world out of nothing; or, when he diſcerns the moſt ſecret thoughts and intentions of the mind; or, when he unites an immaterial ſpirit to a human body, and maintains, perhaps, for very many years, that matchleſs union, with all the wonderful conditions he has annex'd to it; he ſupplies us with inſtances of things that are inexplicable: for ſuch operations are not reducible to any of the ways of working known to us, ſince our minds can but modify themſelves by various manners of thinking. And, as for things without us, all that one body can do to another, by acting on it, is to communicate motion to it; and thereby produce in it the natural conſequences of ſuch motion; in all which, there is no action like any of thoſe I juſt now aſcribed to God. And ſince the preſcience of thoſe future events, which we call contingent, being a perfection, is not to be deny'd to God; and ſince this ſeems irreconcileable to the freedom of human actions, it will afford us an inſtance of truths, whoſe conſiſtency, and whoſe ſymmetry with the body of other truths, our reaſon cannot diſcern; and which, therefore, ought to be referr'd to that ſort of things above reaſon, which I call unſociable.

I come now to the third ſort of theſe things, that mention'd under the name of incomprehenſible, or ſupra-intellectual; which title, whether it belongs to any other object, or no, certainly belongs to God, whoſe nature comprehending all perfections, in their utmoſt poſſible degrees, is not likely to be comprehenſible by our minds, who wholly want ſeveral of thoſe perfections, and have but moderate ſhares of the reſt. We are, indeed, born with, or, at leaſt, have a power, and ſeveral occaſions to frame an idea of a Being infinitely perfect; and, by this idea, we may ſufficiently diſcriminate the origin of it, God, from all other objects whatſoever: but, when we come to conſider attentively, and minutely, what is contain'd in the notion of omnipotence, omniſcience, eternity, and thoſe other attributes that are all united in that great affluence of perfections, God; we may be ſure to find, that our faculties are exceedingly ſurmounted by the vaſtneſs and glory of that unlimited, and unparallel'd object; about which, as we can diſcover that it exiſts, and that it poſſeſſes all the perfection we can conceive; ſo we may, at the ſame time, diſcern, that it muſt have degrees of perfection, which, becauſe of the inferiority of our nature, we are not able to comprehend. Yet this diſcovery of God's incomprehenſibility, may be made, without ſubtile inquiries, and without trains of conſequences, tho' not without attention, by a direct view of the mind; which finds itſelf, upon trial, as unable fully to meaſure the divine perfections, as the dimenſions of ſpace, which we can conceive to be greater and greater, without ever being able to determine any extent beyond whoſe limits they cannot reach.

'Tis indeed, therefore, arrogance to talk of infinite, or of privileg'd things, with the ſame confidence, or to pretend to do it with the ſame clearneſs wherewith knowing men may ſpeak of things, unqueſtionably within the compaſs of our underſtandings; but this need not hinder us, nor doth diſable us from ſpeaking rationally of privileg'd things themſelves. For all allowable notions are not of the ſame ſort, or order; and if none were to be admitted but thoſe that enable us to comprehend the object, that is, which give us a clear and diſtinct knowledge of all that it contains, or that belongs to it; I muſt confeſs we have no good notions of privileg'd things in particular. I muſt add, that I fear we have few or none, even of many things, that we think ourſelves very knowing in; and when we ſpeak of things as being above reaſon, tho' we have no clear, diſtinct, and adequate idea of them; yet we may have a general, confuſed, and inadequate notion of them, which may ſuffice to make us diſtinguiſh their reſpective objects from all elſe, and from one another; as may be obſerv'd in ſeveral ideas that are negatively framed; ſuch as thoſe we have of inviſible, incomprehenſible, and in others which I formerly call'd inferr'd, becauſe they accompany the remote inferences whereby one truth is concluded from another: as when geometricians infer from ſome propoſitions in *Euclid*, that any ſtrait line may be divided farther and farther, without ſtop. For of this and ſome other propoſitions about privileg'd things, we are not quite deſtitute of allowable notions, as may appear by ſome admirable ſpeculations of mathematicians about the affections of ſurd numbers, and incommenſurable magnitudes, of ſome of which we have no ſuch clear and ſymmetrical conceptions as we have of many other things that are of a nearer and more intelligible order. I ſhall not therefore ſcruple to acknowledge, that by my own experience, the confeſſions of others, and by their unſucceſsful attempts, I am induced to think, that God, who is a moſt free agent, having been pleaſed to make intelligent Beings, may perhaps have made them of different ranks, or orders, whereof men may not be the principal; and that whether there be ſuch orders or no, he hath at leaſt made us men of a limited nature in general, and of a bounded capacity, and accordingly hath furniſh'd man either with certain innate ideas, or models, and principles; or with a faculty, or power, and diſpoſition, eaſily to frame them, as it meets with occaſions to excite them. But becauſe God intended the mind of man of a limited capacity, his underſtanding is ſo conſtituted, that the in-bred, or eaſily acquired ideas, and primitive axioms, wherewith it is furniſh'd, and by relation or analogy whereto it judges of all other notions and propoſitions, do not extend to all knowable objects whatſoever, but reach only to ſuch as have a ſufficient affinity, or bear ſome proportion to thoſe primary ideas, and rules of truth, which are ſufficient, if duly improv'd, to help us to attain, tho' not the perfect knowledge of truths of the higheſt order, yet the competent knowledge of as much truth as God thought fit to allow our minds, in their preſent ſtate of union with our bodies.

And, indeed, I ſee no reaſon to repine at the limits which the author of nature aſſigns to human knowledge; for the number of privileg'd things is altogether inconſiderable, in compariſon of the multitude of others, to which our knowledge may be improv'd to reach, and which it far more concerns us well to know, than it doth to reſolve puzzling queſtions about things incomprehenſible; there being, within the compaſs of thoſe truths, enough to employ and reward our diligence, without ſtraining and tiring our reaſon about objects that tranſcend it. Yet even about theſe ſome inquiries may be allowed; for an object that is, on account of ſome of its properties, privileg'd, may have ſeveral others belonging to it, that do not ſurpaſs our reaſon, and whoſe knowledge may therefore be attain'd by the due employment of it. Thus we uſefully ſtudy the nature of bodies, which make up the object of natural philoſophy, tho' the true notion of body in general be a thing ſo difficult to frame, that the beſt of our modern philoſophers can by no means agree about it. This I do not wonder at, becauſe if we purſue the notion of body to the utmoſt, 'twill lead us to the perplexing controverſy *de compoſitione continui*; and there the underſtanding will be left in the dark. Thus ſurveyors, carpenters, architects, and many others, know ſeveral properties of the ſquare figure, that are of great uſe to them in their reſpective employments, tho' this, that its ſide and diagonal are incommenſurable, be unknown to moſt of them: and if they were told of it, and ſhould proſecute the ſpeculation, 'twould involve them in exceeding great, and, probably, inſuperable difficulties.

And even about privileg'd things, our inquiries, if diſcreetly manag'd, may not only be allowable, but ſometimes profitable. For of ſuch ſubjects a ſtudious ſearch may bring us to know more than we did, tho' not ſo much as we would, nor enough to be acquieſced in. This may probably teach us to know both the objects better, and ourſelves the better too, by giving us ſuch a ſenſible diſcovery of the inſufficiency of our underſtandings to comprehend all ſorts of things, as may be very uſeful, tho' not pleaſing; and may richly reward the pains that ended in ſo inſtructive a diſappointment. Thus in the nobleſt inſtance that can be given, the contemplation of God himſelf, tho' he has ſo order'd all things, that 'tis ſcarce poſſible for us to be deſtitute of an idea of him; yet when we come, with a ſufficient application of mind, to pry into the wonderful attributes of this moſt ſingular and adorable Being, we are ſure to find ourſelves unable to comprehend ſo unbounded an object. This, however, ought not to diſcourage us from ſo noble a ſtudy, ſince we are allowed the great contentment and honour to make further and further diſcoveries of the moſt excellent of objects, by that very immenſity of his perfections that renders it impoſſible for us to reach to the bounds of his excellency, or rather to diſcover that it has any bounds at all.

SECT. II.

Rules for judging of things above reaſon.

I Proceed, in the next place, to offer ſome rules and directions whereby to regulate and eſtimate the reaſonings we meet with concerning things above reaſon.

The firſt rule.

And, firſt, I would obſerve, that as to privileg'd ſubjects, we ſhould not admit any affirmative aſſertion, without ſuch proofs as are ſufficient in their kind.

For 'tis not reaſonable to give aſſent to any thing, as a truth, without a ſufficient ground for that aſſent: and tho' it may well be granted in the general that a thing which ſurpaſſes our reaſon may have belonging to it ſome property that is alſo above reaſon; yet we are not, in particular, to believe that this or that affection belongs to it, without particular and competent proof. For ſince about a privileg'd thing, as well as about any other, propoſitions may be framed, and often are ſo, contrary to one another; to aſſent to both were certainly to believe one falſity, if not two. And if we will aſſent but to one, we muſt either judge at adventures, or allow ourſelves to examine the mediums of probation, employ'd on both ſides; and thereupon judge why one of the propoſitions is to be aſſented to, and the other rejected. This manly freedom muſt be allow'd, without which our underſtandings were liable to be impos'd on, in matters of the higheſt concern; for there ſcarce ever did, or, I fear, ever will, want ſome men, who, either out of ignorance, and paſſive deluſion, ſelf-confidence, or deſign, take upon them with great boldneſs to affirm what they pleaſe about privileg'd ſubjects: and when they are oppoſed in their extravagancies, by reaſonings they cannot anſwer, they urge that theſe things being above reaſon, are not to be judg'd of by it. But of ſuch men as theſe I uſually demand, whether their own aſſent to the things they would have us believe, be grounded upon ſome rational argument, or not. If they ſay 'tis not, they are fools to believe it themſelves; and I ſhould add to the number, if, after this acknowledgment, I believ'd them. But if they ſay it is, I deſire them to produce their argument; for ſince 'tis framed by a human underſtanding, the force of it may alſo be comprehended, and judg'd of by a human underſtanding: and 'tis to no purpoſe to ſay the ſubject ſurpaſſes human reaſon; for if it do ſo indeed, it will ſurpaſs theirs, as well as mine, and ſo leave us upon even terms. And let the thing aſſented to be what it will, the aſſent itſelf ought to be founded upon a ſufficient reaſon, and conſequently upon one that is intelligible to the human underſtanding that is wrought on by it.

The poſitive proofs requir'd for an aſſertion, about a privileg'd thing, muſt, we ſay, be ſufficient in their kind; but we are not to expect rigid demonſtrations of ſuch aſſertions: for ſince 'tis manifeſt that there are many truths, ſuch as hiſtorical and political ones, that, by the nature of the things, are not capable of mathematical or metaphyſical demonſtrations,

tions, yet, being really truths, have a juſt title to our aſſent; it muſt be acknowledg'd, that a rational aſſent may be founded upon proofs that reach not to rigid demonſtrations; it being ſufficient that they are ſtrong enough to make a wiſe man acquieſce in them. And therefore if any things can be made out to be reveal'd by God, concerning his own nature, actions, or decrees, we ought to receive them; becauſe, of ſome of thoſe things, as his preſcience, mercy, *&c.* we can have no better proofs; and of others, as what he did before our world was made, and what he will do with us after we are dead, we can have no other conſiderable proofs at all. There is no reaſon to think, that becauſe an object ſurpaſſes the human underſtanding, it muſt therefore ſurpaſs the divine intellect itſelf. Even in the things that are tranſacted in the mind of man, I may learn from another, who is not my ſuperior, what I can by no means attain to know, unleſs he be pleas'd to diſcover it to me.

Intelligibility to the human underſtanding ſeems no more neceſſary to the truth or exiſtence of a thing, than that viſibility to a human eye ſhould be neceſſary to the exiſtence of an atom, or of a corpuſcle of air, or of the effluvia of a load-ſtone, or the fragrant exhalations of amber-greece, or musk. The natural incapacity of a child to underſtand the abſtruſe properties of parabola's, hyperbola's, and the incommenſurable lines of a ſquare, hinders not thoſe figures from exiſting, or their properties from being true and demonſtrable. And tho' we do admit ſome privileg'd things, yet there is no neceſſity that we ſhould be debarr'd from all knowledge of thoſe ſublime objects in which there are many things whereof we muſt confeſs ourſelves ignorant. The ancient geometricians knew very well what a rectangular triangle was, when they conceiv'd it to be a figure conſiſting of three ſtrait lines, two of which comprize a right angle; tho' probably, for a great while, they did not know all its chief properties; ſince, for ought appears, before *Pythagoras*, it was not known that the ſquare of the hypothenuſe is equal to the ſquares of both the other ſides taken together; and much more likely it is that they were not able to ſolve thoſe difficulties which attend the endleſs diviſibility of lines inferable from that equality.

But every thing hard to be underſtood, or contrary to the common rules of probability, has not a right to paſs for privileg'd; * for then the paradoxes about ſurd quantities, the duplicate proportion, and ſeveral other ſurprizing doctrines, capable of mathematical demonſtrations, would be be privileg'd. Nor are all thoſe worthy of this title, that are by many propoſed and embraced as philoſophical myſteries; ſuch as ſubſtantial forms, which are really only ſcholaſtic chimeras. But tho' I ſhall not preſume, poſitively, to ſet down the diſcriminating bounds, and ſigns of privileg'd things; yet moſt, if not all of them, are either primary in their kind, as God himſelf, and the things whoſe nature flows immediately from him; or elſe, things that, if thoroughly inſpected, neceſſarily involve the conſideration of ſome kind of *Infinitum*; or elſe, are ſuch, that tho' in ſome principal queſtions about them, one ſide muſt be taken, both ſides are encumber'd with abſurdities, or ſcarce ſuperable difficulties. Theſe being the uſual marks that belong to privileg'd things, their number cannot be very great; and therefore, we ſhall not, by allowing them, want objects whereon to exerciſe our faculties.

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* "Let it be propoſed (ſays the ingenious Mr. *Hauksbee*) to repreſent the "figure of an object, placed behind an opake body, upon the contrary ſide of that "opake body, and this without the help "of optic-glaſſes, or any foreign adventitious lights: perhaps the ſolution might "be thought impoſſible, or the very terms "of the problem abſurd and contradictory. For the body, on which the figure is to be ſeen, muſt be opake by "the hypotheſis, and the object placed "on the contrary ſide to that whereon "'tis ſeen; ſo that either the light muſt "be tranſmitted thro' this body, and then "'tis not opake, contrary to the ſuppoſition; or elſe the light muſt not be "tranſmitted, and then no figure could

A ſecond rule.

2*dly*, We ſhould not be haſty to frame negatives about privileg'd things; or to reject explications, or propoſitions of them, as if they were abſurd, or impoſſible.

We obſerve, that even in natural things, 'tis very unſafe to affirm, or reject opinions, before men have a competent hiſtorical information of what belongs to the ſubject they take upon them to judge of. And therefore, it muſt, in reaſon, be thought much more unwary to be forward to reſolve upon negative propoſitions, about things which, ſince we ourſelves acknowledge to be above the reach of human reaſon, 'twill become us, at leaſt to forbear a rude, and inſulting way of rejecting the opinions of learned men, who diſſent from us, about ſuch things; for the ſublimity of the ſubject may render miſtakes the more excuſable, becauſe difficult to be avoided; and our own ſharing in the inability of penetrating ſuch abſtruſe things, ſhould keep us from being over-confident, that we, alſo, may not be miſtaken; and incline us to tolerate other men's opinions, about matters wherein ourſelves have only opinion, not ſcience.

But, I muſt not be underſtood to ſpeak againſt all framing of negative propoſitions, about privileg'd things; my deſign being to diſſuade from doing it haſtily: for, ſometimes, 'tis much more eaſy and ſafe, to deny things,

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" be ſeen; for all diſtributions of light, " by optical artifices, are excluded. " The thing, however, is plain matter of " fact;" as he ſhews, by lining a globe of glaſs, in part, with melted ſealing-wax, common ſulphur, or melted pitch, and giving it a circular motion; for when his hand was then applied to the lined part, he diſtinctly, and perfectly ſaw the ſhape and figure thereof, upon the concave ſurface of the wax, &c. within. Hence that Gentleman thinks it may be uſeful to obſerve, " that many odd effects, and ap" pearances, againſt the poſſibility where" of, men may ſeem to argue very plau" ſibly, and to find downright abſurdities, " and contradictions in, may yet be brought " about by the genuine force of nature, " acting in convenient circumſtances, upon " proper and ſuitable bodies;" and therefore, that, " we ſhould not, upon ſuch " occaſions, proceed to conclude too pe" remptorily, what may, or may not be " done; and think that every difficulty, " or apparent impoſſibility to us, is a real " one to nature." See *Hauksb. Experiment.* p. 167,—171. & p. 268.

than to affirm them to belong to a ſubject that ſurpaſſes our reaſon. And, the obſervation may be of uſe, eſpecially in two caſes; one, when the negative, we aſſert, is grounded not upon axioms taken from the uſual courſe of nature, or upon propoſitions dubious, or remote from the firſt principles of knowledge; but upon either univerſal, or metaphyſical axioms; or elſe upon truths manifeſtly flowing from ſome clear, tho' inadequate notion, we have of the nature of the things we treat. The other caſe is, when we have a clear and ſufficient proof by revelation, or otherwiſe, of the poſitive attributes of the things we contemplate; for then we may ſafely deny of that ſubject, any other thing that is really inconſiſtent with the poſitive attribute. Upon which account, tho' we do not fully comprehend what God is, yet knowing by the clear light of nature, that he is a Being intelligent, and infinitely perfect, we may ſafely deny, againſt *Epicurus*, *Vorſtius*, and Mr. *Hobbs*, that he is a corporeal ſubſtance; as alſo that he is mortal, or corruptible.

A third rule for judging of things above reaſon.

3*dly*, A matter of fact, or other propoſition about privileg'd things, being prov'd by arguments competent in their kind, we ought not to deny it merely becauſe we cannot explain, or, perhaps, ſo much as conceive the *Modus* of it.

I have obſerv'd a want of clearneſs in ſeveral diſcourſes, where the term *Modus* has been employ'd. Sometimes we would deny by it, ſo much as a poſſibility that one thing can belong to, or be truly ſaid of another; as when we ſay, we underſtand not how one creature can create another, or how there can be a line that is neither ſtrait nor crooked, or a finite whole number that is neither even nor odd. But moſt commonly we mean, by our not underſtanding the *Modus* of a thing, that we do not clearly and diſtinctly conceive after what manner the property, or other attribute, of a ſubject belongs to it, or performs its operations. The firſt kind may be call'd a poſſible *Modus*, and the other an actual *Modus*. Now in both the foregoing acceptations of the term, we may find inſtances fit for our preſent purpoſe. For we cannot imagine, how a ſhort line, or other infinite quantity, ſhould be endleſly diviſible; or, on the contrary, how infinite parts ſhould make but a finite total: and yet geometry conſtrains us to admit that it is ſo. But tho' there be few inſtances of this kind, yet of the other ſort of our ignorance of the *Modus* of things, there may be found more inſtances than we could wiſh there were; for even in natural and corporeal things, the eager diſputes of the beſt philoſophers, and the ingenious confeſſion of the moſt judicious and moderate, ſufficiently manifeſt, that as yet we know not the manner of operation, whereby ſeveral bodies perform, what we well know they bring to paſs. And not to enter into thoſe nice and tedious diſputes, about the cauſe of the coheſion of the parts of matter in the ſmalleſt, moſt principal, and moſt primary bodies; perhaps the way whereby the rational ſoul exerciſes any power over the human body, and the way whereby the underſtanding and the will act upon one another, have not yet been intelligibly explain'd by any. The like I may ſay of the phenomena of the memory; eſpecially in thoſe who are eminent for that faculty. 'Tis hard to conceive, how in ſo narrow a compaſs as part of a human brain, there ſhould be ſo many thouſand diſtinct cells or impreſſions, as are requiſite to harbour the characters or ſignatures of ſeveral languages, each of them conſiſting of many thouſand different words; beſides the images or models of ſo many thouſand faces, ſchemes, buildings, and other ſenſible objects, and the ideas of ſo many thouſand notions and thoughts, and the diſtinct traces of multitudes of other things: and how all theſe ſhall, in ſo ſmall a ſpace, have ſuch deep and laſting impreſſions made for them, and be oftentimes lodg'd ſo exactly in the order wherein they were at firſt committed to the memory, that upon a ſudden command of the will, or a ſlight caſual hint, a whole ſett of words, things, and circumſtances, will in a trice, as it were, ſtart up, and preſent themſelves, even in the very ſeries, order, and manner, wherein they long before were ranged. And I doubt not, that beſides thoſe abſtruſe things, about the *Modus* of which, the more candid philoſophers have confeſs'd their ignorance, there would others have been taken notice of, if we did but as fully and impartially inquire into the nature of all the things we think we know. And conſidering the yet depending diſputes between philoſophers and mathematicians, about the nature of place and motion, which are things obvious and familiar to us; one might, without other inducements, be inclined to think, we ſhould find many difficulties in many other ſubjects, wherein we do not now take notice of any, if we particularly ſtudied their nature; and that our acquieſcing in what we have learn'd about many things, proceeds not from our greater knowledge of their nature, but from our having exerciſed leſs curioſity and attention in conſidering it. And if in things corporeal, that are the familiar objects of our ſenſes, we are often reduced to confeſs our ignorance of the *Modus* of their exiſting, or operating; it will not be denied, that to a Being wholly unapproachable by our ſenſes, natural philoſophy may be allow'd to aſcribe ſome things, whoſe *Modus* is not attainable by our underſtanding: as the divine preſcience, which, as 'twere impious to deny, ſo, I fear, 'tis impoſſible to explain, as to the *Modus* of it.

And, perhaps, upon the ſame grounds, 'tis as inconceivable, that God ſhould ſee our thoughts, as how he can know our outward actions; for ſince we have no way of diſcerning the particular motions of mens bodies, but by ſome of our ſenſes, eſpecially our ſight; and ſince thoſe ſenſations themſelves neceſſarily require organs duly conſtituted, that is, made up of ſeveral parts, framed, and join'd together after a determinate manner; I ſee not how we can explain the perception of viſible objects, without an eye, or ſo much as any corporeal organ or ſubſtance; eſpecially, ſince 'tis very juſtly aſſerted, that the deity is not united to any portion of matter, as the human ſoul is to the human body.

We do not, however, deny to God either the power of moving matter, or that of perceiving all its motions; for which there is ſome poſitive proof, competent in its kind. For if there be an effect, that we diſcern muſt proceed from ſuch a cauſe or agent, we may conclude, that ſuch a cauſe there is; tho' we do not particularly conceive how, or by what operation, 'tis able to produce the acknowledg'd effect. Thus, tho' a man, otherwiſe of good judgment, being wholly a ſtranger to mathematics, cannot conceive how a skilful aſtronomer ſhould, many years before-hand, foretel eclipſes to a day and hour, and perhaps to a few minutes; yet when the ſucceſs, as it often happens, verifies ſuch predictions, he will be ſatisfied that the maker of them had the skill to foreknow the things foretold in them. And ſo the generality of learned men among us, who are not much acquainted with that part of navigation, which ſome moderns call *Limeneuretica*, or the art of ſteering to harbours, cannot well conceive how a ſhip that is, for inſtance, in the vaſt *Atlantic* ocean, above a thouſand miles from any ſhore, ſhould be ſo directed, as to arrive juſt at a little harbour, not cannon-ſhot over; which perhaps neither the pilot, nor any other in the ſhip, ever ſaw. And yet, as little as we diſtinctly conceive how ſuch an art of finding ports can be framed, we ſcruple not to allow there is ſuch an one, becauſe navigators to the *Eaſt* and *Weſt-Indies* could not, without it, find the remote ports they are bound for.

There is a ſecond ſort of poſitive proofs, conſiſting of thoſe conſequences that are clearly and juſtly drawn from any manifeſt, acknowledg'd, or already demonſtrated truth. To this belong ſeveral mathematical propoſitions and corollaries; which, tho' being nakedly propoſed, they ſeem incredible to the generality of learned men, and ſometimes to mathematicians themſelves, are yet fully aſſented to, becauſe they clearly follow from either manifeſt or demonſtrated truths. Thus, many cannot conceive how 'tis poſſible there may be millions of circles, whoſe circumferences ſhall each of them come nearer and nearer to another, and to a ſtrait line aſſign'd; and yet none of them touch, and much leſs cut either any other circle, or that line, but in the ſame point: and yet this is one of thoſe odd propoſitions that geometricians have rightly deduced, as corollaries from a propoſition of *Euclid*. And, tho' we cannot clearly conceive, how two lines, that, at their remoteſt ends, are but little diſtant from each other, ſhould perpetually incline to each other, without ever meeting; yet geometricians, that is, the moſt rigid reaſoners we know of, have been compell'd to admit this in the conchoid of *Nicomedes*.

A fourth rule.

4*thly*, When we treat of privileg'd ſubjects, we are not bound to ſuppoſe every thing falſe, that ſeems to oppoſe ſome receiv'd dictate of reaſon.

For, it being evident, that a great part of the dictates of reaſon are negative, and that negative propoſitions uſually ſpring from the repugnancy we judge ſome things have to a poſitive dictate of reaſon; if thoſe poſitive dictates contain but gradual and limited truths, and come to be unduly extended to privileg'd ſubjects, it may very poſſibly happen, that a thing, really true, will appear falſe, if judg'd of by its agreement to one of thoſe limited, and but reſpective dictates. 'Tis alſo clear, that, in natural philoſophy, the uſual ground on which we reject many things, is, that we judge them unintelligible. And, I cenſure not the practice in general, but think it may eaſily miſlead us, when extended to things that apparently tranſcend our reaſon, as, for ought yet appears, ſome of the modus's, even of things corporeal, are found to do. And, we ſometimes think we have made compleat enumerations of the ſeveral ways of the exiſtence of an attribute in a ſubject, or of the operation of one thing upon another, when, indeed, we have over-look'd one or other of them; and, perhaps, that which we have thus omitted, may be the true one; tho', poſſibly, no attention, and diligence of ours, could, in ſome caſes, have ſerv'd the turn; the *Modus* inquired after, being not conceivable by us, tho' it may be by a higher than a human intellect.

The ſchool-philoſophers, for many ages, in the catalogues they made of the means of a body's working upon another at a diſtance, did not think of the true ways by which odours and ſounds are communicated to us; and therefore had recourſe to certain unintelligible things, which they call'd intentional ſpecies. But the moderns acknowledge, that odours are communicated by effluvia exhaled from the odorous body, and fitted to affect our noſtrils; and that ſounds are tranſmitted to the ear, by the undulating motion which the air is put into by the impulſe of the vibrating, or otherwiſe agitated parts of the ſonorous body.

And ſuppoſing the reaſonable ſoul to be an immaterial ſubſtance; tho', men think they have ſufficiently expreſs'd the ways of determining the motion of a body, by ſaying, that the determination muſt be either in the line wherein the impellent made it move, or in the line wherein it was determin'd to move by the ſituation of the reſiſting body; yet the motions of the animal ſpirits, if not alſo ſome other internal parts of the body, may, the body being duly diſpoſed, be determin'd by the human will; which is a way quite different from the other. And how the power of determining the motion of a body, without any power to impart motion to that body, ſhould belong to an immaterial creature, which has no corporeal parts to reſiſt the free paſſage of a body, and thereby change the line of its motion; is not yet, nor, perhaps, ever will be, in this life, clearly conceived by men: tho' there is no doubt, that he who endow'd the ſoul with this attribute or power, perfectly underſtands both how it exiſts in the ſoul, and how the ſoul, by exerting it, operates on the body.

I am, however, in no wiſe againſt rejecting opinions that are found contrary to thoſe rules of reaſon, at the framing of which, the things in queſtion were duly conſider'd: but in caſes not thought on, when ſuch rules were deviſed, we are not always bound to ſubmit to be judg'd by them; and to maintain an opinion conformable to ſuch a rule, may be not to oppoſe a genuine and abſolute dictate of reaſon, but to rectify one that is erroneouſly thought ſo; by ſhewing that the rule is expreſs'd in more general and indefinite terms than it ought to have been. And, doubtleſs, of two opinions, that is the moſt rational, which is moſt agreeable to thoſe rules of reaſon, which are framed upon the fulleſt information. 'Tis eaſy to ſee, that in the rule I propoſe, very few of the caſes occurring in ordinary diſcourſe, or even in that of philoſophers, will be at all concern'd, cern'd. And in thoſe few caſes wherein I intend the rule ſhould take place, I obviate inconveniencies by a double caution. The firſt, by ſuppoſing that the opinion which claims an exemption from the common rules, is not an arbitrary or precarious tenet, but ſufficiently made out by proper arguments; and the ſecond, by declaring that 'tis not to contradict right reaſon, but bad reaſoners, to give limitation to rules which have been too haſtily framed and conceived in too general terms, by men who either were not competently inform'd of the variety of particulars, when they took upon them to make analyſes and enumerations; or elſe preſumed to infer, that a thing was not, becauſe they did not underſtand the *Modus* of its exiſtence or operation. And I have often thought that the cauſes of the great clamour that is made againſt ſome men, for not obſequiouſly ſubmitting to what ſome others call the rules of reaſon, are, that men do not ſufficiently underſtand the nature of things, and of themſelves, but entertain too narrow conceptions of the former, and too high an opinion of the latter.

Reaſon, what? Reaſon is often taken for a ſett of notions and propoſitions employ'd and acquieſced in by this or that ſort of reaſoners, who receive names from this or that particular diſcipline, as aſtronomy, chymiſtry, optics; of whoſe receiv'd doctrines they are ſuppoſed to be intirely maintainers. But it is alſo, with at leaſt as much propriety, uſed to ſignify the rational faculty itſelf, furniſh'd with the light that accompanies it, when it is rightly diſpos'd and inform'd. In the firſt of theſe two ſenſes it ſeems but equitable to allow, that ſome things ought not to be judg'd by all the ſame rules employ'd to judge other things by; for ſome of theſe rules were framed upon a ſlight conſideration of common and familiar things, either by the vulgar, or by men who, for want of skill or application of mind, did not critically regard the diſtinct natures of things; and yet preſumed to ſettle rules which other mens inadvertence or lazineſs has made them receive for certain dictates of reaſon: whereas other natures ſhould have been then conſider'd, as well as thoſe; but not having been ſo, the rules I ſpeak of are not always proper and ſafe, when applied to theſe overlook'd natures. Thus ſucceſſive Beings, as time and local motion, in ſome caſes require to be eſtimated by other meaſures than ſubſtances, whether material or incorporeal. And ſo, likewiſe, the more nice metaphyſicians, eſpecially among the moderns, have thought themſelves obliged to diſcourſe of modus's, relations, privations, extrinſecal denominations, *&c.* in a very different way from that which belongs to bodies and ſpirits; tho' the unskilful are ſtill apt to confound all theſe ſubjects, by applying to them indiſcriminately the ſame rules, or, as they imagine, dictates of reaſon.

The generality of philoſophers, after *Ariſtotle*, conceive place to be the immoveable and immediately contiguous concave ſurface of the ambient body; ſo that 'tis a kind of veſſel, which every way contains the body lodg'd in it, but with this difference, that a veſſel is a kind of moveable place; as when a bottle of wine is carried from the cellar to the table:

but place an immoveable veſſel, or a veſſel conſider'd as immoveable. Now ſuppoſing, with *Ariſtotle*, and the generality of philoſophers, the plenitude of the world, it may be truly ſaid, that all plants, animals, minerals, ſtars, and other bodies, are each of them in ſuch an *Ariſtotelian* place; whence it has been uſually affirm'd that what is in no place, is not at all; yet it appears not how the outermoſt heaven can be properly ſaid to be in a place; ſince theſe philoſophers, aſſerting the world to be finite, muſt grant there is no ambient body without it, to contain it. And if the outermoſt heaven ſhould be impell'd, by the power of God, in a ſtrait line this way or that way, there would enſue a motion without change of place; for the outermoſt heaven was in none before, and does not, by its progreſſion, come to be contained by a new ambient body: and in this caſe, even according to thoſe modern favourers of *Ariſtotle*, who approve *Des Cartes*'s definition of local motion, the world may be ſaid to move, without changing place; for it does not paſs from the neighbourhood of ſome bodies to that of others; ſince comprizing all bodies, and yet being bounded, there is no body for it to leave behind, nor any beyond, for it to approach: and therefore tho' every particular body in the univerſe is naturally capable of local motion, yet the univerſe itſelf is not; and tho' every particular body in the world has ſome determinate figure, yet the world itſelf, if it be, according to the *Carteſians*, indefinite, has none.

PHYSICS.

Ariſtotle, and the philoſophers ſince his time, have generally admitted the diviſion, eſtabliſh'd by him, of all Beings into ſubſtance and accident; and accommodated their rules to one of them, or to both. But *Gaſſendus* and his followers have introduced a third ſort of Beings, as not being either ſubſtances or accidents. Of this kind of things, they make place or ſpace to be; for they will not allow it to be a ſubſtance, becauſe it is neither body nor ſpirit, but only ſomewhat that has a capacity to receive or contain bodies, and would ſubſiſt, tho' God ſhould annihilate all the ſubſtances he has created. And for the ſame reaſon it is not to be call'd an accident, ſince that neceſſarily requires a ſubſtance to reſide in; whereas, in caſe of the annihilation of the world itſelf, and conſequently all the ſubſtances that compoſe it, their place or ſpace would ſtill remain, and be capable of admitting a new world of the ſame extent, if God ſhould be pleaſed to create it: whence *Gaſſendus* wittily infers, that bodies are rather accidental in reſpect of ſpace, than ſpace in reſpect of bodies. But he who ſhall with an heedful and unprejudiced eye ſurvey the ſeveral hypotheſes, or ſyſtems, maintain'd by the different ſects of philoſophers, may find, that tho' the inſtances will not be all of them the ſame, yet there are none of theſe ſyſtems, wherein there may not be obſerv'd ſomething or other whereto every one of the rules, which reach to the other ſubjects treated of in that philoſophy, cannot ſafely be applied. And, indeed, the mind of man being naturally far more deſirous to know much, than to take the pains requiſite to examine whether it does ſo or not, is very apt to think that any ſmall number of things it has not diſtinctly conſider'd, muſt be of the ſame nature and condition with the reſt, that it judges of the ſame kind. For by thus eaſily attaining to the knowledge of things, the mind gratifies, at once, both its vanity, and its lazineſs; looking upon theſe concluſions, as marks of the excellency of its rational faculty; whilſt they rather proceed from a want of the due exerciſe of it.

The internal light of the mind.

But, ſince there is no progreſs *ad infinitum* in the *Criteria* of truth; and ſince our faculties are the beſt inſtruments that God has given us, to diſcover, and to examine it by; I think, a clear light, or evidence of perception, ſhining in the underſtanding, affords us the greateſt aſſurance we can have, in a natural way, of the truth of the judgments we paſs upon things. And, it is not by induction, but by evidence, that we know nothing but truth follows from truth. By which it appears, that the innate light of the rational faculty, is more primary than the very rules of reaſoning; ſince, by that light, we judge even of this axiom, which is itſelf the grand principle of reaſoning by inference. And, as the underſtanding is uſually look'd upon to be the eye of the mind, there is this analogy between them, that there are ſome things which the eye may diſcern mechanically, or by the help of inſtruments; as when it judges a line to be ſtrait, by the application of a ruler to it, or to be perpendicular, by the help of a plumb-line; or a circle to be perfect, by the help of a pair of compaſſes: but there are other things which the eye perceives and judges of immediately, by intuition, and without the help of organs, or inſtruments; as when, by the bare evidence of the perception, it knows, that this colour is red, and that blue; that ſnow is white, not black; and a coal, black, not white. For, thus there are ſome things, which the intellect uſually judges of in a kind of organical way; that is, by the help of certain rules, or hypotheſes, ſuch as are a great part of the theorems, and concluſions in philoſophy, and divinity; but there are others, which it knows without the help of theſe rules, more immediately, and, as it were, intuitively, by evidence, or perception; as that "two contradictory propoſitions cannot both be true;" that "from truth, nothing but truth can juſtly be deduced," *&c.* 'Tis alſo, upon this evidence of perception, that we receive, with an undoubted aſſent, many primitive ideas and notions; ſuch as thoſe of extended ſubſtance, or body; diviſibility, or local motion, a ſtrait line, a circle, *&c.*

And, it ſeems to me, that the internal light, which the author of nature has ſet up in the mind of man, qualifies him, if he makes a right uſe of it, not only to apply the inſtruments of knowledge, but alſo to frame, and to examine them. For, by the help of this light, the underſtanding is enabled to look about, and both to conſider a-part, and compare together, the natures of all kind of things; without being neceſſitated to employ, in its ſpeculations, the rules, or dictates of any particular ſcience, or diſcipline; being ſufficiently aſſiſted by its own light, and thoſe axioms and notions that are of a general nature, and perpetual truths; and ſo, of a higher order than the dictates or rules of any particular, or ſubordinate ſcience, or art. And, by theſe means, the underſtanding may perceive the imperfection and falſity of ſuch rules, or theorems, as thoſe men who look no higher, nor further than their own particular ſcience or art embrace for certain and unqueſtionable. Thus, philoſophers obſerving that they could frame a clear notion of a thing, without conſidering whether it were actually in being or not, or even when they ſuppoſed that 'twas not actually in being; as we can frame a clear conception of a roſe in winter, when there are none to be found growing; and have a notion of a myriagon, tho' very likely there is no ſuch figure really exiſting in the world; men have generally concluded, that the eſſence of things is different, and ſeparable from their exiſtence: yet when we conſider that God is a Being infinitely perfect, and that actual exiſtence, being a perfection, muſt belong to him; we may, by the ſame light of reaſon that dictated eſſence and exiſtence to be two ſeparable things in all other Beings, diſcern, that they muſt be inſeparable in God, and conſequently that the forementiond rule, tho' more general than almoſt any other, is not abſolutely univerſal, but muſt be limited by the light of reaſon. And thus, alſo, philoſophers conſidering that not only all ſorts of bodies, but the immaterial ſouls of men, are endow'd with qualities, which are accidents, have included it in the very notion of a ſubſtance, to be the ſubject of accidents. But the free intellect, finding in itſelf a notion of an abſolutely perfect, and, therefore, exiſtent Being; and conſidering that to be the ſubject of accidents, is not a thing agreeable to the higheſt perfection poſſible, it concludes, that in God there are no accidents. And this concluſion has been embraced as a part, not only of chriſtian, but of natural theology, and maintain'd by ſeveral philoſophers themſelves, upon metaphyſical and other merely rational grounds. In ſhort, the native light of the mind may enable a man, who will make a free and induſtrious uſe of it, both to paſs a right judgment of the extent of thoſe very dictates that are commonly taken for rules of reaſon, and to frame others on purpoſe for privileg'd things.

A fifth rule.

5thly, Where privileg'd things are concern'd, we are not bound to reject as falſe whatever we know not how to reconcile with ſomething that is true.

I think it will not be doubted that there are, or may be conceiv'd, ſtrait lines, whereof one is a hundred, or a thouſand times longer than another; 'tis alſo generally granted, that a longer line conſiſts of, or may afford more parts, than a ſhorter: and, laſtly, 'tis generally acknowledg'd, that no number can be greater than infinite. Now I ſhould gladly ſee theſe propoſitions reconciled to the demonſtrations of geometricians, about the endleſs diviſibility of all ſtrait lines; whence they deduce, that tho' they be very unequal among themſelves, yet the ſhorteſt of them contains, or may afford, infinite parts. I am far from affirming that one truth can really contradict another; yet I think that which is but gradual, or limited truth, may, in ſome few caſes, be irreconcileable by us to an abſolute and univerſal truth. We may diſtinguiſh thoſe propoſitions we call true, into axioms metaphyſical, or univerſal, that hold in all caſes, without reſerve; and axioms collected, or emergent; by which I mean ſuch as reſult from comparing together many particulars that agree in ſomething common to them all. And ſome of theſe, tho' they be ſo general, that, in the uſual ſubjects of our reaſoning, they admit of no exceptions, yet may not be abſolutely, and without limitation, true. Of this we have an inſtance even in that axiom which almoſt all natural philoſophers have ſuppoſed, and built on, that ſomething cannot be made out of nothing; which tho' at leaſt one of the higheſt or gradual truths, may yet not be univerſally true, ſince for ought we know, God, who is acknowledg'd a Being infinitely perfect, may poſſeſs, and have exercis'd the power of creating. And, in ſuch caſes as this, not to be able to reconcile a truth concerning a privileg'd thing, with a propoſition that generally paſſes for true, will not preſently oblige us to reject either propoſition, as falſe; but, ſometimes, without deſtroying either, only to give one of them a due limitation, and reſtrain it to thoſe ſorts of things on which 'twas at firſt grounded, and to which 'twas, becauſe of man's ignorance, or inconſiderateneſs, not at firſt confined. And if the miracles vouch'd for any religion, be any of them granted true, it cannot well be denied that phyſical propoſitions are but limited, and ſuch as I call collected truths; being gather'd from the ſettled phenomena of nature, and liable to this limitation or exception, that they hold, only where the irreſiſtible power of God, or ſome other ſupernatural agent, is not interpos'd, to alter the courſe of nature.

Now the reaſon why we judge things are repugnant, being, that the notions or ideas we have of them ſeem to us inconſiſtent; if either of theſe notions be wrong framed, or be judg'd of by an unfit rule, we may think thoſe propoſitions to be contradictory that really are not. Thus thoſe uſed to employ their imaginations about things, which are the proper object of the intellect, are apt to pronounce others to be inconceivable, only becauſe they find them unimaginable; as if the fancy, and the intellect, were faculties of the ſame extent. Upon which account, ſome have ſo groſly erred, as to deny all immaterial ſubſtances, and choſe rather ſo far to degrade the Deity itſelf, as to impute to it a corporeal nature, than to allow any thing to have a being, that is not comprehenſible by their imagination; which themſelves acknowledge to be but a corporeal faculty. But beſides this miſtake of things repugnant, which ariſes from the miſapplication or miſmanagement of our diſcerning faculties, there may be another that proceeds from the imperfection and limitation of our underſtanding; which being unable to judge of privileg'd things, as it does of other objects, may ſometimes be unable to diſcover the reconcileableneſs which a more illuminated and penetrating faculty may diſcern. This may be illuſtrated by what uſually happens at ſea, where there is a free proſpect; when, looking towards the main, the ſky and the waters ſeem to meet at the edge of the ſenſible horizon; tho', indeed, they are as far diſtant, as the heavens from the earth. On the other hand, if you skilfully mix together the dry and fine powder of orpiment, and that of indigo, you will produce a green colour, as is known to painters; wherein in the eye takes notice but of an uniform mixture, in which it diſtinguiſhes neither blue nor yellow; but if you look on this mixture thro' a microſcope, the former colour diſappears, and you will plainly ſee, inſtead of it, blue and yellow grains of the powder, diſtinct from one another. Theſe inſtances may ſerve to ſhew the weakneſs of our viſive faculty, and the latter of them teaches, that a thing may appear one and different, as 'tis look'd upon by a more or leſs diſcerning eye. But an inſtance more to our preſent purpoſe is afforded by yellow diamonds, which, becauſe of their colour, the generality of goldſmiths take to be counterfeit gems; tho' very skilful lapidaries will, by ſure ſigns, diſcover and acknowledge them to be true diamonds. Whence we learn, that a more skilful judge may diſcern an agreement in things that almoſt all other men think they ſee manifeſtly to be of different natures.

I have alſo ſeveral times obſerved, that men judge two things to be irreconcileable, not only when they are both of them repreſented to the underſtanding in the form of propoſitions, but when one of them is only a notion, or a current definition. For ſeveral of theſe notions contain in them a propoſition, or are equivalent to it: as, when a circle is defined to be a figure contain'd by a line, all whoſe parts are equally diſtant from a point in the middle; this definition contains an affirmation of the eſſential property of a circle, which by the generality of geometricians is therefore diſtinguiſh'd from that conic ſection they call an ellipſis, tho' that be alſo a figure terminated by one curve line.

But if a man has not genuine and adequate notions of the things he judges of, he may confidently, and even upon very probable grounds, take things to be inconſiſtent, that in reality are not ſo. If an ordinary mathematician ſhould hear one man ſay that ſuch a figure is an ellipſis, and another affirm it to be a circle, he would think their aſſertions to be inconſiſtent, having his mind prepoſſeſſed with an ellipſis being a conic ſection, whoſe properties muſt therefore, he ſuppoſes, be very different from thoſe of a circle; whereas ſuch wary geometricians as the learned Dr. *Wallis*, will tell him, that the vulgar notions of conic ſections are not adequate to the figures producible by them. 'Tis true, when a right cone is cut quite thro' by an inclining plane, the figure produced by the ſection agrees well with the receiv'd notion of an ellipſis, in which the diameters are of an unequal length; yet if the plane cut the cone parallel to the baſis, that conic ſection will be a true circle, having all its diameters equal.

'Tis indeed an uncommon and unheeded account, but ſuch an one upon which I have obſerv'd not only logicians, but philoſophers themſelves, to err about judging things reconcileable, or inconſiſtent; that if a man be not ſufficiently acquainted with the nature of either of the two things under conſideration, he may think there is a contradiction between them, when there is none to a ſuperior underſtanding. For taking it for granted that he knows one thing for a truth, if ſome other thing be affirm'd to be ſo, which he has not skill enough to ſee how to reconcile to the other; no wonder, how well ſoever this may be evinced, that he ſhould as little know how to admit, as how to reject it. Thus, a novice in arithmetic, for example, finding that, according to his rules, there is not one mean proportional number between 4, and 32, will ſcarce be able to reconcile that propoſition to this other, that there are two mean proportionals between the ſame numbers; for he may, with a great appearance of reaſon, ask how, if there be not ſo much as one mean proportional, there can be two? whereas, thoſe who are acquainted with the nature of ranks, or ſeries of numbers, proceeding in geometrical proportion, will eaſily diſcern, that between thoſe two, both the number 8, and the number 16, are proportionals. Thus, if an old ſchool-philoſopher, or a mathematician, not acquainted with the later diſcoveries made by teleſcopes, ſhould hear one man ſay, that the moon is the moſt enlighten'd when ſhe appears full to us; and another affirm, that ſhe is more enlighten'd at the new moon than at the full: he would readily conclude, upon the ſuppoſition that the moon receives all her light immediately from the ſun, that the latter affirmation cannot be true; which, yet, he would not conclude, if he knew that the moon is as well enlighten'd by the earth, as the earth by the moon: upon which account, as at the full ſhe receives but thoſe rays that come to her directly from the ſun; at the change, ſhe receives both them in that part of her body turn'd to him, and thoſe other beams of his, which are reflected from the terreſtrial globe to that part of the moon that is neareſt to us. And thus, alſo, before the time of *Pythagoras*, not only the vulgar of the *Greeks*, but their philoſophers, and mathematicians too, often obſerving, that a bright ſtar preceded the riſing ſun; and that, frequently, likewiſe on other days, after ſun-ſet, another ſtar appear'd, which was none of the fixed ones; they, confidently, concluded, from the diſtant times of apparition, that the ſun was attended by two different ſtars; to which, accordingly, they gave two different names: but *Pythagoras*, (who was a far better aſtronomer, as appears among other things, by his maintaining, in thoſe early times, the motion of the earth about the ſun) undertook to diſabuſe them, and effected it. Now, if one who had obſerv'd *Venus*, only in the morning, ſhould have affirm'd, that, beſides the ſix known planets, there was a ſeventh, *Phoſphorus*, which preceded the riſing-ſun; and another, who had taken notice of her only in the evenings, ſhould aſſert, that, beſides the ſame ſix known ones, the only ſeventh was *Heſperus*, which, ſometimes, appear'd after his ſetting; a by-ſtander would preſently have concluded, that their aſſertions were not reconcileable, either to one another, or to the truth; which, in his judgment, was, that there muſt be no leſs than eight viſible planets. Yet *Pythagoras*,, who had more skill, diſcern'd, and taught, that theſe two phenomena were produced by one and the ſame planet, *Venus*, determin'd, by its peculiar motion about the ſun, to ſhew itſelf near our horizon, ſometimes before he aſcends it, and ſometimes after he had left it. Such inſtances as theſe, tho' offer'd but as illuſtrations, may diſſuade us from being too forward to reject every propoſition that we ſee not how to reconcile to what we take for a truth; provided the diſtruſted propoſition be ſuch as we would acquieſce in, if we could reconcile it to the ſuppoſed truth.

Upon the whole it appears, that when two propoſitions are laid down, one whereof is made evident to us by experience, or by reaſon, acting within its own juriſdiction or compaſs, and the other ſufficiently prov'd, by being mathematically demonſtrated, or duly atteſted by divine revelation; we ought not to reject either of theſe propoſitions as no truth, merely becauſe we do not yet know how to reconcile them; but we ſhould rather think that the collected propoſition is only a gradual or limited truth; or elſe conſider, that we knowing but ſo imperfectly as we do, the particular natures of privileged ſubjects, there may poſſibly be a ſuperior intellect, able to diſcern an agreement between what is deliver'd about that ſubject, and the affirmation which ſeems repugnant to it; tho' we are not quickſighted enough to perceive this agreement.

Nor will this doctrine, if duly limited to the ſubjects wherein alone I would have it admitted, expoſe us to have falſities impoſed on us at the pleaſure of bold and dictating men. For if neither of the things be privileged, but both in the juriſdiction of ordinary reaſon; I require, that the propoſitions framed about them be eſtimated according to the common dictates of reaſon. And even in caſes where one of the propoſitions is about a privileged thing, I do not at all think fit that it ſhould be receiv'd, in ſpite of its being repugnant to the gradual truth deliver'd in the other; unleſs it can, by ſome other argument, ſufficient in its kind, be prov'd to be true. Thus, tho' men know not how to reconcile the liberty of their own will with the infallible knowledge that God has of thoſe actions that flow from it; yet they have unanimouſly judg'd it reaſonable to believe both free-will, and preſcience; the former, becauſe they felt it in themſelves; and the latter, becauſe the fore-knowledge of things, being manifeſtly a perfection, ought not to be deny'd to God, whom they look'd upon as a Being ſupremely perfect; and becauſe ſome actions and events that they all judg'd to flow from mens free will, were, as the generality believ'd, foretold by prophetic oracles. But ſince we have ſcarce any way of diſcovering a falſity, but by its being repugnant to ſomewhat that is true; to deny, that, in caſes within the juriſdiction of ordinary reaſon, the repugnancy of a propoſition to any manifeſt truth ought to ſway our judgments, were to deprive us of the moſt uſeful criterion to diſtinguiſh between truth and falſhood.

The ſixth and laſt rule for judging of things above reaſon.

Laſtly, in privileged things we ought not always to condemn that opinion which is liable to ill conſequences, and encumber'd with great inconveniences, provided the poſitive proofs of it be ſufficient in their kind.

That this rule may be the more eaſily admitted, I ſhall ſeparately ſuggeſt three things, which I deſire may be afterwards conſider'd all together.

Firſt, clear poſitive proofs, proportionate to the nature of things, are genuine, and proper motives to induce the underſtanding to aſſent to a propoſition as true; ſo that 'tis not always neceſſary to the evidence and firmneſs of an aſſent, that the underſtanding takes notice of the conſequences that may be drawn from it, or the difficulties wherewith it may be encumber'd. This is plain in thoſe aſſents, which, of all others, merely natural at leaſt, are, by knowing men, thought to be the moſt undoubted, and the beſt grounded; I mean the aſſents that are given to the truth of geometrical demonſtrations: yet *Euclid*, for inſtance, in all his elements of geometry, in ſome of which ſurprizing paradoxes are deliver'd, contents himſelf to demonſtrate his aſſertions in a mathematical way, and does not anſwer, or take notice of any one objection. And the geometricians of our days think they may ſafely receive his propoſitions upon the ſtrength of demonſtrations annexed to them, without knowing, or troubling themſelves with the ſubtilties employ'd by the ſceptic *Sextus Empiricus*, or others of that ſect, in their writings againſt the mathematicians, and all aſſertors of aſſured knowledge.

The ſecond thing I would offer is, that we have ſhewn there are ſome things which our imperfect underſtandings either cannot or at leaſt do not perfectly comprehend; and that nevertheleſs men have not refrain'd from preſuming to dogmatize, and frame notions and rules about ſuch things, as if they underſtood them very well. Whence it muſt needs come to paſs that if they were miſtaken, as in things ſo abſtruſe 'tis very likely they often were; thoſe who judge by the rules they laid down, muſt conceive the propoſitions oppoſite to their miſtakes to be liable to very great, if not inſuperable difficulties and objections.

Thirdly, as we need not wonder that privileg'd things, which are uſually ſo ſublime as to have been out of the view of thoſe who framed the rules whereby we judge of other things, ſhould be thought liable to great objections by them who judge of all things only by thoſe rules; ſo we ſhould not require or expect more evidence of a truth relating to ſuch things, than that there are for it ſuch ſufficient poſitive reaſons, as notwithſtanding objections and inconveniencies, make it, upon the whole, worthy to be embraced.

Euclid, indeed, beſides that more ſatisfactory way of direct probation, which perhaps he might have oftener employ'd than he did, has ſometimes, where he thought it needful, had recourſe to a way of arguing to an abſurdity. But in theſe caſes he never goes out of the diſcipline he treats of; and confining himſelf to arguments drawn from quantity, he urges nothing as abſurd, but what is undeniably repugnant to ſome truth he had already demonſtrated, or to thoſe clear and undiſputed definitions, axioms, or *poſtulata*, which he ſuppoſes to have been already granted by thoſe he would convince. But tho' he thus argues, to prove that his readers cannot contradict him without contradicting themſelves; yet we find not that he was at all ſolicitous to clear thoſe difficulties, that a man ſo quick-ſighted could not but know ſome of his theorems were attended with; but he is contented to demonſtrate the incommenſurability of the ſide and diagonal of a ſquare, without troubling himſelf to take notice of the ſeeming abſurdities that attend the endleſs diviſibility of a line, which would follow from what he demonſtrated.

'Tis true, about ſome privileg'd things there are, and about ſome others there may be, contradictory opinions maintain'd. Now as both of theſe cannot be true, one of them muſt be ſo; as tho' it be hotly diſputed whether quantity be endleſly diviſible, yet certainly it either muſt, or muſt not be diviſible without end; and, as was formerly obſerv'd, which ſide ſoever you take, the inconveniencies will be exceeding great, and perhaps there will lie objections againſt it ſcarce to be directly anſwer'd. And ſince one of the two oppoſite opinions muſt be true, it will not always be neceſſary that an opinion muſt be falſe, which is encumber'd with great difficulties, or liable to puzzling objections. And therefore if the poſitive proofs on one ſide be clear and cogent, tho' there be perplexing difficulties objected by the other, the truth ought not, for their ſake, to be rejected; becauſe ſuch difficulties proceeding uſually either from notions that men preſume to frame about things above their reach, or from rules that were not made for ſuch points as are in diſpute; the objections are not to be judg'd ſo well founded, as is that acknowledg'd principle in reaſoning, "from truth nothing but truth can be juſtly inferr'd."

I confeſs I have always thought it reaſonable in ſuch caſes to compare as well the poſitive proofs of one opinion with thoſe of the other, as the objections that are urg'd on either ſide; and thence make my eſtimate, upon the whole, tho' with a peculiar regard to that opinion which has a great advantage in point of poſitive arguments; becauſe thoſe are, unqueſtionably, the proper inducements to aſſent. And then the objections may well enough be ſuſpected to proceed from the abſtruſe nature of privileged things, and the great narrowneſs of the rules whereby men uſually judge of things. For we may have a ſufficiently clear proof that a thing is, whilſt we have no ſatisfactory conception of its manner of exiſting, or operating; our inferr'd knowledge being clearer, and extending farther, than our intuitive, or apprehenſive knowledge.

But even about things that we cannot ſufficiently underſtand, we may in ſome caſes exerciſe our reaſon, in anſwering objections that are thought unanſwerable, becauſe not directly ſo. For we may ſometimes ſhew, by framing in another caſe a like argument, which the adverſary muſt confeſs does not conclude well; that neither does the argument, containing his objection, conclude right.

However, we muſt not expect to be able, as to privileged things, and the propoſitions that may be framed about them, to reſolve all difficulties, and anſwer all objections; ſince we can never directly anſwer thoſe which require for their ſolution a perfect comprehenſion of what is infinite. As a man cannot well anſwer the objections that may be made againſt the *Antipodes*, the doctrine of eclipſes, that of the different phaſes of the moon, and of the long days and nights of ſome months near the

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poles, the theory of the planets, *&c.* unleſs he underſtand the nature of the ſphere, and ſome other principles of coſmography and aſtronomy. So that where privileged things are concern'd, clear and poſitive arguments ought to be of great weight, in favour of the opinion they conclude for; even when, on the contrary ſide, we may diſcourſe ourſelves into ſuch difficulties as perplex, and, perchance, puzzle our limited underſtandings.

THE
Philoſophical Difficulties
Relating to the
RESURRECTION,
CONSIDER'D.

Preliminary obſervations.

THE queſtion, I here deſign to conſider, is, Whether to believe the reſurrection of the dead, which the chriſtian religion teaches, be to believe an impoſſibility?

I do not pretend, that the reſurrection is a thing knowable, or directly provable, by the mere light of nature; nor that it may poſſibly be effected by mere phyſical agents: ſo that, treating of the poſſibility of the general reſurrection, I here take it for granted, that God has been pleaſed to promiſe and declare, that there ſhall be one; and that it ſhall be effected, not according to the ordinary courſe of nature, but by his own immediate power.

It muſt alſo be obſerved, that the reſurrection taught by the chriſtian religion, is not here meant, in ſuch a latitude, as to comprize all that any particular church, or ſect of chriſtians, much leſs what any private writer hath taught about it; but only what is plainly deliver'd as to this point, in the ſcripture.

Identity; the difficulties of conceiving it.

I muſt further premiſe, that 'tis no eaſy matter to determine what is abſolutely neceſſary, and but ſufficient to make a portion of matter, conſider'd at different times, or places, fit to be reputed the ſame body. That the generality of men, in vulgar diſcourſe, allow themſelves a great latitude, in this caſe, will be eaſily granted, by him who obſerves the receiv'd forms of ſpeaking. Thus *Rome* is ſaid to be the ſame city, tho' it hath been often taken, and ruin'd by the *Barbarians*, and others. Thus, an univerſity is ſaid to be the ſame, tho' ſome colleges fall to ruin, and new ones are built; and, tho' once in an age, all the perſons who compos'd it, are ſucceeded by others. Thus, the *Thames* is ſaid to be the ſame river, that it was in the time of our fore-fathers; tho' indeed, the water, that now runs under *London-bridge*, is not the ſame that ran there an hour ago, and is quite other than that which will run there an hour hence. And, ſo the flame of a candle, is ſaid to be the ſame, for many hours together, tho' it, indeed, be every minute a new body; and tho' the kindled particles that compoſe it, at any time aſſign'd, are continually putting off the form of flame, and are repair'd by a ſucceſſion of the like.

Nor is it by the vulgar only, that the notion of identity has been hard to be obtain'd; for, it ſeems, that even the ancient philoſophers have been puzzled about it; witneſs their diſputes, whether the ſhip of *Theſeus* were the ſame, after it had been ſo far repair'd, from time to time, to preſerve it as a monument, that ſcarce any plank remain'd of the former timber. And, even, in metaphyſics, I think it no eaſy task, to eſtabliſh a true and adequate notion of identity; and clearly to determine, what is the true principle of individuation. And, this is not ſurprizing; for, almoſt every man, who thinks, conceives in his mind, this, or that quality, relation or aggregate of qualities, to be that which is eſſential to a certain body, and proper to give it a peculiar denomination; whereby it comes to paſs, that, as one man chiefly reſpects this thing, and another that, in a body that bears ſuch a name; ſo one may eaſily look upon a body as the ſame, becauſe it retains what he chiefly conſider'd in it; whilſt another thinks it to be chang'd, becauſe it has loſt that which he thought was the denominating quality, or attribute. Thus philoſophers, and phyſicians diſagree about water, and ice; ſome taking the latter to be but the former diſguiz'd; becauſe they are both of them cold, and ſimple bodies; and the latter eaſily reducible to the former, by being freed from the exceſſive adventitious degree of coldneſs; whilſt others, looking upon fluidity, as eſſential to water, think ice, upon account of its ſolidity, to be a diſtinct ſpecies of body. And ſo the *Peripatetics*, and chymiſts often diſagree about the aſhes and *Calces* of burnt bodies; the firſt referring them to earth, becauſe of their permanency, and fixedneſs; and many chymiſts taking them to be bodies *ſui generis*, becauſe common aſhes uſually contain a cauſtic ſalt; whereas earth ought to be inſipid. And, the like may be ſaid of ſome wood-aſhes, and lime-ſtone, and even coral; which when well calcin'd, and freſh, have a pungent taſt: beſides, ſome of them, that are inſipid, may be eaſily reduced into metals, as the *Calces* of lead and copper.

Theſe difficulties, about the notion of identity, being obſerv'd, will render it leſs ſtrange, that, among the ancient *Hebrews* and *Greeks*, whoſe languages were ſo remote, in ſeveral reſpects from ours, the familiar expreſſions, employ'd about the ſameneſs of a body, ſhould not be ſo preciſe, as thoſe who maintain the reſurrection, in the moſt rigid ſenſe, would have them.

In the next place, I obſerve, we may, agreeably with ſcripture, ſuppoſe, that a comparatively ſmall quantity of the matter of a body, being increas'd, either by aſſimilation, or other convenient appoſition of parts, may bear the name of the former body; as a large crop of corn, ariſing from a comparatively ſmall quantity of ſeed ſown.

And,

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And here it may be proper to mention an experiment, made by two acquaintance of mine, men of great veracity and judgment, who both aſſured me, that having ſown in a garden ſome aſhes of a plant like our *Engliſh* red poppy, they, ſooner than was expected, produced certain plants, larger and fairer than any of that kind which had been ſeen in thoſe parts. Now this ſeems to argue, that in the ſaline and earthy, that is, the fix'd particles of a vegetable, that has been diſſipated, and deſtroyed by the violence of the fire, there might remain a plaſtic power, enabling them to contrive diſpos'd matter ſo, as to re-produce ſuch a body as was formerly deſtroy'd. But to this plaſtic power, reſiding in any portion of the deſtroy'd body itſelf, it will not perhaps be neceſſary to have recourſe in our preſent undertaking, ſince an external and omnipotent agent can without it perform all that I need contend for.

The firſt objection againſt the reſurrection,

To come then to a more cloſe conſideration of thoſe difficulties which are ſaid to demonſtrate the impoſſibility of the reſurrection.

'Tis ſaid, when a man is once really dead, many parts of his body will, according to the courſe of nature, reſolve themſelves into multitudes of ſteams, that wander in the air; and the remaining parts, which are either liquid, or ſoft, undergo ſo great a corruption and change, that 'tis not poſſible ſo many ſcatter'd corpuſcles ſhould be again brought together, and re-united, after the ſame manner wherein they exiſted in a human body, whilſt it was yet alive. And, they ſay, much more impoſſible is it to effect this re-union, if the body have been, as it often happens, devour'd by wild beaſts, or fiſh; ſince, in this caſe, tho' the ſcatter'd corpuſcles of the carcaſs might be recover'd, as particles of matter; yet, having already paſs'd into the ſubſtance of other animals, they are quite tranſmuted by the new form of the beaſt or fiſh that devour'd them, and of which they now make a ſubſtantial part.

Yet far more impoſſible will this renewal be, if we put the caſe, that the body was devoured by *Cannibals*; for then the ſame fleſh, belonging ſucceſſively to two different perſons, 'tis impoſſible that both ſhould have it reſtor'd to them at once, or that any footſteps ſhould remain of the relation it had to the firſt poſſeſſor.

Anſwer'd.

But, in anſwer to this grand objection, I have ſeveral things to offer.

And, 1*ſt*, A human body is not like a ſtatue of braſs, or marble, that may continue, as to ſenſe, whole ages, in a permanent ſtate; but is in a perpetual flux, or changing condition; ſince it grows, in all its parts, and all its dimenſions, from a corpuſcle no bigger than an inſect, to the full ſtature of a man; which could not happen but by a conſtant appoſition, and aſſimilation of new parts, to the primitive ones, of the little embryo. And ſince men, as other animals, grow but to a certain degree, and till a certain age, and therefore muſt diſcharge a great part of what they eat and drink by inſenſible perſpiration, which *Sanctorius*'s ſtatical experiments and mine aſſure me to be ſcarce credibly great, as to men, and ſome other animals, both hot and cold; it will follow, that in no very great compaſs of time, a large part of the ſubſtance of a human body, muſt be changed. Yet 'tis conſiderable, that the bones are of a ſtable and laſting texture; as I found, not only by ſome chymical trials, but by the skulls and other bones of men, whom hiſtory records to have been kill'd an exceeding long time ago.

2*dly*, There is no determinate bulk, or ſize, neceſſary to make a human body paſs for the ſame; and a very ſmall portion of matter, will ſometimes, ſerve the turn. Thus an embryo, for inſtance, in the womb, a new-born child, a man at his full ſtature, and a decrepit, aged perſon, notwithſtanding the vaſt difference of their ſizes, are ſtill reputed to be the ſame perſon; as is evident by the cuſtom of crowning kings and emperors in the mothers womb; and by putting malefactors to death in their old age, for crimes committed in their youth. And, if a very tall, and unwieldy fat man, ſhould as it ſometimes happens, be reduced by a conſumption, almoſt to a skeleton; yet none would deny that this waſted man, were the ſame with him that had once ſo vaſt a body.

3*dly*, A body may either conſiſt of, or abound with ſuch corpuſcles, as may be variouſly aſſociated with thoſe of other bodies, and exceedingly diſguized by the mixtures, yet retain their own nature: and of this we have various inſtances in metals. Thus gold, for example, when diſſolv'd in *Aqua regia*, paſſes for a liquor; and, when dextrouſly coagulated, appears a ſalt, or vitriol; by another alteration, I have made it part of the fewel of a flame; being dextrouſly conjoin'd to another mineral, it may be reduced to glaſs; well precipitated with mercury, it makes a glorious tranſparent powder; precipitated with ſpirit of urine, or oil of tartar *per deliquium*, it makes a fulminating calx, that goes off very eaſily, yet is far ſtronger than gun-powder; precipitated with another certain alkali, the fire turns it to a fix'd and purple calx. But notwithſtanding all theſe, and various other diſguizes, the gold retains its nature, as may be prov'd by chymical operations, eſpecially by reductions. And mercury, is of a more changeable nature than gold; ſometimes putting on the form of a vapour; ſometimes appearing in that of an almoſt inſipid water; ſometimes aſſuming the form of a red powder; ſometimes that of a white, or yellow one; of a cryſtalline ſalt; of a malleable metal; and of what not? Yet, all theſe are various dreſſes of the ſame quickſilver, which a skilful artiſt may eaſily make it put off, and appear again in its native ſhape *.

And,

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* If groſs bodies, and light, be mutually convertible into each other, as Sir *Iſaac Newton* ſeems to think they are; what tranſmutations may there not be, in the compaſs of nature and art? Nature, as Sir *Iſaac* obſerves, ſeems delighted with tranſmutations: " Water (ſays he) ſhe changes into vapour; which is a ſort of air; and, by " cold, into ice; which is a hard, pellucid, fuſible, brittle ſtone; and this ſtone " returns into water by heat; and vapour " returns into water by cold. Earth, by " heat, becomes fire; and, by cold, returns into earth. Denſe bodies, by fermentation, rarify into ſeveral ſorts of " air; and this air by fermentation, and " ſometimes without it, returns into denſe " bodies. Mercury ſometimes appears in

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PHYSICS.

And tho' it be true that inſtances of the permanence of corpuſcles undergoing ſucceſſive diſguizes, may be much eaſier found among metals and minerals, than vegetables and animals; yet there are ſome to be met with among theſe too: for, not to mention *Hippocrates*'s affirmation, about purging a child with the milk of an animal, that had taken elaterium, I once, in *Savoy*, obſerv'd all the butter, that was made in ſome places, during the ſpring ſeaſon, taſted very much of a certain weed, which, at that time, abounds in the fields there. And conſidering how many elaborate alterations the rank corpuſcles of this weed muſt have undergone, in the various digeſtions in the cow's ſtomach, heart, udder, *&c.* and that afterwards two ſeparations, at leaſt, were made, the one of the cream from the reſt of the milk, and the other of the unctuous parts of the cream from the ſerum; it will ſcarce be deny'd, that vegetable corpuſcles may, by aſſociation, paſs thro' various diſguizes, without loſing their nature, eſpecially ſince the eſſential attributes of ſuch corpuſcles may remain undeſtroy'd, tho' no ſenſible quality ſurvive, to make proof of it, as is afforded by our example in the offenſive taſte. And beſides what we commonly obſerve on the ſea-coaſt, of the fiſhy taſte of thoſe ſea-birds that feed only upon fiſh; I purpoſely inquired of an obſerving man, who lived upon a part of the *Iriſh* coaſt where the cuſtom is to fat their hogs with a ſort of ſhell-fiſh, wherewith that place very much abounds, about the taſte of their pork; and he aſſured me, that the fleſh had ſo ſtrong and rank a ſavour of the fiſh, that ſtrangers could not endure to eat it. There is a certain fruit in *America*, well known to our *Engliſh* planters, which many of them call the prickle-pear, whoſe very red juice, being eaten with the pulp of the fruit, of which it is a part, paſſes thro' the various ſtrainers, and digeſtions of the body, ſo unalter'd, as to render the urine red enough to perſuade thoſe, who are unacquainted with this property, that they make bloody urine; as I have been ſeveral times aſſured, by unſuſpected eye-witneſſes. But, more odd is that, related by a learned man, who ſpent ſeveral years upon the *Dutch* and *Engliſh* plantations, in the *Charibbe* iſlands; when ſpeaking of

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" the form of a fluid metal, and ſometimes " in the form of a hard, brittle metal; " ſometimes in the form of a corroſive, " pellucid ſalt, called ſublimate; ſometimes in the form of a taſtleſs, pellucid, volatile white earth, call'd *Mercurius dulcis*; or in that of a red, opake, " volatile earth, call'd cinnabar; or in " that of a red or white precipitate; or " in that of a fluid ſalt: and diſtillation " turns it into vapour; and being agitated " *in vacuo*, it ſhines like fire. And, after " all theſe changes, it returns into its firſt " form of mercury. Eggs grow from inſenſible magnitudes, and change into animals; tadpoles into frogs; and worms " into flies. All birds, beaſts, and fiſhes, " inſects, trees, and vegetables, with their " ſeveral parts, grow out of water, and watry tinctures, and ſalts; and, by putrefaction, return again into watry ſubſtances. " Water, ſtanding a few days, in the open air, yields a tincture, which (like " that of malt) by ſtanding longer, yields " a ſediment, and a ſpirit; but, before putrefaction, is fit nouriſhment for animals " and vegetables. And, among ſuch various and ſtrange tranſmutations, why " may not nature change bodies into light, " and light into bodies?" *Newton. Optic. p.* 349, 350.

a fruit call'd *Janipa*, or *Junipa*, growing in ſeveral of thoſe iſlands, he ſays, that at the ſeaſon when this fruit falls from the tree, the hogs, which feed on it, have both their fleſh and fat of a violet colour, as experience witneſſeth; which colour is the ſame that the juice dyes. And the like happens to the fleſh of parrots, and other birds, that feed upon it.

Having thus ſhewn that the particles of a body may retain their nature under various diſguizes, I proceed to add, that they may be ſtripp'd of thoſe diſguizes, or, to ſpeak without a metaphor, be extricated from thoſe compoſitions wherein they are diſguized; and that ſometimes, by ſuch ways as thoſe who are ſtrangers to the nicer operations of nature, would never have thought on; nor will not, perhaps, judge probable, when propoſed.

Tho' vitrification be look'd upon by chymiſts as the ultimate action of the fire, and the moſt powerful way of making inſeparable conjunctions of bodies; yet even out of glaſs of lead, for inſtance, made of ſand, and the aſhes of a metal, (tho' the tranſmutation ſeems ſo great, that the dark and flexible metal is turn'd into a very tranſparent and brittle maſs) we have recover'd opake and malleable lead. And tho' there be ſeveral ways beſides precipitations, of divorcing ſubſtances, that ſeem ſtrictly, if not inſeparably, united; yet, by precipitation alone, if a man have the skill to chuſe proper precipitants, ſeveral ſeparations may be eaſily and thoroughly made, that every one would not think of: for 'tis not neceſſary that in all precipitations, as is obſerv'd in moſt of the vulgar ones, the precipitant ſhould indeed make a ſeparation of the diſſolv'd body from the maſs or bulk of that liquor, or other adjunct, whereto 'twas before united, and not be able to perform this, without aſſociating its own corpuſcles with thoſe of the body it ſhould reſcue, and ſo making, in ſome ſenſe, a new and farther compoſition. That ſome bodies may precipitate others, without uniting themſelves with them, is eaſily prov'd by the experiment of refiners in ſeparating ſilver from copper; for the mixture being diſſolv'd in *Aqua fortis*, if the ſolution be afterwards diluted, by adding fifteen or twenty times as much common water, and you put into this liquor a copper-plate, you ſhall quickly ſee the ſilver begin to adhere to the plate; not in the form of calx, as when gold is precipitated to make *Aurum fulminans*, or tin-glaſs, to make a fine white powder for a fucus; but in the form of a ſhining metalline ſubſtance, that needs no farther reduction, to be employ'd as good ſilver. And, by a proper precipitant, I have alſo, in a trice, reduced a large quantity of well-diſguized mercury, into running quick-ſilver. And, if one can well appropriate the precipitants to the bodies they are to recover, very ſlight, and unpromiſing agents, may perform great matters in a ſhort time.

If you let a piece of camphire lie a while upon oil of vitriol, ſhaking them now and then, it will be ſo corroded by the oil, as totally to diſappear therein, without retaining ſo much as its ſmell, or without any manifeſt ſign of there being camphire in that mixture; yet that a vegetable ſubſtance, thus ſwallow'd up and changed by one of the moſt fretting and deſtroying ſubſtances known in the world, ſhould not only retain the eſſential qualities of its nature, but be reſtorable to its obvious and ſenſible ones in a minute, and that by ſo unpromiſing a medium, as common water, will appear by pouring the ſolution into a large proportion of that fluid, to whoſe upper part there will immediately riſe white, brittle, ſtrong-ſcented, and inflammable camphire, as it was before.

'Tis here a principal conſideration, that all bodies being but parcels of the univerſal matter mechanically different, they may ſucceſſively put on forms, in a way of circulation, till they return to their original form, whence they firſt begun; by having only their mechanical properties alter'd.

That all bodies agree in one common matter, the ſchools themſelves allow; making what they call the *Materia prima* to be the common baſis of them all; and their ſpecific differences to ſpring from their particular forms: and ſince the true notion of body conſiſts either alone in its extenſion, or in that and impenetrability together, it will follow, that the differences which make the varieties of bodies we ſee, muſt not proceed from the nature of mere matter, of which we have but one uniform conception; but from certain attributes, ſuch as motion, ſize, poſition, *&c.* that we call mechanical affections. Hence a determinate proportion of matter being given, if we ſuppoſe that an intelligent and otherwiſe duly qualify'd agent were to watch this portion of matter in its whole progreſs thro' the various forms it is made to put on, till it come to the end of its courſe or ſeries of changes; and that this intelligent agent ſhould lay hold of this portion of matter, cloth'd in its ultimate form; and, extricating it from any other parcels of matter wherewith it may be mix'd, make it exchange its laſt mechanical properties for thoſe which it had when this agent firſt began to watch it: in ſuch a caſe, I ſay, this portion of matter, how many changes and diſguizes ſoever it may have undergone in the mean time, will return to be what it was; and if it were before part of another body to be reproduced, it will become capable of having the ſame relation to it, that formerly it had.

Thus, ſuppoſe a man to cut a large ſphere of ſoft wax into two equal parts, and of the one to make cones, cylinders, rings, ſcrews, *&c.* and kneading the other with paſte, make an appearance of cakes, vermicelli, wafers, biskets, *&c.* 'tis plain, that one may, by diſſolution, and other ways, ſeparate the wax from the paſte, and reduce it in a mould to the ſame hemiſphere of wax it was before; and ſo we may deſtroy all that made the other part of the wax paſs for cones, cylinders, rings, *&c.* and reduce it in a mould to one diſtinct hemiſphere, fit to be re-conjoin'd to the other; and ſo to recompoſe ſuch a ſphere of wax, as they conſtituted before the ſeparation was made. And to view precipitate, carefully prepared *per ſe*, one would think that art had here made a body extremely different from common mercury; this being conſiſtent, like a powder, very red in colour, and purgative, and for the moſt part vomitive in operation, tho' given but in the quantity of four or five grains: yet if you but urge this powder with a due heat; by putting the component particles into a new and fit motion, you may re-unite them together, ſo as to re-produce the ſame running mercury you had before the precipitate *per ſe* was made of it.

4thly, But the chriſtian doctrine doth not aſcribe the reſurrection to nature, or any created agent, but to the peculiar and immediate operation of God; who has declared, that he will raiſe the dead. Wherefore, when I mention chymical ways of recovering bodies from their various diſguizes, I am far from deſiring, that ſuch ways ſhould be thought the only ones, or the beſt that can poſſibly be employ'd to ſuch an end. For, as the generality of men, without excepting philoſophers themſelves, would not have believed or thought, that by eaſy chymical ways, bodies, which are reputed to have paſs'd into quite another nature, ſhould be reduced or reſtored to their former condition; ſo, till chymiſtry, and other parts of natural philoſophy, be more thoroughly underſtood, and farther promoted, 'tis probable, that we can ſcarce now imagine, what expedients to re-produce bodies, a further diſcovery of the myſteries of art and nature may lead us to. And much leſs can our ſlender knowledge determine, what means, even of phyſical ones, the moſt wiſe author of nature is able to employ, to bring the reſurrection to paſs; ſince 'tis a part of the imperfection of inferior natures, to have but an imperfect apprehenſion of the powers of one that is incomparably ſuperior to them. And even among ourſelves, a child, who is endow'd with a reaſonable ſoul, cannot conceive how a geometrician ſhould meaſure inacceſſible heights and diſtances; much leſs, how a coſmographer can determine the whole compaſs of the earth and ſea; or an aſtronomer ſhew how far 'tis from hence to the moon; and tell many years before-hand, what day and hour, and to what degree, ſhe will be eclipſed. And, indeed, in the *Indies*, not only children, but rational men, could not perceive how 'twas poſſible for the *Europeans* to converſe with one another, by the help of a piece of paper, at a hundred miles diſtance; and in a moment produce thunder and lightning, and kill men a great way off, as they ſaw done by guns; and much leſs, how they ſhould foretel an eclipſe of the moon, as *Columbus* did, to his great advantage: which things made the *Indians*, even the chiefeſt of them, look upon the *Spaniards* as perſons of more than human nature. Now, among thoſe who have a true notion of a Deity, a Being both omnipotent and omniſcient, that he can do all, and more than all, that is poſſible to be perform'd by any way of diſpoſing of matter and motion, is a truth that will be readily acknowledg'd; ſince he was able at firſt to produce the world, and contrive ſome part of the univerſal matter of it, into the bodies of the firſt man and woman. And that his power extends to the re-union of a ſoul and body, which have been ſeparated by death, we may learn from the experiments God has been pleaſed to give of it, both in the old teſtament and the new; eſpecially in the raiſing *Lazarus* and *Chriſt* again to life: of the latter of which, particularly, we have proofs ſtrong enough to ſatisfy any unprejudiced perſon, who deſires but competent arguments to convince him.

Since then a human body is not ſo confined to a determinate bulk, but that the ſame ſoul being united to a portion of duly-organiz'd matter, is ſaid to conſtitute the ſame man, notwithſtanding the vaſt differences of bigneſs that there may be, at ſeveral times, between the portions of matter whereto the human ſoul is united: ſince a conſiderable part of the human body conſiſts of bones, which are bodies of a very determinate nature, and not apt to be deſtroy'd by the operation either of earth or fire: ſince of the leſs ſtable, and eſpecially the fluid parts of a human body, there is a far greater expence made, by inſenſible tranſpiration, than even philoſophers would imagine: ſince the ſmall particles of a reſolved body may retain their own nature, under various alterations and diſguizes, of which 'tis poſſible they may be afterwards ſtript: ſince, without making a human body ceaſe to be the ſame, it may be repair'd and augmented by the adaptation of fitly-diſpoſed matter to that which pre-exiſted in it: ſince theſe things are ſo, why ſhould it be impoſſible, that a moſt intelligent agent, whoſe omnipotence extends to all that is not truly contradictory to the nature of things, or to his own, ſhould be able ſo to order and watch the particles of a human body, that of thoſe remaining in the bones, of thoſe that plentifully fly away by inſenſible tranſpiration, and of thoſe that are otherwiſe diſpoſed of upon their reſolution, a competent number may be preſerved or retrieved; ſo that ſtripping them of their diſguizes, or extricating them from other parts of matter, to which they may happen to be conjoin'd, he may re-unite them betwixt themſelves, and, if need be, with particles of matter fit to be intervoven with them; and thereby reſtore or reproduce a body, which being united with the former ſoul, may, in a ſenſe agreeable to the expreſſions of ſcripture, re-compoſe the ſame man, whoſe ſoul and body were formerly disjoin'd by death.

5thly, Hitherto we have taken the doctrine of the reſurrection in a more ſtrict and literal ſenſe, becauſe I would ſhew, that even according to that, the difficulties of anſwering what is mention'd againſt the poſſibility of it, are not inſuperable; tho' it would much facilitate the defence and explanation of ſo abſtruſe a thing, to allow, that as the human ſoul is the form of man, ſo that whatever duly-organiz'd portion of matter is thereto united, it therewith conſtitutes the ſame man; the import of the reſurrection is fulfill'd in this, that after death there ſhall be another ſtate, wherein the ſoul ſhall no longer perſevere in its ſeparate condition, but be again united, not to an ethereal, or the like fluid matter, but to ſuch a ſubſtance, as may, with tolerable propriety of ſpeech, be call'd a human body.

They who aſſent to this account of the poſſibility of the reſurrection of the ſame bodies, will, I preſume, be much more eaſily induced to admit the poſſibility of the qualifications the chriſtian religion aſcribes to the glorified bodies of the raiſed ſaints. For, ſuppoſing the truth of the ſcripture, we may obſerve, that the power of God has already extended itſelf to the performance of ſuch things, as import as much as we need infer; ſometimes by ſuſpending the natural actions of bodies upon one another, and ſometimes by endowing human and other bodies with preternatural qualities.

lities. And indeed lightneſs, or rather agility, indifference to gravity and levity, incorruption, tranſparency, opacity, figure, colour, *&c.* being but mechanical affections of matter; it cannot ſeem incredible, that the moſt free and powerful author of thoſe laws of nature, according to which, all the phenomena of qualities are regulated, may, as he thinks fit, introduce, eſtabliſh, or change them, in any aſſign'd portion of matter, and conſequently in that whereof a human body conſiſts. Thus tho' iron be a body above eight times ſpecifically heavier than water; yet in the caſe of *Eliſha*'s helve, its native gravity was render'd ineffectual, and it aſcended from the bottom to the top of the water: and the gravitation of St. *Peter*'s body was ſuſpended, whilſt his maſter enabled him to walk on the ſea. Thus the operation of the moſt active body in nature, flame, was ſuſpended in *Nebuchadnezar*'s fiery furnace, whilſt *Daniel*'s three companions walk'd unhurt therein. And thus the body of our Saviour, after his reſurrection, tho' it retain'd the very impreſſions that the nails of the croſs had made in his hands and feet, and the wound of the ſpear in his ſide, as the hiſtory of the goſpel aſſures us; was endow'd with far nobler qualities, than before its death. And, as the apoſtle tells us, that this great change of ſchematiſm in the ſaints bodies will be effected by the irreſiſtible power of Chriſt; we ſhall not ſcruple to allow of ſuch an effect from ſuch an agent, if we conſider how much the bare ſlight mechanical alteration of the texture of a body, may change its ſenſible qualities for the better. Thus, without any viſible addition, I have ſeveral times changed dark and opake lead, into finely-colour'd tranſparent glaſs, of a leſs ſpecific gravity; and thick fetid ſmoke, into a bright and ſcentleſs flame.

THE
Chriſtian Virtuoſo.

Experimental philoſophy leads to religion, in general,

THE Propoſition I ſhall here endeavour to eſtabliſh, is, that a man may be a virtuoſo, or experimental philoſopher, without forfeiting his chriſtianity.

And firſt, 'tis certain, that a due courſe of experimental philoſophy greatly conduces to ſettle in the mind a firm perſuaſion of the exiſtence, and ſeveral of the chief attributes of God; which perſuaſion is, in the order of things, the firſt principle of that natural religion we allow as the very foundation of reveal'd religion in general.

By diſcovering the exiſtence of God.

That the conſideration of the vaſtneſs, beauty, and regular motions of the heavenly bodies, the excellent ſtructure of animals and plants, beſides a multitude of other phenomena of nature, and the ſubſerviency of moſt of theſe to man, may juſtly induce him, as a rational creature, to conclude, this vaſt, beautiful, orderly, and admirable ſyſtem of things, which we call the world, was framed by an author ſupremely powerful, wiſe, and good, can ſcarce be deny'd by an intelligent and unprejudiced perſon. And this is ſtrongly confirm'd by experience, which witneſſeth, that in almoſt all ages and countries the generality of philoſophers and contemplative men were perſuaded of the exiſtence of a Deity, from the conſideration of the phenomena of the univerſe; whoſe fabric, and conduct, they rationally concluded, could not be juſtly aſcribed either to chance, or to any other cauſe than a divine Being.

But tho' God hath manifeſted himſelf, even to ſuch as conſider things but ſuperficially, by ſtamping upon ſeveral of the more obvious parts of his works ſuch conſpicuous impreſſions of his attributes, that a moderate degree of underſtanding and attention may ſuffice to make men acknowledge his being; yet this aſſent is greatly inferior to the belief which the ſame objects are fitted to produce in him, who with care and skill conſiders them. For, the works of God are ſo worthy of their author, that, beſides the impreſſes of his wiſdom and goodneſs, left, as it were, upon their ſurfaces; there are a great many more curious and excellent tokens, and effects of divine artifice, in the hidden and innermoſt receſſes of them: and theſe are not to be diſcover'd by the ſlight glances of the lazy, and the ignorant; but require the moſt attentive and prying inſpection of curious and well-qualify'd minds. And ſometimes in one creature there may be great numbers of admirable things, that eſcape a vulgar eye, which yet are clearly diſcernible by that of a true naturaliſt; who brings with him, beſides a more than common curioſity and attention, a competent knowledge of anatomy, optics, coſmography, mechanics, and chymiſtry. In ſhort, God has couch'd ſo many things in his viſible works, that the clearer light a man uſes, the more he may diſcover of their unobvious beauty, and exactneſs; and the more fully and diſtinctly diſcern thoſe qualities that lie more concealed. And the more wonderful things he diſcovers in the works of nature, the more corroborating proofs he meets with to eſtabliſh and enforce the argument drawn from the univerſe, and its parts, to demonſtrate that there is a God; a propoſition of ſo vaſt a weight and importance, that it ought to endear every thing to us, that is able to confirm it, and afford us new motives to acknowledge and adore the divine author of things.

Now, with regard to this matter, we may confidently ſay, that experimental philoſophy has a great advantage of the ſcholaſtic: for, in the *Peripatetic* ſchools, where things are aſcribed to certain ſubſtantial forms, and real qualities; the accounts of nature's works may be eaſily given in a few words, general enough to be applicable on almoſt all occaſions. But theſe uninſtructive terms neither oblige nor conduct a man to deeper ſearches into the ſtructure of things, their manner of being produced, and of operating upon one another; and conſequently are very inſufficient to diſcloſe the exquiſite wiſdom which the omniſcient maker has expreſſed in the peculiar fabric of bodies, and the well-regulated motions of them, or of their conſtituent parts. From the diſcernment of which things, nevertheleſs, it is, that there ariſes, by way of reſult in the philoſophic mind of an intelligent contemplator, a ſtrong conviction of the being of a divine artificer, and a juſt acknowledgment of his admirable wiſdom. To be told that an eye is the organ of ſight, and that this is perform'd by that faculty of the mind, which, from its function, is call'd viſive, will give a man but a ſorry account of the inſtrument, and manner of viſion itſelf, or of the knowledge of the artificer who form'd the eye. And he who can take up with this eaſy theory of viſion, will not think it neceſſary to beſtow pains to diſſect the eyes of animals, or ſtudy the writings of mathematicians, to underſtand the doctrine of it; and, accordingly, will have but mean thoughts of the contrivance of the organ, and the skill of the artificer, in compariſon of the ideas that will be ſuggeſted of both to him, who, being thoroughly skill'd in anatomy, and optics, by their help takes aſunder the ſeveral coats, humours, and muſcles, of which that exquiſite dioptrical inſtrument conſiſts; and, having ſeparately conſider'd the figure, ſize, conſiſtence, texture, tranſparency, or opacity, ſituation, and connexion of each of them, and how they are all adjuſted in the whole eye, ſhall diſcover, by the laws of optics, how admirably this little organ is fitted to receive the incident rays of light, and diſpoſe them in the beſt manner poſſible, to give a lively repreſentation of the numberleſs objects of ſight.

'Tis eaſy to ſay, in general terms, that the world is wiſely framed; but I doubt it often happens, that men confeſs the creatures are wiſely made, rather becauſe they, upon other grounds, believe God to be a wiſe agent, than becauſe ſo ſlight an account as the ſchool-philoſophy gives of particular creatures, convinces them of any divine wiſdom in the creator. And tho' I am willing to grant that ſome impreſſions of God's wiſdom are ſo conſpicuous, that even, as was ſaid before, a ſuperficial philoſopher may thence infer that the author of ſuch works muſt be a wiſe agent; yet how wiſe an agent he has in thoſe works expreſſed himſelf to be, none but an experimental philoſopher can well diſcern. And 'tis not by a ſlight ſurvey, but by a diligent and ſkilful ſcrutiny of the works of God, that a man muſt be, by a rational and affective conviction, engaged to acknowledge that the author of nature "is wonderful in counſel, and excellent in "working."

The immortality of the ſoul.

2. After the exiſtence of the Deity, the next grand principle of natural religion is, the immortality of the rational ſoul; the genuine conſequence whereof is, the belief and expectation of a future and everlaſting ſtate. For this important truth, many arguments may be alledged, to perſuade a ſober and well-diſpoſed man to embrace it; but to convince a learned adverſary, the ſtrongeſt argument that the light of nature ſupplies us with, ſeems to be that afforded by real philoſophy: for this teacheth us to form true and diſtinct notions of the body, and the mind, and thereby manifeſts ſo great a difference in their eſſential attributes, that ſhews the ſame thing cannot be both. This it makes out more diſtinctly, by enumerating ſeveral faculties and functions of the rational ſoul; ſuch as to underſtand, and that ſo as to form conceptions of abſtract things, of univerſals, of immaterial ſpirits, and even of that infinitely perfect one, God himſelf; and alſo to conceive and demonſtrate that there are incommenſurable lines, and ſurd numbers; to form chains of reaſoning, and draw both cogent and concatenated inferences about theſe things; to expreſs intellectual notions, *pro re natâ*, by words, or inſtituted ſigns, to other men; to exerciſe free-will about many things; and to make reflections on its own acts, both of underſtanding and will. For theſe, and the like prerogatives, that are peculiar to the human mind, and ſuperior to any thing that belongs to the outward ſenſes, or to the imagination itſelf, manifeſt, that the rational ſoul is a Being of an higher order than corporeal; and, conſequently, that the ſeat of theſe ſpiritual faculties, and the ſource of theſe operations, is a ſubſtance, that, being in its own nature diſtinct from the body, is not naturally ſubject to periſh with it.

And, with regard to this truth, our virtuoſo hath an advantage of a mere ſchool-philoſopher. For, being acquainted with the true and real cauſes of putrefaction, and other phyſical kinds of corruption, and thereby diſcerning that the things which deſtroy bodies, are the avolation, or other receſs, of ſome neceſſary parts, and ſuch a depraving tranſpoſition of the component portions of matter, as is altogether unſuitable to the ſtructure, and mechanical modification, eſſential to a body of that ſpecies, or kind, whereto it belongs; he plainly perceives, that theſe cauſes of deſtruction can have no place in the rational ſoul; which being an immaterial ſpirit, and, conſequently, a ſubſtance not really diviſible, can have no parts expell'd, or tranſpoſed; and ſo being exempted from the phyſical cauſes of corruption, that deſtroy bodies, it ought to laſt always. And, being a rational creature, endow'd with internal principles of acting, as appears in free-will, it ought to live for ever, unleſs it pleaſe God to annihilate it; which we have no reaſon to ſuppoſe he will do. On the other hand, the modern *Peripatetics* maintain ſubſtantial forms, by ſome of them, ſtiled *Semi-ſubſtantiæ*; to which, in apes, elephants, and others, that paſs for ingenious animals, they aſcribe ſome ſuch faculties, and functions, as ſeem to differ only in degree from thoſe of the rational ſoul; but, how innocent ſoever their intentions may be, their doctrine tends greatly to weaken the chief phyſical way of proof, from whence the immortality of man's mind is juſtly inferr'd. For ſince, according to the *Peripatetics*, ſubſtantial forms are, as they ſpeak, educed out of the power, or potentiality, of the matter; and ſo depend upon it, not only as to action, but as to being, that they cannot at all ſubſiſt without it; but when the particular body, as an herb, a ſtone, or a bird, is deſtroy'd, they periſh with it; I think they give great advantage to atheiſts, and cavillers, to oppoſe the immortality of the mind.

For if to an ape, or other brute animal, there belongs a Being more noble than matter, that can actuate, and inform it, and make itſelf the architect of its own manſion, tho' ſo admirable as that of an ape, or an elephant; if this Being can, in the body it hath framed, perform all the functions of a vegetable ſoul; and, beſides thoſe, ſee, hear, taſt, ſmell, imagine, infer, remember, love, hate, fear, hope, expect, *&c.* and yet be a mortal thing, and periſh with the body; 'twill not be difficult for thoſe enemies of religion, who are willing to think the ſoul mortal, to fancy, that human minds are only a more noble, but not leſs mortal kind of ſubſtantial forms: as, amongſt ſenſitive ſouls themſelves, which they acknowledge to be equally mortal, there is a great diſparity in degree; that of a monkey, for inſtance, being very far ſuperior to that of an oyſter.

And the belief of a divine providence.

3. A third chief principle of natural religion, and, conſequently, of reveal'd religion, which ſuppoſes the former as its foundation, is a belief of the divine providence. And, in this grand article, as well as in the two foregoing, a man may be much confirm'd by experimental philoſophy; both as it affords him poſitive inducements to acknowledge the article, and as it ſhews the great improbability of the two principal grounds, on one or other of which is founded the denial of God's providence.

A virtuoſo, who, by manifold and curious experiments, ſearches deep into the nature of things, has great, and peculiar advantages, to diſcover, and obſerve, the excellent fabric of the world, as 'tis an immenſe aggregate of the ſeveral creatures that compoſe it; and to take notice in its particular parts, eſpecially thoſe that are animated, of ſuch exquiſite contrivances, and ſuch admirable co-ordinations, and ſubordinations, in reference to each other, as lie hid from thoſe who are not both attentive, and skilful. When our virtuoſo contemplates the vaſtneſs, ſcarce conceivable ſwiftneſs, and yet conſtant regularity, of the various motions of the ſun, moon, and other celeſtial lights; when he conſiders how the magnetiſm of the earth makes its poles conſtantly point the ſame way, notwithſtanding its various motions; how, by daily turning about its own centre in four and twenty hours, it receives as much light and benefit from the ſun, and all the glorious conſtellations of the firmament, as if they, with all the vaſt heavenly region they belong to, mov'd about it in the ſame time; how, by its ſituation among them, it enjoys the regular viciſſitudes of day and night, ſummer and winter, *&c.* how the ſeveral parts of the ſublunary world are mutually ſubſervient to one another, and moſt of them ſerviceable to man; how excellently the bodies of animals are contrived; what various and ſuitable proviſion is made for different animals; how admirable and aſtoniſhing a proceſs is gone through in the formation of the fœtus; how various animals are endow'd with ſtrange inſtincts, whoſe effects ſometimes ſeem much to ſurpaſs thoſe of reaſon itſelf: when, I ſay, a philoſopher duly reflects on theſe things, and many others of the like import, he will think it highly rational to infer from them theſe three concluſions.

Firſt, that a machine immenſe, beautiful, well contrived, in a word, ſo admirable as the world, cannot have been the effect of mere chance, or the tumultuous juſtlings, and fortuitous concourſe of atoms; but muſt have been produced by a cauſe exceedingly powerful, wiſe, and beneficent.

Secondly, that this moſt powerful author and contriver of the world hath not abandon'd a work ſo worthy of him, but ſtill maintains and preſerves it; ſo regulating the ſtupendouſly ſwift motions of the great globes, and other vaſt maſſes of the mundane matter, that they do not, by any great irregularity, diſorder the grand ſyſtem of the univerſe, and reduce it to a chaos, or confuſed ſtate of things.

Thirdly, that as it is not above the ability of the divine author, tho' a ſingle Being, to preſerve and govern all his viſible works, how great and numerous ſoever; ſo he thinks it not below his dignity and majeſty, to extend his care and beneficence to particular bodies, and even to the meaneſt creatures; providing, not only for the nouriſhment, but for the propagation of ſpiders, and ants themſelves. And, indeed, ſince the truth of this aſſertion, that God governs the world he has made, would appear by the conſtancy, and regularity, and aſtoniſhingly rapid motions of the vaſt celeſtial bodies, and by the long trains of as admirable artifices employ'd in the propagation of various ſorts of animals; I ſee not why it ſhould be deny'd that God's providence may reach to his particular works here below, eſpecially to the nobleſt of them, man; ſince moſt of thoſe who deny this, as derogatory to God's majeſty, and happineſs, acknow-

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ledge, that, at the firſt formation of things, the great author of them muſt not only have extended his care to the grand ſyſtem of the univerſe in general; but allow'd it to deſcend ſo low, as to contrive all the minute and various parts, not only of greater, and more perfect animals, as elephants, whales, and men; but ſuch ſmall, and abject ones, as flies, ants, mites, *&c.* which being manifeſtly propagated by eggs, laid by the female, cannot reaſonably be thought the offspring of putrefaction. Whence I gather, as from matter of fact, that to be concern'd for the welfare even of particular animals, as it is agreeable to God's wiſdom, and exuberant beneficence; ſo it is not truly derogatory to his adorable greatneſs and majeſty.

And ſince man is the nobleſt of God's viſible works; ſince very many of them ſeem made for his uſe; ſince, even as an animal, he is wonderfully made, and curiouſly or artificially wrought; and ſince God has both given him a rational mind, and endow'd it with an intellect, whereby he can contemplate the works of nature, and by them acquire a conviction of the exiſtence, and ſeveral attributes of their ſupremely perfect author; and, laſtly, ſince God hath planted notions and principles in the mind of man, fit to make him ſenſible that he ought to adore his maker, as the moſt perfect of Beings, the ſupreme Lord, and governor of the world; natural reaſon dictates to him, that he ought to expreſs the ſentiments he has for this divine Being, by a veneration of his excellencies; by gratitude for his benefits; by humiliation, in view of his greatneſs, and majeſty; by an awe of his juſtice; by reliance on his power, and goodneſs; and, in ſhort, by thoſe ſeveral acts of natural religion that reaſon ſhews to be ſuitable, and therefore due to thoſe ſeveral divine attributes of his, which it has led us to acknowledge.

And here I ſhall add, that, from the *Carteſian* principles, a double argument may be drawn for divine providence.

For, firſt, according to the *Carteſians*, local motion, which is the grand principle of all action among things corporeal, is adventitious to matter, and was originally produced in it, and is ſtill every moment continu'd, and preſerv'd, immediately by God: whence it may be inferr'd, that he concurs to the actions of each particular phyſical agent, and conſequently that his providence reaches to all and every one of them.

And, ſecondly, the ſame *Carteſians* believe the rational ſoul to be an immaterial ſubſtance, really diſtinct, and ſeparable from the body. Whence I infer, that the divine providence extends to every particular man; ſince, whenever an embryo, or little human body form'd in the womb, is, by being duly organized, fitted to receive a rational mind, God is pleas'd to create one, and unite it to that body. In which tranſaction, there ſeems to me a neceſſity of a direct and particular intervention of the divine power; ſince I underſtand not by what phyſical charm or ſpell an immaterial ſubſtance can be allured into this or that particular embryo, of many that are, at the ſame time, fitted to receive a human ſoul; nor by what merely mechanical ties, or bond, an immaterial ſubſtance can be durably join'd, and united, with a corporeal one, in which it finds no parts, that it has organs to take hold of, and to which it can furniſh no parts to be faſten'd upon by them. No better can I conceive, how a mere body can produce pain, pleaſure, *&c.* by its own mere action, or endeavour to act on an immaterial ſpirit. Nor will the force of all this reaſoning be eluded, by ſaying, with ſome deiſts, that, after the firſt formation of the univerſe, all things are brought to paſs by the ſettled laws of nature. For tho' this be confidently, and not without colour, pretended; yet I confeſs it does not ſatisfy me. For, not to mention the inſuperable difficulty there is to give an account of the firſt formation of things, which many deiſts will not aſcribe to God; nor that the laws of motion, without which the preſent ſtate and courſe of the world could never be maintain'd, did not neceſſarily ſpring from the nature of matter, but depended upon the will of the divine author of things; I look upon a law as a moral, not a phyſical cauſe; as being, indeed, but a notional thing, according to which, an intelligent and free agent is bound to regulate its actions. But inanimate bodies are utterly incapable of underſtanding what a law is, or what it enjoins, or when they act conformably or unconformably to it; and therefore the actions of inanimate bodies, which cannot incite or moderate their own actions, are produced by real power, not by laws; tho' the agents, if intelligent, may regulate the exertions of their power by ſettled rules.

4. I have taken notice of two other accounts, upon which the experimental knowledge of God's works may, in a well-diſpoſed mind, conduce to eſtabliſh the belief of his providence.

And, firſt, when our virtuoſo ſees with how many, and how various, and how admirable ſtructures, inſtincts, and other contrivances, the wiſe artificer hath furniſhed even brutes and plants to acquire, and aſſimilate their food; to defend, or otherwiſe ſecure themſelves from hoſtile things; to maintain their lives, and propagate their ſpecies; it will very much conduce to perſuade him that ſo wiſe an agent who has at command ſo many differing and excellent methods and inſtruments to accompliſh what he deſigns, and often actually employs them for the preſervation and welfare of beaſts, and even of plants, can never want means to compaſs his moſt wiſe and juſt ends, with relation to mankind; being able, by ways that we ſhould never dream of, to execute his purpoſes, and fulfil his promiſes.

Secondly, when we duly conſider the very different ends to which many of God's particular works, eſpecially thoſe that are animated, ſeem deſign'd, with reſpect both to their own welfare, and the utility of man; and how exquiſitely the great creator has been pleaſed to ſupply them with means admirably fitted to attain theſe reſpective ends; we cannot but think it highly probable, that ſo wiſe and ſo bountiful a Being has never left his nobleſt viſible creature, man, unfurniſhed with means to procure his own welfare, and obtain his true end, if he be not wanting to himſelf. And ſince man is endow'd with reaſon, which may convince him, what neither a plant, nor a brute animal is capable of knowing, that God is both his maker and continual benefactor; ſince his reaſon likewiſe teacheth him, that upon both thoſe accounts, beſides others, God may juſtly expect and require worſhip and obedience from him; ſince alſo the ſame rational faculty may perſuade him, that it well becomes the majeſty and wiſdom of God, as the ſovereign governor of the world, to give a law to man, who is a rational creature, capable of underſtanding and obeying it, and thereby glorifying the author of it; ſince, finding in his own mind a principle, which tells him, he owes a veneration, and other ſuitable ſentiments, to the divine author of his being, and his continual and munificent benefactor; ſince, on theſe accounts, his conſcience will convince him of his obligation to all the eſſential duties of natural religion; and ſince, laſtly, his reaſon may aſſure him, that his ſoul is immortal, and is therefore capable and deſirous of being everlaſtingly happy, after it has left the body; he muſt in reaſon be ſtrongly inclined to wiſh for a ſupernatural diſcovery of what God would have him believe and do. And therefore, if, being thus prepared, he ſhall be very credibly inform'd, that God hath actually been pleaſed to diſcover, by ſupernatural means, what kind of worſhip and obedience, which by reaſon alone he could but gueſs at, will be moſt acceptable to him; and to encourage man to both theſe, by expreſs promiſes of that felicity, which man, without them, can but faintly hope for; he would be ready, then, thankfully to acknowledge, that this way of proceeding becomes the tranſcendent goodneſs of God, without derogating from his majeſty and wiſdom. And by theſe and the like reflections, a philoſopher, who takes notice of the wonderful providence, that God deſcends to exerciſe for the welfare of inferior and irrational creatures, will have an advantage above men not verſ'd in the works and courſe of nature, in believing, upon the hiſtorical and other proofs which chriſtianity offers, that God has actually vouchſafed to man, his nobleſt, and only rational viſible creature, an explicite and poſitive law, enforced by ſevere penalties denounced againſt the ſtubborn tranſgreſſors, and promiſing to the ſincere obſervers of it, rewards ſuitable to his own greatneſs and goodneſs. And thus the conſideration of God's providence, in the conduct of things corporeal, may convey a well-diſpoſed mind from natural to reveal'd religion.

Experimental philoſophy draws the mind from ſenſual things.

5. Another thing, that diſpoſes an experimental philoſopher to embrace religion, is, that his genius and courſe of ſtudies accuſtom him to value and delight in abſtract truths; by which term, I here mean ſuch truths, as do not at all, or very little, gratify mens ambition, ſenſuality, or other inferior paſſions and appetites. For, as the generality of thoſe who have an averſion to religion, are led to it by a contempt of all truths, that do not gratify their paſſions or intereſts; ſo, he who is addicted to experimental knowledge, is accuſtom'd both to purſue, eſteem, and reliſh many truths, that do not delight his ſenſes, or gratify his paſſions, or intereſts; but only entertain his underſtanding with that manly and rational ſatisfaction, which is naturally afforded by the attainment of clear and noble truths,

its genuine objects and delight. And tho' the diſcoveries made by the help of phyſical or mechanical experiments, are not, for the moſt part, allied to revealed religion; yet, beſides that ſome of them manifeſtly conduce to eſtabliſh or illuſtrate natural religion, we may argue *à fortiori*, that he who is accuſtomed to prize truths of an inferior kind, becauſe they are truths, will be much more diſpoſed to value divine truths, which are of a vaſtly higher and nobler order, and of an ineſtimable and eternal advantage.

Gives it a docility.

6. But farther, both the temper of mind, that qualifies a man for a virtuoſo, and the way of philoſophizing he chiefly employs, greatly conduce to give him a ſufficient, well-grounded, and duly-limited docility; which is a great diſpoſition to the entertainment of reveal'd religion. In the vulgar and ſuperficial philoſophy, wherein a man is allow'd to think that he has perform'd his part well, when he has aſcribed things to a ſubſtantial form, or to nature, or to ſome real quality, whether manifeſt or occult, without proving that there are ſuch cauſes, or intelligibly declaring, how they produce the phenomena, or effects refer'd to them; 'tis eaſy for a man to have a great opinion of his own knowledge, and be puff'd up by it. But a virtuoſo, who cannot ſatisfy himſelf, nor dares pretend to ſatisfy others, till he can, by hypotheſes that may be underſtood and prov'd, declare intelligibly the manner of the operation of the cauſes he aſſigns, will often find it ſo difficult a task to effect all this, that he muſt eaſily diſcern, he needs further information; and therefore ought to ſeek for it where 'tis the moſt likely to be had. Beſides, the litigious philoſophy of the ſchools ſeldom furniſhes its diſciples with better than dialectical or probable arguments, which are not proper fully to ſatisfy the perſon who employs them, or to leave his adverſary without an anſwer, as probable as the objection: upon which account, men who have more wit than a ſincere love for truth, will be able to diſpute ſpeciouſly enough, as long as they pleaſe. And as ſuch ſlippery arguments are not able to convince even the perſon who uſes them, if he be a man of judgment; ſo, if he deals with a witty adverſary, they will leave him able to elude any arguments of the like nature, with which he ſhall be preſs'd. And in effect, we ſee, that in the *Ariſtotelian* philoſophy there are ſeveral eſtabliſh'd queſtions; ſuch as, Whether the elements retain their diſtinct natures in a mix'd body? Whether the celeſtial orbs are moved by intelligences? *&c.* which have been diſputed from age to age, and are like to continue queſtions for many more, if that philoſopy ſhall laſt ſo long. But a virtuoſo, who in his reaſonings attends to the principles of mathematics, and ſound philoſophy, and to the clear teſtimonies of ſenſe, or well verify'd experiments, acquires a habit of diſcerning the cogency of an argument, or way of probation; and eaſily perceives, that dialectical ſubtilties, and ſchool-tricks, cannot ſhift off its force; but he finds more ſatisfaction in embracing a demonſtrated truth, than in the vain glory of ſubtilly diſputing againſt it.

And a fitneſs for ſearching into deep truths.

7. Another thing that may, by means of experiments, diſpoſe a ſtudious ſearcher of truth for reveal'd religion, is, that his inquiries, and courſe of ſtudies, make him both willing and fit to ſearch out and diſcover

deep and unobvious truths. I have, with trouble, obſerv'd, that the greater part of the libertines among us, being men of *Pilate*'s humour, (who, when he had ſcornfully ask'd, what is truth? would not ſtay for an anſwer) with great diſdain, decline the ſtudy of all truths that require a ſerious and ſettled application of mind. Theſe men are, for the moſt part, a ſort of ſuperficial and deſultory wits, who go no further than the outſide of things, without penetrating into the receſſes of them; and, being eaſily tir'd with contemplating one, paſs quickly to another, the conſideration whereof, they, with the ſame lightneſs, forſake. And upon this account, among others, it is, that ſuch men, tho' often much applauded by others, becauſe the moſt are but ſuperficial, as well as they, almoſt as ſeldom make good philoſophers, as good chriſtians. For tho' all the ſound arguments that may be brought to evince the truth of natural and reveal'd religion, be not abſtruſe; yet ſome of the principal, eſpecially thoſe that prove the exiſtence and ſpecial providence of God, and the immortality of the ſoul, are, if not of a metaphyſical, yet at leaſt of a philoſophical nature, and will ſcarce be clearly underſtood, and duly reliſh'd, but by a perſon capable of, and accuſtom'd to attentive and prolong'd ſpeculations. Now a man addicted to proſecute diſcoveries of truths, not only by ſerious meditation, but by intricate and laborious experiments, will not eaſily be deterr'd from effectually purſuing his end, by the toils or difficulties that attend the clearing of thoſe notions, and matters of fact, whereon ſolid arguments for natural or reveal'd religion are founded, how remote ſoever thoſe truths may be from vulgar apprehenſions. In ſhort, a ſuperficial wit, ſuch as is frequently found in libertines, and often helps to make them ſuch, may be compared to an ordinary ſwimmer, who can reach but ſuch things as float upon the water; whilſt an experimental philoſopher, like a ſkilful diver, not only obtains thoſe things that lie upon the ſurface of the ſea, but makes his way to the very bottom of it, and thence fetches up pearls, corals, and other precious things, that, in ſuch depths, lie conceal'd from other men's ſight and reach.

Experimental philoſophy leads to the chriſtian religion, in particular.

We have ſeen, then, that experimental philoſophy is, in its own nature, favourable to religion in general; it alſo greatly conduces to ſhew the truth of the chriſtian religion, in particular.

This excellent religion is recommended to minds rightly diſpoſed, by a great number of prerogatives, whereof I ſhall mention a few.

1. And firſt, the three grand arguments that jointly evince the truth of the chriſtian religion in general, are, in my opinion, the excellency of the doctrine which makes it worthy to have proceeded from God; the teſtimony of the divine miracles, wrought to recommend it; and the great effects, produced in the world by it. Two of theſe three arguments are bottom'd upon matters of fact, and, conſequently, are likely to be the moſt prevalent upon thoſe who have a great veneration for experience, and are duly diſpoſed to frame ſuch pious reflections, as it warrants and leads to.

Now,

Different kinds of experience.

Now an experimental philoſopher, who is maſter of others experience, as well as of his own, and duly qualified to reflect upon both, will find ſtrong motives to the belief of chriſtianity, in the two laſt of theſe arguments of its truth. We muſt here obſerve, that the word experience may admit of ſeveral ſenſes, whereof one is far more comprehenſive than another, and likewiſe of ſeveral diviſions and diſtributions; for, beſides its more reſtrained acceptation, it is ſometimes ſet in contra-diſtinction to reaſon, ſo as to comprehend not only thoſe phenomena that nature or art exhibits to our outward ſenſes, but the things we perceive to paſs within ourſelves, and all thoſe ways of information, whereby we attain any knowledge that we do not owe to abſtracted reaſon. So that without ſtretching the word to its utmoſt extent, and to which it has been enlarg'd, it may be look'd upon as a very comprehenſive term, and fit to be branched into parts. I ſhall therefore divide it into perſonal, hiſtorical, and ſupernatural, or theological.

Perſonal.

I call that perſonal experience, which a man acquires immediately by himſelf, and which accrues to him by his own ſenſations, or the exerciſe of his faculties, without the intervention of any external teſtimony. 'Tis by this experience we know, that the ſun is bright, fire hot, ſnow cold, and white; that, upon the want of aliment, we feel hunger; that we hope for future good; that we love what we judge good, and hate what we think evil; and diſcern that there is a great difference between a triangle and a circle, and can diſtinguiſh them by it.

Hiſtorical.

By hiſtorical experience, I mean that, which tho' it were perſonal in ſome other man, is but by his relation, or teſtimony, whether immediately, or mediately conveyed to us. 'Tis by this we know that there were ſuch men as *Julius Cæſar*, and *William* the conqueror, and that *Joſeph* knew *Pharaoh* had a dream, which the *Egyptian* wiſe-men could not expound.

And theological, or ſupernatural.

Theological experience is that by which we know what (ſuppoſing a divine revelation) God is pleaſed to relate or declare concerning himſelf, his attributes, his actions, his will, or his purpoſes; whether immediately, as he ſometimes did to *Job* and *Moſes*, and conſtantly to our Saviour; or by the intervention of angels, prophets, apoſtles, or inſpired perſons; as he did to the *Iſraelites*, and the primitive chriſtian church; and ſtill does to us, by thoſe written teſtimonies we call the ſcriptures.

By perſonal experience we know that there are ſtars in the heavens; by hiſtorical experience we know that there was a new ſtar ſeen by *Tycho*, and other aſtronomers, in *Caſſiopeia*, in the year 1572; and by theological experience we know that the ſtars were made on the fourth day of the creation.

I do not therefore here take experience in the ſtricteſt ſenſe of all, but in a greater latitude, for the knowledge we have of any matter of fact; which, without owing it to our reaſoning faculty, we either acquire by the immediate teſtimony of our own ſenſes and other faculties, or it accrues to us by the communicated teſtimony of others. And even in common acceptation, the word experience is not always meant of that which is immediate, but is often taken in a latitude. As when we ſay, experience teaches us, who, perhaps, were never out of *England*, that the torrid zone is inhabited; and perſuades learned men, who had opportunity to make nice aſtronomical obſervations, that ſtars may be generated, and periſh; or, at leaſt, begin to appear, and then diſappear in the celeſtial region of the world. On this kind of hiſtorical experience, conſiſting of the perſonal obſervations of *Hippocrates*, *Galen*, and other phyſicians, tranſmitted to us, a great part of the practice of phyſic is founded. And the moſt rational phyſicians take, as matters of fact, not only what other phyſicians have left upon record, but many preſent things, which themſelves can know but by the relation of their patients; as, that a man has a particular antipathy to ſuch a thing, which the doctor, perhaps, judges fit for him to uſe; or, that a woman with child, longs for this, or that determinate thing. And, phyſicians reduce theſe, and the like matters of fact, to experience, as to one of the two columns of phyſic, diſtinguiſhed from reaſon.

Since, then, learned men, as well as cuſtom, confine not the application of the word experience, to that which is perſonal, but employ it in a far greater latitude; I ſee not, why that, which I call theological experience, may not be admitted; ſince the revelations that God makes concerning what he has done, or purpoſes to do, are but teſtimonies of things, moſt of them matters of fact, and all of them ſuch, as, (ſo far as they are merely revelations) cannot be known by reaſoning, but by teſtimony; whoſe being divine, and relating to theological ſubjects, does not alter its nature, tho' it give it a peculiar and ſuper-eminent authority.

To apply this diſtribution of experience, to the matters of fact, that recommend the credibility of the chriſtian religion, I ſhall offer my thoughts in two diſtinct propoſitions.

PROP. I.

We ought to believe ſeveral things upon the information of experience, mediate and immediate, which, without that information, we ſhould judge unfit to be credited; or, antecedently to it, actually judged contrary to reaſon.

This propoſition may be underſtood, either of perſons, or of things, and will hold true in both.

And, firſt, as to perſons; if our own obſervation of what occurs among mankind, does not ſatisfy us, that we are oblig'd, after ſufficient tryal, frequently to alter the opinions, which, upon probable reaſons, we had before entertain'd, of the fidelity, prudence, juſtice, chaſtity, &c. of this or that perſon; we need only turn to the records of hiſtory, or appeal to the tribunals of judges. For, in both we find but too many inſtances and proofs from matters of fact, that perſons look'd on, even by intelligent men, as honeſt, virtuous, and holy, have prov'd falſe, perfidious, diſloyal, unjuſt, ſacrilegious, perjur'd, &c. And, in the courts of juſtice, we find a great deal of time employ'd to detect not only civil tranſgreſſions;

as thefts, cheats, forgery, falſe-witneſs, adultery, and the like, perpetrated by thoſe, who, before they were throughly ſifted, paſs'd for honeſt; but, even ſins againſt nature; as, the murders of parents by their children, and children by their parents, *&c.* Whence 'tis plain, that we ought, upon the teſtimony of experience, to change the opinions we thought we had rationally taken up of perſons. I now proceed to make good the propoſition, in the ſenſe I chiefly intended, which is, as it relates to things.

If experience did not inform and certify us, who would believe, that a little heap of light, black grains of matter, ſhould be able to over-turn ſtone-walls, blow up whole caſtles and rocks themſelves, and do thoſe other ſtupendous things, that we ſee actually perform'd by gun-powder, made uſe of in ordinance, and in mines? Who would think, that two or three grains of opium, ſhould ſo ſtupify a large human body, as to force a ſleep; and often, even without that, ſuſpend the ſharpeſt torments in the cholic, gout, and other the moſt painful diſeaſes, in patients of quite different ages, ſexes, and conſtitutions; in whom alſo the diſeaſes are produc'd by differing, or even by contrary cauſes? Who would believe, that the poiſon adhering to the tooth of a mad dog, tho', perhaps, ſo little, as to be ſcarce diſcernible by ſenſe, ſhould be able, after the ſlight hurt is quite healed, to continue in the warm, perſpirable body of the bitten perſon, not only for ſome days, or months, but, ſometimes, for very many years? And, after having lurk'd all that while, without giving any trouble to the patient, that it ſhould, on a ſudden, pervert the whole œconomy of his body, and produce a madneſs like that of the dog which bit him, diſcovering itſelf by that ſtrange and fatal ſymptom, an *Hydrophobia?*

But, beſides a multitude of inſtances that may be given of truths, which, were it not for experience, we ſhould refuſe to believe; becauſe the ſmall ſtrength of ſuch agents, ſeems altogether diſproportionate to the effects aſcribed to them; many other inſtances might be alledg'd, wherein we aſſent to experience, even when its informations ſeem contrary to reaſon, and that which, perhaps, we did actually, and, without ſcruple, take to be true.

Since gravity is the principle, that determines falling bodies to move towards the center of the earth; it ſeems very rational to believe, with the generality of philoſophers, that, in proportion as one body is more heavy than another, ſo it ſhall fall to the ground faſter than the other. Whence it has been inferr'd, that of two homogeneous bodies, whereof one, for example, weighs ten pounds, and the other but one pound; the former, being let fall from the ſame height, and at the ſame time with the latter, will reach the ground ten times ſooner.

But, notwithſtanding this plauſible reaſoning, experience ſhews us, (and I have purpoſely try'd it) that bodies of very unequal weight, let fall together, will reach the ground at the ſame time; or ſo near it, that 'tis not eaſy to perceive any difference in the velocity of their deſcent, from a moderate height.

Physics.

'Tis generally taken for granted, by naturaliſts, as well as others, that ſtrong and loud ſounds, as they are heard much farther off, than fainter; ſo if the ſonorous bodies be equally diſtant from the ear, the very ſtrong ſound will arrive much ſooner at it than the other; yet, by the experiments of the moderns about the velocity of ſounds, (in making many of which I have endeavour'd at accuracy) it appears, that weaker ſounds are (at leaſt as to ſenſe) tranſmitted thro' the air as ſwiftly as the ſtronger. And indeed 'tis often obſerv'd, that when cannons and muskets are diſcharg'd together, the reports of both arrive together at the ear.

It ſeems irrational to conceive, that a ſmaller and weaker load-ſtone ſhould draw away a piece of ſteel from a larger and ſtronger; yet my experience (and that of others) proves, that in ſome caſes this paradox is a truth.

It has generally, by philoſophers, as well as other men, been look'd upon as manifeſt, and agreeable to reaſon, that cold condenſes water, more or leſs, according to its degree; and, conſequently, that ice is water reduced into a leſs bulk. But 'tis plain, by experiments carefully made, that water is, by glaciation, rather expanded, or at leaſt that ice takes up more room than the water did before it was congeal'd. And of this ſort of inſtances it were eaſy to add a vaſt number.

And to extend the force of our arguments to that experience which is not immediate, or perſonally our own, but communicated by others, provided it be competently atteſted, and duly convey'd to us; there will need but a little reflection on what is judg'd reaſonable, and freely practis'd, by philoſophers themſelves. For how many concluſions have the modern naturaliſts admitted, tho' abſtract reaſoning never led men to make them, and even while plauſible arguments, and the notions and axioms of the moſt generally receiv'd philoſophy, were contrary to them? Thus, that in the heavens there ſhould be generations and corruptions, was not only unobſerv'd before the time of *Ariſtotle*, but is contradicted by his arguments; yet I, with many others, have ſeen great ſpots (perhaps bigger than *Europe)* generated and diſſipated on or near the ſurface of the ſun: and ſeveral of the modern philoſophers and aſtronomers, having never beheld any of theſe, muſt take the phenomena upon the credit of thoſe who have obſerv'd them. And much more muſt they do ſo, who, in ſpite of the vulgar philoſophy, which made all comets ſublunary, believe they are celeſtial bodies. For that they appear above the concave of the moon's orb, we credit upon the affirmation of thoſe who obſerv'd them; which very few have done themſelves. And the height of the famous comet, or diſappearing ſtar, in *Caſſiopeia*, in the year 1572, whereon ſo much ſtreſs is laid by our philoſophers and mathematicians, is admitted and urged, chiefly from the opinion they have, not only of *Tycho*'s veracity, but his ſkill in obſerving the motions and phenomena of that celeſtial light, and particularly its having no parallax.

In ſhort, the great architect of experimental hiſtory, Sir *Francis Bacon*, when he divides it but into three parts, aſſigns the ſecond of them to what he calls preter-generations; ſuch as monſters, prodigies, and other things; which being, as to us, but caſualties, all thoſe that happen'd in other times and places than we have lived in, we muſt take upon the credit of others. And yet theſe mediate experiments, by ſuggeſting new inſtances of nature's power, and uncommon ways of working, and by overthrowing or limiting received rules and traditions, afford us a conſiderable and inſtructive part of natural hiſtory, without which, it would not be either ſo ſound, or ſo compleat.

PROP. II.

We ought to have a great and particular regard to thoſe things that are recommended to our belief, by what we have reduced to real, tho' ſupernatural experience.

For, 1. 'Tis manifeſt, that the moſt rational men ſcruple not to believe, upon competent teſtimony, many things, whoſe truth did no way appear to them, by conſidering the nature of the things themſelves; nay, tho' what is thus believ'd upon teſtimony, be ſo ſtrange, and, ſetting aſide that teſtimony, would ſeem ſo irrational, that antecedently to it, the things, at laſt, admitted as truths, were actually rejected as errors, or judg'd altogether unfit to be believ'd. And the points wherein experience over-rules that, which, before it happen'd, was judg'd to be moſt agreeable to reaſon, concern things merely natural, or civil, whereof human reaſon is held to be a proper judge; whereas many of the points recommended by ſupernatural experience, concern things of a ſuperior order, many of which are not to be adequately eſtimated by the ſame rules with things merely corporeal or civil, and ſome of which, as the eſſence and manner of exiſtence, and ſome peculiar attributes of the infinite God, involve or require ſuch a knowledge of what is infinite, as vaſtly exceeds the reach of our limited underſtanding.

But this is not all: for, 2. As 'tis with juſtice generally granted, that the better qualified a witneſs is, in the capacity of a witneſs, the ſtronger aſſent his teſtimony deſerves; ſo we ought, of all the things that can be recommended to us by teſtimony, to receive thoſe with the higheſt degree of aſſent, that are taught us by God, thro' the means of thoſe perſons who appear to have been commiſſioned by him to declare his mind to men. For the two grand requiſites of a witneſs, being the knowledge he has of the things he delivers, and his faithfulneſs in truly delivering what he knows; all human teſtimony muſt, on theſe accounts, be inferior to divine teſtimony: ſince the latter is warranted both by the veracity of God, and by his boundleſs knowledge; which makes it as impoſſible he ſhould be deceiv'd himſelf, as the other does, that he ſhould deceive us. And, becauſe that, for the delivery of divine teſtimony, it has often pleas'd God, who is a moſt free, as well as a moſt wiſe agent, to make uſe of unpro-

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miſing perſons as his inſtruments; I ſhall not altogether overlook this circumſtance, that an experimental philoſopher ſo often increaſes his knowledge of natural things, by what he learns from the obſervations and practices, even of mean, and, perhaps, of illiterate perſons, becauſe they are converſant with the works of nature; that he is not only willing to admit, but often curious to ſeek for informations from them: and therefore he is not likely to ſhew much reluctance in receiving the doctrines of revealed religion, ſuch as Chriſtianity, if the teachers of it were honeſt men, and had opportunity to know the truth of the things they deliver; tho' they were of ſome mean profeſſion.

And, indeed, ſuch a perſon as our virtuoſo, will, with great willingneſs, exerciſe himſelf in peruſing, with attention, and much regard, the writings of the apoſtles, evangeliſts, and ancient prophets; notwithſtanding any meanneſs of their firſt condition, or of their ſecular employments. And here, he will not only readily ſuffer himſelf to be inſtructed in the grand and general articles of religion, which, becauſe of their neceſſity, or very great uſefulneſs, are to be met with in many places, and in variety of expreſſions, by honeſt and duly-diſpoſed readers; but he will, inſtead of diſdaining ſuch tutors, both expect, and carefully ſtrive, to improve his knowledge of divine things in general, even by thoſe hints, and incidental paſſages, that a careleſs or ordinary reader would overlook, or not expect any thing from. For, as the fertility of the ſcriptures is not uſually enough diſcerned by vulgar readers, when the ſacred writers tranſiently touch upon a great many ſubjects, that they do not expreſly handle; ſo, the docility we have aſcribed to our virtuoſo, will make him repoſe a great deal of truſt in the teſtimony of inſpired Perſons, ſuch as Chriſt and his apoſtles, about things of all ſorts, whether uſually taken notice of, or not, that relate to objects of a ſupernatural order; eſpecially if among theſe, God himſelf, and his purpoſes be comprized: ſince ſeveral of thoſe things are not knowable without revelation, and others are beſt known by it. And to be allow'd to ground a belief about ſuch things, on the relations and other teſtimonies of thoſe that were "eye-witneſſes and miniſters" of the things they ſpeak of, will, by our virtuoſo, be juſtly reputed ſuch an advantage, in order to the knowledge of things divine; as the conſulting with navigators and travellers to *America*, is, to a perſon curious to learn the ſtate of that new world. For an ordinary ſea-man or traveller, who had the opportunity, with *Columbus*, to ſail along the ſeveral coaſts of it, and paſs up and down thorough the country, was able at his return to inform men of a hundred things, that they would never have learn'd by *Ariſtotle*'s philoſophy, or *Ptolemy*'s geography; and might not only acquaint them with many particulars, agreeable to the opinions which their receiv'd phyſics and coſmography ſuggeſted, but alſo rectify ſeveral erroneous preſumptions and miſtakes, which, till then, they thought very agreeable to the dictates of thoſe ſciences, and to reaſon. And as one, who had a candid and knowing friend, intimate with *Columbus*, might better rely on his informations about many particulars of the natural hiſtory of thoſe parts, than on thoſe of a hundred ſchool-philoſophers, who knew only what they learned from *Ariſtotle*, *Pliny*, *Ælian*, and the like ancient naturaliſts; much more may we rely on the accounts given us of divine things, by the apoſtles, and conſtant attendants of him who lay in the "boſom of God his father," and commiſſion'd them to declare to the world the "whole counſel of God," as far as 'twas neceſſary for man to know.

Fuller trials are allow'd, among ingenious men, to rectify the informations of the more imperfect ones; and therefore I ſhall add, that, tho' the innate notions and ſentiments, which nature gives us of the attributes and mind of God, be highly to be prized; yet the informations that theological experience affords of thoſe abſtruſe things, is far more excellent and compleat.

I have dwelt the longer upon the miracles that may be pleaded to recommend the chriſtian religion, becauſe I thought, that an argument grounded on them is little leſs than abſolutely neceſſary, to prove that any religion men believe to be ſupernaturally reveal'd, really proceeds ftom God.

For, tho' the excellency of the chriſtian doctrine, and other concurrent motives, may juſtly perſuade me, that 'tis worthy and likely to be given by God; yet that, in fact, this doctrine comes from him by way of ſupernatural revelation, I can ſcarce be ſufficiently aſcertained, but by the miracles wrought by Chriſt and his diſciples, to evince, that the doctrine they preach'd, as commiſſion'd by God to do ſo, was indeed his; being, as ſuch, own'd by him. But theſe miracles having been wrought in the firſt ages of the church; we can have no knowledge of them by our own ſenſes, or immediate obſervation, but muſt take them upon the credit of hiſtorical experience, which is afforded us by the duly tranſmitted teſtimony of thoſe, who were themſelves eye-witneſſes of the things they relate. And ſince we ſcruple not to believe ſuch ſtrange prodigies, as celeſtial comets, vaniſhing and re-appearing ſtars, iſlands founded by ſubterraneal fires in the ſea, the darkneſs of the ſun for many months together, earthquakes reaching above a thouſand miles in length, and the like amazing irregularities of nature, upon the credit of human hiſtories; I ſee not, why that hiſtorical experience ſhould not more be truſted, which has many peculiar and concurrent Circumſtances to confirm it; and particularly the death that moſt of the firſt promulgators chearfully ſuffer'd to atteſt the truth of it; and the ſucceſs and ſpreading of the doctrine authorized by thoſe miracles, and receiv'd chiefly upon their account. To which things, ſome perhaps would add, that 'tis leſs incredible, that the author of nature ſhould, for moſt weighty purpoſes, make ſtupendous alterations of the courſe of nature; than that nature herſelf, for no ſuch end, ſhould, by ſuch prodigies, as were lately mention'd, throw herſelf, as it were, out of her own courſe.

Miracles being ſo neceſſary to the eſtabliſhment of reveal'd religion in general, it may be look'd upon as a farther diſpoſition in our virtuoſo to receive the chriſtian religion, that the philoſophy he cultivates, much conduces to enable him to judge aright of thoſe ſtrange things, that are by many propoſed as miracles, and believed to be ſo.

For, firſt, the knowledge he has of the various and wonderful operations of ſome natural things, eſpecially when they are skilfully improv'd, and dextrouſly apply'd by art, particularly in mathematics, mechanics, and chymiſtry, will qualify him to diſtinguiſh between things that are only ſtrange and ſurprizing, and thoſe that are truly miraculous: ſo that he will not miſtake the effects of natural magic for thoſe of a divine power. And by this means he will be able to diſcover the ſubtile cheats, and colluſions of impoſtors, whereby not only the multitude of all religions, eſpecially the heathen, but even learned men of moſt others, for want of an inſight into real philoſophy, have formerly been, or are at this day, deluded, and drawn into idolatrous, ſuperſtitious, or otherwiſe erroneous tenets, or practices.

On the other ſide, the knowledge our virtuoſo may have of what cannot be juſtly expected or pretended from the mechanical powers of matter, will enable him to diſcern that many things are not producible by them, without the intervention of an intelligent ſuperior power; on which account he will frankly acknowledge, and heartily believe ſeveral effects to be truly miraculous, that may be plauſibly enough aſcribed to other cauſes in the vulgar philoſophy; where men are taught to attribute ſtupendous and unaccountable effects to ſympathy, antipathy, *Fuga vacui*, ſubſtantial forms, and, eſpecially, to a certain Being, preſumed to be almoſt infinitely potent and wiſe, which they call nature: for this is repreſented as a kind of goddeſs, whoſe power may be little leſs than boundleſs. Thus I remember *Galen* himſelf compares it to that of God, and ſaith, that he could not do ſuch a thing, becauſe nature could not; and cenſures *Moſes* for ſpeaking as if he were of another mind.

I know it may be objected, that thoſe who are ſo well acquainted with the myſteries of nature, and her various and ſtrange ways of working, as our virtuoſo is ſuppoſed to be, may, by that knowledge, be ſtrongly tempted to think, that thoſe ſurprizing things other men call miracles, are but effects of her power, the extent of which is not eaſily diſcern'd by ordinary men, nor ſafely defined by philoſophers themſelves. To this I ſhall only here reply, that to make it reaſonable to judge a particular performance ſupernatural, it is not at all neceſſary that it ſurpaſs the whole power of nature, that is, of phyſical agents, provided it ſurpaſs the power of that cauſe, or that complex of cauſes, from which the effect muſt in reaſon, if it be purely natural, or phyſical, be ſuppoſed to have proceeded. For inſtance, that a fiſherman or two ſhould ſpeak other languages than their own, does not at all exceed the power of nature, if they employ'd a competent time in learning them: but, that a great number of fiſhermen, and other illiterate perſons, ſhould, all on a ſudden, become linguiſts, and, in an hour's time, be able to ſpeak intelligibly to a great number and variety of nations, in their reſpective languages, as the new teſtament relates, that the apoſtles, and their companions, did on the day of pentecoſt; this gift of tongues, I ſay, was an ability which in thoſe circumſtances of place, time, and perſons, wherein 'twas exerciſed, may juſtly be concluded to have been ſupernatural, or miraculous.

To proceed; 'tis a notorious matter of fact, that, in leſs than half an age, the chriſtian religion was ſpread over a great part of the then known world; ſo that in a few years after it began to be preach'd, the apoſtle of the *Gentiles* could tell the *Romans*, with joy, "that their faith was ſpoken "of throughout the whole world." And in the ſecond century *Tertullian*, and other famous writers, ſhew, that the goſpel had already numerous proſelytes in a great number of different kingdoms and provinces. This wonderful quick progreſs of the chriſtian religion being aſcertain'd to our virtuoſo, from a thing whereby he is ſo much ſway'd, as experience; it greatly diſpoſes him to believe the truth of ſo prevalent a religion. For if he conſiders the perſons who firſt promulgated it, they were but half a ſcore of illiterate fiſhermen, a few tent-makers, and other tradeſmen. If he conſiders the means employ'd to propagate this doctrine, he finds, that they had neither arms, nor external power, to compel men to receive it; nor riches, honours, or preferments, to bribe or allure them to it; nor were they men of philoſophical ſubtilty, to intrap or entangle the minds of their auditors: nor did they make uſe of the pompous ornaments of rhetoric, and gloſſes of oratory, to inveigle, or entice men; but treated of the moſt ſublime and abſtruſe matters in a moſt plain and unaffected ſtyle, as became lovers and teachers of truth. If he conſiders the nature of the doctrine that in a little time obtain'd ſo many proſelytes, he will find, that, inſtead of being ſuited to the natural apprehenſions, or the receiv'd opinions of men, and inſtead of gratifying their corrupt affections, or complying with ſo much as their moſt innocent intereſts; it preſcribed ſuch mortifications, and ſuch great ſtrictneſs of life, and high degrees of virtue, as no legiſlator had ever dar'd to impoſe upon his ſubjects, nor any philoſopher on his diſciples. And this doctrine was propoſed in ſuch a way, and was accompany'd with predictions of ſuch hardſhips and perſecutions, that ſhould, in thoſe times, be the portion of its ſincere profeſſors; as if the law-giver had deſign'd rather to fright men from his doctrine, than allure them to it: ſince they could not believe what he ſaid, and foretold, to be true, without believing that they ſhould be made great ſufferers by that belief. If our virtuoſo conſiders the oppoſition made to the progreſs of the goſpel, he will find cauſe to wonder that it could ever be ſurmounted. For the heathens, which made by far the greateſt part of the world, were deeply engaged in polytheiſm, idolatry, magical rites, and ſuperſtitions, and almoſt all kind of crimes; and ſome of theſe were ſhameleſs debaucheries, which oftentimes made a part of their worſhip. And the *Jews*, by the corrupt leaven of the phariſees, the impious errors of the ſadduces, and the general miſtakes of the nation about the perſon, office, and kingdom of the Meſſias; and by their dotage upon their vain traditions, and numerous ſuperſtitions, grounded upon them, were, on theſe and other accounts, highly indiſpoſed, as well as the *Romans*, the two nations to be converted, to be made proſelytes:

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eſpecially when they could not own themſelves to be ſuch, without expoſing their perſons to be hated and deſpiſed; their poſſeſſions to be confiſcated; their bodies to be impriſon'd and tormented; and oftentimes their lives to be, in an ignominious and cruel manner, deſtroy'd. And, whilſt the ſecular magiſtrates made them ſuffer all theſe hardſhips, the venerated prieſts, the ſubtile philoſophers, and the eloquent orators, perſuaded the world, that they deſerv'd yet more than they endured; and employ'd all their learning and wit to make the religion odious and ridiculous, as well as the embracers of it miſerable: accuſing the martyrs, and other chriſtians, of no leſs than atheiſm, inceſt, and the inhuman ſhedding and drinking the innocent blood of infants. Theſe, and the like matters of fact, when our virtuoſo reflects on, and conſiders by what unpromiſing means ſuch ſeemingly inſurmountable difficulties were conquer'd; he cannot, by this hiſtorical experience, be inclined to think, that effects, ſo diſproportionate to the viſible means, could be brought to paſs, without the peculiar aſſiſtance, and extraordinary bleſſing of God; by whom thoſe ſucceſsful preachers averr'd themſelves to be commiſſioned. For, that the ſupernatural help which the chriſtian doctrine appears to have had, was divine, not diabolical, will ſeem evident to our virtuoſo, from the nature, tendency, and effects of the doctrine itſelf; which expreſly teacheth, that there is but one God; that he alone is to be worſhipped, and not idols, nor any of the heathen demons, or deities; that the devils are wicked, apoſtate, malicious, and miſerable creatures, hated of God, and who extremely hate mankind; and that thoſe vices, as well as rites of worſhip, eſtabliſhed by them in the world, were abominable to God, and would be, by degrees, deſtroy'd by him: as, in effect, they ſoon began to be in many places of the world, where the worſhippers of Chriſt caſt the devil out of his temples, out of mens veneration, and, oftentimes, out of their bodies too.

One circumſtance there is of the propagation of the goſpel, which, tho' it may ſeem more extrinſecal than thoſe hitherto mention'd, is yet too conſiderable to be here omitted; I mean, that the quick ſpreading, and ſucceſs of the chriſtian doctrine in the world, was foretold both by the prophets of the old teſtament, and the author and promulgators of the new. For, it being notorious, that many errors and ſuperſtitions, have, with too much celerity, been ſpread far and wide in the world; either by reputed mere accidents, that were very favourable to them, or by the induſtry and artifices of men; it ought to be no ſmall ſatisfaction to equitable judges, that the ſudden progreſs, and notable effects of the chriſtian religion, were foretold by the ancient prophets, and by the Meſſias and his apoſtles. For, by theſe accompliſh'd predictions, it may appear, that the wonderful ſucceſs of the goſpel was not an effect of chance, but was, long before, determin'd by divine providence, to be accompliſh'd in a wonderful manner, by his peculiar aſſiſtance.

But true prophecies of unlikely events, fulfill'd by unlikely means, are ſupernatural things; and, as ſuch, may properly be reckon'd among miracles. We might add, that theſe have a peculiar advantage above moſt other miracles, on the account of their duration: ſince the manifeſt proofs of the predictions continue ſtill, and are as viſible as the extent of the chriſtian religion; and ſome of them are ſtill more and more accompliſh'd, by the converſions made of multitudes of infidels, in ſeveral vaſt regions of *America*. So that if we may call ſome tranſient miracles, ſuch as the turning water into wine, at a marriage-feaſt in *Galilee*; and the darkening of the ſun when the moon was in the full, at the crucifixion of Chriſt; accompliſh'd predictions may be ſtyled permanent ones; and their difference may appear by the differing ſtates of the *Moſaic* manna: for, tho' both that which fell daily (except on the ſabbath) in the wilderneſs, and that which was laid up in a pot before the teſtimony, were ſupernatural productions; yet, tho' a portion of the former out-laſted not two or three days; that kept in the pot, was preſerv'd for many ages, and continu'd to be (as it was foretold it ſhould) a viſible miracle.

Beſides, the preachers of the chriſtian religion, both pretended and appeal'd to miracles, as proofs of the truth of their doctrine: and if we conſider the great diſadvantages they lay under, and the powerful oppoſition of all ſorts, that they met with and ſurmounted; it cannot reaſonably be thought, that ſuch unlikely men ſhould ſo ſucceſsfully preach ſo uninviting a doctrine, unleſs it were confirm'd by conſpicuous miracles. Or, at leaſt, if ſo uneaſy and perſecuted a religion, was propagated without miracles, that propagation itſelf may juſtly paſs for a miracle; and be no leſs fit than another, to confirm the religion ſo admirably propagated.

Thus, I think, I have ſhewn, that a virtuoſo has ſome helps, which other men, generally ſpeaking, want, to make him judiciouſly approve the arguments for the truth of the chriſtian religion, grounded on the miracles wrought in its favour, and the wonderful ſucceſs of it in the world. But, becauſe a reveal'd religion, how true ſoever, can ſcarce be proved but by moral demonſtrations; and becauſe it is not, therefore, always ſufficient, that the arguments be good in their kind; but there are ſome qualifications requir'd in the minds of thoſe who are to be convinc'd by them; I ſhall now add, that experimental philoſophy alſo diſpoſes the mind of its cultivator to receive due impreſſions from ſuch proofs, as miracles, as well as other topics, afford the chriſtian religion.

Another thing, then, that qualifies an experimental philoſopher, for the reception of reveal'd religion, is, the habit of endeavouring to give clear explanations of the phenomena of nature, and to diſcover the weakneſs of thoſe ſolutions which ſuperficial wits uſually make and acquieſce in, inſenſibly works in him a great and ingenuous modeſty of mind. And, on account of this intellectual, as well as moral virtue, he will not only be very inclinable, both to deſire and admit further information, about things which he perceives to be dark, or abſtruſe; but he will be very unapt to take, for the adequate ſtandard of truth, a thing ſo imperfectly inform'd, and narrowly limited, as his mere, or abſtracted reaſon. And tho' a vulgar philoſopher, who allows himſelf to refer the obſcureſt things in nature, to ſubſtantial forms, real qualities, ſympathy, antipathy, *&c.* which need not, and perhaps cannot be clearly underſtood; and thence preſumes that he underſtands every thing, and that thoſe things muſt be falſe, or impoſſible, which agree not with his philoſophy; yet a ſober and experienc'd naturaliſt, who knows what difficulties remain unſurmounted in the conception and explanations even of things corporeal, will not, by a lazy or arrogant preſumption that his knowledge about things ſupernatural is already ſufficient, be induced to reject or to neglect any information that may increaſe it.

And this frame of mind is a very happy one for a ſtudent in reveal'd religion, where cautiouſneſs is not more neceſſary for the avoiding of errors, than docility is advantageous in learning of truth; ſince the knowledge and goodneſs of the divine teacher is ſuch, that a ſcholar, to improve his intellect, needs but bring a mind fitted to receive the genuine informations that are moſt liberally offer'd, and will never deceive him who employs a due care not to miſtake the meaning of them.

A well-cultivated converſation with the works of God, brings a man to diſcover, from time to time, ſo many things to be feaſible, or true, which, whilſt he argued but upon the ſhallow grounds of uninform'd reaſon, he judg'd falſe, or unpracticable; that by degrees he acquires a habit of receiving ſome ſorts of opinions, and eſpecially thoſe that ſeem unfavourable to religion, with a diſpoſition to reform or diſcard them, upon further information. And this, as he is reſolv'd to ſubmit to, in caſe he meets with it, ſo he is diſpos'd to receive it, by having often found himſelf obliged, upon ſubſequent information, to mend, or lay aſide his former opinions, tho' very agreeable to the beſt light he had to judge by when he entertain'd them. Thus, tho' it ſeems a viſible truth that the diſc of *Venus* is, in all reſpects to the ſun, totally luminous; yet, when the teleſcope diſcovers her to have her full and her wane, like the moon, he will believe this further obſervation, againſt the firſt, made with his naked eyes.

And, indeed, I have ſometimes doubted, whether to be verſ'd in mathematics, and other demonſtrative parts of philoſophy, brings a greater advantage to the mind, by accuſtoming and aſſiſting it ſtrictly to examine things propoſed for truths, and ſtrongly to evince the truths a man knows, to others; than by fitting him to diſcern the force of a good argument, and ſubmit willingly to truths clearly proved, how little ſoever he may have expected to find ſuch concluſions true. 'Twill not be difficult to apply theſe reflections to our preſent purpoſe, ſince there are ſeveral paſſages in the ſcripture that ſufficiently declare, both that multitudes perſiſt in a criminal infidelity, out of a fond conceit of their own knowledge, and a readineſs to be ſway'd rather by ſtrong prejudices, than by the ſtrongeſt arguments that would remove them; and that docility is a very happy diſpoſition to the entertainment of reveal'd religion. And this qualification will be the more eaſily found in our virtuoſo, becauſe tho' the things about which he has long been ſenſible of his ignorance, or deſires further inſtruction, are within the ſphere of nature, and the juriſdiction of philoſophy; many of the things which reveal'd religion declares, are ſo ſublime and abſtruſe, that they may well be look'd upon as of an higher order than merely phyſical ones, and therefore cannot be ſatisfactorily reach'd by the mere light of nature. 'Tis true, our philoſopher will examine more ſtrictly than ordinary men, the proofs brought for this or that propoſed revelation; but that is no diſadvantage to a ſupernatural religion, ſuch as the chriſtian, if it be true: and the real truth about religion itſelf does not require credulity, but only docility. If a piece of coin, that paſſes for true gold, be offer'd to an ordinary man, and to a skilful refiner, the latter will indeed examine it more ſtrictly, and not acquieſce in the ſtamp, the colour, the ſound, and other obvious marks, that may ſatisfy a ſhop-keeper, or a merchant; yet, when he has try'd it by the ſeverer ways of examining, ſuch as the touch-ſtone, the cupel, *Aqua fortis*, &c. and finds it to hold good in thoſe proofs, he will readily and frankly acknowledge that 'tis true gold, and be more thoroughly convinc'd of it than the other perſon; whoſe want of skill will make him ſtill apt to retain a diſtruſt, and render him, indeed, more eaſy to be perſuaded, but more difficult to be fully ſatisfy'd. Thus, tho' our virtuoſo will examine with more ſtrictneſs and skill than ordinary men are able, miracles, prophecies, or other proofs, ſaid to be ſupernatural, that are alledg'd to evince a reveal'd religion; yet if the certain and genuine characters of truth appear in them, he will be more thoroughly convinc'd of it, than a leſs skilful man, whoſe want of good criteria, and ſound judgment, incline him to be diffident, and to be ſtill afraid of having been impoſed on.

I expect it will be here objected, that I degrade the human underſtanding, by aſcribing ſo much to experience, natural or ſupernatural, that it has left nothing for reaſon to do, but ſervilely to obey.

This objection, indeed, is plauſible; yet the anſwer to it will not be very difficult, if the matter itſelf be duly conſider'd, and reaſon be brought to act, not as an intereſted party, but an unbyaſs'd judge.

For we have already ſhewn, that rational philoſophers ſcruple not to alter or renounce the opinions which ſpecious reaſons had ſuggeſted to them, when once they either find thoſe opinions contradicted by experience, or meet with other opinions more conformable to it. And *Ariſtotle* himſelf, tho' he be accuſed of having wreſted phyſics to logical and metaphyſical fancies, confeſſes, not only that in the ſcience of nature, reaſon ought to comport with the phenomena, and the phenomena with reaſon; but that to adhere to plauſible reaſonings, and neglect ſenſible obſervations, is a weakneſs or diſeaſe of the mind. And whether the underſtanding be, as *Ariſtotle* taught, like blank paper, and receives no knowledge but what has been convey'd to it thro' the ſenſes; or whether the notions are congenite with the underſtanding, or ſo eaſily and early acquired by it, as to appear innate; they are but very few, in compariſon of thoſe that are requiſite to judge aright about any one thing, that occurs either in

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in natural philoſophy, or theology. For, in the divine nature, power, wiſdom, and other attributes, there is an exuberance that has produced numberleſs contrivances, laws, and other things, which exceedingly ſurpaſs both the number and variety that the dim and limited intellect of man could reach to, by framing, and compounding ideas, without the aſſiſtance of the patterns afforded by the works and declarations of God.

On account of the ſame prerogative of the divine knowledge, it muſt frequently happen, that the notions and opinions men take up of the works and mind of God, upon the mere ſuggeſtions of abſtract reaſon; will not only be almoſt conſtantly very deficient, but often very erroneous. Of this we ſee evident proofs in many opinions of the old philoſophers; who, tho' men of ſtrong natural parts, were miſ-led, by what they miſtook for reaſon, to maintain ſuch things about the works, and the author of nature, as we, who, by the favour of experience and revelation, ſtand in a much clearer light, know to be falſe, and often juſtly think utterly extravagant.

Abſtract reaſon is a narrow thing, and reaches but to a very ſmall ſhare of the knowable truths, whether human or divine; which may be obtain'd by the help of further experience, and ſupernatural revelation. This reaſon, furniſh'd with no other notices than it can ſupply itſelf with, is ſo ſhallow and deceitful, that he who ſeeks for knowledge only within himſelf, ſhall be ſure to be quite ignorant of far the greateſt part of things; and will ſcarce eſcape being miſtaken about a large part of thoſe he thinks he knows.

I am far from intending to deny reaſon any of its juſt prerogatives; experience itſelf is but an aſſiſtant to reaſon, and ſupplies informations to the underſtanding; which ſtill remains the judge, and has the power, or right, to examine, and make uſe of the teſtimonies that are preſented to it. The outward ſenſes are but the inſtruments of the ſoul, which hears by the intervention of the ear, and in reſpect of which, the eye itſelf is but a more immediate optical tube; and the ſenſe does but perceive objects, not judge of them. Nor do the more wary among the philoſophers truſt their eye, to teach them the nature of the viſible object; but only employ it to perceive the phenomena it exhibits, and the changes that happen to itſelf by the action of it. 'Tis confeſs'd too, that the ſenſes may deceive us, if the requiſites of ſenſation be wanting; as, when a ſquare tower appears round at a diſtance, and a ſtrait ſtick, half in the water, appears crooked: 'tis, therefore, the part of reaſon, not ſenſe, to judge whether none of the requiſites of ſenſation be wanting; which, by the way, often requires, not only reaſon, but philoſophy: and then, alſo, 'tis the part of reaſon to judge what concluſions may, and what cannot, be ſafely grounded on the informations of the ſenſes, and the teſtimony of experience. So that, when we ſay, experience corrects reaſon, 'tis an improper way of ſpeaking; ſince 'tis reaſon itſelf, that, upon the information of experience, corrects the judgment it had made before.

To illuſtrate the uſe of reaſon, let us ſuppoſe an able judge coming to hear, and decide cauſes, in a ſtrange country: 'tis plain, that the general notions he brings with him, and the dictates of juſtice and equity, can give him but a very ſhort and imperfect knowledge of many things, that are requiſite to frame a right judgment about the caſes which will firſt be brought before him; and, 'till he has heard the witneſſes, he may be very apt to fall into prejudiced opinions of things: but, when an authentic and ſufficient teſtimony has clear'd matters to him, he then pronounces, according to the light of reaſon he is maſter of; to which the witneſſes did but give information; tho' that ſubſequent information may have obliged him to lay aſide ſome prejudices he had entertain'd before he receiv'd it. And, what is ſaid of natural experience, with regard to the underſtanding, may, with due alteration, be apply'd to ſupernatural revelation: for here, alſo, the underſtanding is to examine, whether the teſtimony be indeed divine; and whether a divine teſtimony ought to be believ'd, in what it clearly teaches. I do not, therefore, degrade reaſon from the dignity that belongs to it, of perceiving, and judging; tho' it be obliged, by its own dictates, to take in all the aſſiſtance it can from experience, whether natural, or ſupernatural; and by the fuller accounts of things it receives from thoſe informations, to rectify, if need be, its former leſs mature judgment.

In ſhort, thoſe who cry up abſtract reaſon, as if it were ſelf-ſufficient, exalt it in words; but we who addreſs reaſon to phyſical and theological experience, and direct it how to conſult both, and take its informations from thence, exalt it in effect: and reaſon is much leſs uſefully ſerv'd by the former ſort of men, than by the latter; ſince, whilſt thoſe do but flatter it, theſe take the right way to improve it.

THE High Veneration MAN'S INTELLECT owes to GOD.

'TIS with indignation and wonder that I hear many men, and ſome of them divines too, who, little conſidering what God is, and what themſelves are, talk of him and his attributes as freely, and as unpremeditately, as they would of a geometrical figure, or a mechanical engine. And even the leſs preſumptuous frequently diſcourſe, as if the nature and perfections of that unparallel'd Being were objects their intellects could graſp; and ſcruple not to dogmatize about thoſe abſtruſe ſubjects as freely as about other things, that are confeſſedly within the reach of human reaſon.

Yet God may have ſeveral attributes and perfections utterly unknown to us; and of thoſe attributes whereof we have ſome knowledge, there are effects and properties, whoſe ſublimity or abſtruſeneſs ſurpaſſing our comprehenſion, makes the divine cauſe, or author of them, deſerve our higheſt wonder and veneration.

As there are two chief ways to arrive at the knowledge of God's attributes, the contemplation of his works, and the ſtudy of his word; it may be doubted whether either, or both of theſe, will ſuffice to acquaint us with all his perfections.

For tho' philoſophers have rationally deduced the power, wiſdom, and goodneſs of God, from thoſe marks of them which he hath ſtampt upon many of his viſible works; yet, ſince the divine attributes which the creatures point at, are thoſe whereof themſelves have ſome participation, or reſemblance; and ſince the fertility of the divine nature is ſuch, that its ex-

excellencies may be participated, or repreſented many ways; how can we be ſure, but ſo perfect, and exuberant a Being, has excellencies not expreſſed in the viſible world, or any of its known parts?

God may have ſeveral attributes and perfections unknown to us.

Some of thoſe divine attributes we know, being relative to the creatures, could ſcarce be diſcover'd by ſuch imperfect intellects as ours, but from conſidering ſome things actually done by God. Thus, ſuppoſing, before the foundations of the viſible world were laid, the angels had no more knowledge than men; they could ſcarce imagine in God a power of creating matter, and of producing local motion in it: much leſs could they have known how the rational ſoul, and human body act upon one another. Whence it ſeems probable, that if God hath made other ſyſtems than that wherein we live (and who can aſſure us, he hath not?) he may have diſplayed in ſome of the creatures that compoſe them, ſeveral attributes that we have not diſcover'd by the help of thoſe works of his wherewith we are acquainted.

I readily grant, that the revelations God hath vouchſafed us, have clearly taught us ſeveral things concerning their adorable author, which the mere light of nature either would not have ſhewn, or but very dimly diſcover'd. Yet the ſcripture itſelf informs us, that in this life "we know but in part, and ſee things but darkly;" and that we are ſo far from being able "to find out God to perfection," that even the ways of his providence are to us unſearchable. Hence God may have attributes unknown to us.

But it may be alledged, that, beſides the two ways mentioned of attaining the knowledge of God's attributes; there may be a third preferable to both the others, by conſidering the idea of a Being ſupremely, or infinitely perfect; in which idea, all poſſible perfections may be ſaid to be contained; ſo that no new one can be added to it. And, indeed, I readily grant, that this idea is the moſt genuine I am able to frame of the Deity; yet there may be ſeveral attributes, which, tho' in a general way contain'd in this idea, are not, in particular, diſcovered to us thereby. 'Tis true, that when, by whatever means, any divine perfection comes to our knowledge, we may well conclude, 'tis, in a ſenſe, comprized in the comprehenſive notion we have of a Being abſolutely perfect; tho', 'tis poſſible, that perfection would never have come to our knowledge by the bare contemplation of that general idea, but was ſuggeſted by particulars; whence ſuch diſcoveries are not ſo much derived from, as refer'd to the notion we are ſpeaking of.

Theſe conſiderations perſuade me, that God may have, not only various attributes, but various excellencies and perfections, unknown to us. And, farther, many of the attributes we know he hath, we have but an imperfect knowledge of, eſpecially if compared with his; for he poſſeſſes them in a manner peculiar to himſelf, and far tranſcending that wherein men poſſeſs ſome faint reſemblances of them.

The power and wiſdom of God are diſplay'd both in his corporeal, and his incorporeal creatures.

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Effects of the divine power.

Among the manifold effects of the divine power, I ſhall mention only two; which, tho' often very manifeſt, are not very attentively reflected on; the immenſe quantity of corporeal ſubſtance that the divine power provided for the framing of the univerſe; and the great force of the local motion imparted to it, and regulated in it.

The vaſt magnitude of the whole material world.

The vaſtneſs of that huge maſs of matter, whereof the corporeal world conſiſts, cannot but appear ſtupendous to thoſe who skilfully conſider it. That part of the univerſe, which has been already diſcover'd by human eyes, aſſiſted with dioptrical glaſſes, is almoſt inconceivably vaſt. The fix'd ſtars of the firſt magnitude, that to vulgar eyes look but like ſhining ſpangles, are by aſtronomers affirm'd to exceed, each of them, above a hundred times the whole globe of the earth in magnitude: and as little as theſe ſtars ſeem to our naked eyes, they appear much leſs thro' our teleſcopes; which taking off the falſe glittering that makes them look as they are painted, ſhew them little other than phyſical points of light. And the ſun, which is granted to be ſome millions of miles nearer to us than the other fix'd ſtars; tho' it ſeem at this leſs diſtance not to be half a foot broad; is by the generality of mathematicians allow'd to be eight or ten thouſand times as big as the terraqueous globe. And it plainly appears by the parallaxes and other proofs, that the globe of earth and water we inhabit, tho' it be divided into ſo many great empires, and kingdoms, and ſeas; and tho', according to the received opinion, it be 5400 *German* leagues in circumference, and conſequently its ſolid content 10,882,080,000 cubic miles; yet this globe is ſo far from being, for its bulk, a conſiderable part of the univerſe, that 'tis in compariſon thereof but a phyſical point. Nay, thoſe far greater globes, the ſun and fix'd ſtars, and all the ſolid maſſes of the world, if reduced into one, would, perhaps, bear a leſs proportion to the fluid part of the univerſe, than a nut to the ocean. And as an excellent modern aſtronomer obſerves, all the ſtars, crowded into one body, would, at a fit diſtance, appear no bigger than a ſtar of the firſt magnitude. After all, I have hitherto ſpoke but of that part of the corporeal univerſe already ſeen by us: I muſt therefore add, that as vaſt as this is, yet all that the eye, even when powerfully aſſiſted by teleſcopes, hath diſcovered, is far from repreſenting the world of ſo great an extent, as more perfect glaſſes will do hereafter. And even then the viſible part of the world will be far from reaching to the bounds of the univerſe*.

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* "The ſyſtem of the world is now" ſays Dr. *Halley* "underſtood to occupy the whole abyſs of ſpace, and conſequently to be actually infinite; and ſmaller fix'd ſtars being ſtill diſcoverable, as more perfect teleſcopes are employ'd, ſeems to confirm this doctrine. Were the whole ſyſtem finite, it would, tho' ever ſo extended, ſtill occupy no part of the *Infinitum* of ſpace, which neceſſarily and evidently exiſts; whence the whole would be ſurrounded with an infinite *Inane*; and the ſuperficial ſtars would gravitate towards thoſe near the centre, and with an accelerated motion run into them, and in proceſs of time unite into one. But if the whole be "infi-

The prodigious quantity of motion given thereto, and maintain'd therein.

From the vaſt extent of the univerſe, I proceed to conſider the ſtupendous quantity of motion that the divine power has given the parts of it, and continually maintains in it. Of this we may make an eſtimate, by conſidering with what velocity ſome of the greater bodies are moved, and how large a part of the remaining bodies of the univerſe is alſo endow'd with motion.

The leaſt velocity I ſhall mention, is that afforded by the *Copernican* hypotheſis, according to which, the earth revolves from weſt to eaſt, about its own axis, in four and twenty hours; yet this terraqueous globe, which we think ſo great that we commonly call it the world, moves at ſuch a rate, that, as the learned *Gaſſendus* computes, a point ſituated in the equator of it moves about twelve hundred feet in a ſecond minute: ſo that a bullet ſhot out of a canon ſcarce flies ſo ſwift.

But this velocity is ſcarce comparable to that of the fix'd ſtars, if we ſuppoſe them to move in four and twenty hours about the earth. For ſuppoſing the diſtance aſſign'd by *Tycho* between us and the firmament to be ſeven thouſand diameters of the earth; a fix'd ſtar in the equator moves, as *Mullerius* calculates, 3,153,333 miles in an hour, and conſequently 52,555 in a minute, and 875 in a ſecond; that is, three thouſand times faſter than a canon-bullet ſhot into the air. 'Tis true, that according to the *Ptolemaic* hypotheſis, a fix'd ſtar in the equator moves, at moſt, but three ſemidiameters of the earth in a ſecond; but, according to *Ricciolus*, this velocity is fifty times greater than in the *Ptolemaic* hypotheſis, and threeſcore and ten times greater than in the *Tychonian*: for, according to him, ſuch a fix'd ſtar as we ſpeak of moves 629,128 *Engliſh* miles in a ſecond.

But farther, that portion of the univerſe, which, tho' put into motion, is commonly ſuppoſed to be at reſt, is ſo great, that perhaps the quantity of motion diſtributed among ſeemingly quieſcent bodies, may exceed the quantity of motion which the firſt mover has communicated to the fix'd ſtars, tho' we ſuppoſe them whirl'd about the earth with that ſtupendous ſwiftneſs, the

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“ infinite, all the parts of it would be nearly *in æquilibrio*; whence each fix'd ſtar, being attracted by contrary powers, would keep its place, or move till from ſuch an equilibrium it comes to reſt. 'Tis no more abſurd, that the fix'd ſtars ſhould exceed any given number, than that duration ſhould be eternal, becauſe no number of days or years can complete it. 'Tis ſaid, I know, that if the number of the fix'd ſtars were more than finite, the whole ſurface of their apparent ſphere would be luminous. But if we ſuppoſe all the fix'd ſtars to be as far from one another, as the neareſt of them is from the ſun; that is, if we may ſuppoſe the ſun to be one of them; at a greater diſtance, their disks and light will be diminiſh'd, and the ſpace to contain them increaſed, in the proportion of the ſquares: ſo that in each ſpherical ſurface the number of ſtars it might contain, will be as the biquadrate of their diſtances. If then, as it needs muſt, the diſtances be immenſely great, it will follow, that as the light of the fix'd ſtars diminiſhes, the intervals between them decreaſe in a leſs proportion; the one being as the diſtances, the other as the ſquares thereof, reciprocally. And the more remote ſtars are ſo ſmall, that they muſt vaniſh, even in the moſt exquiſite teleſcopes; as a ſmall teleſcopic ſtar is inviſible to the naked eye.” *Philoſ. Tranſ.* N° 364. p. 22.

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ſyſtems of *Ptolemy* and *Tycho* ſuppoſe. For the fix'd ſtars, planets, or all the mundane globes, whether lucid or opake, together bear but a ſmall proportion to the interſtellar part of the univerſe. And tho' I ſhould allow all theſe globes to be ſolid, yet it muſt be confeſs'd that each of them ſwims in a fluid of much greater extent than itſelf. So that the fluid portion of the univerſe will, in bulk, almoſt incomparably exceed the ſolid. And if we conſider the nature of a fluid body, we ſhall find it conſiſt in having its minute parts perpetually and variouſly moved; ſo that tho' the whole body of a liquor ſeems to be at reſt, yet its component parts are in a perpetual motion, continually ſhifting places amongſt themſelves.

And becauſe the quantity of motion ſhared by the corpuſcles that compoſe fluids, is not uſually reflected on by philoſophers; I ſhall add, that we may gueſs how great and vehement a motion there may be in the parts of fluid bodies, perhaps, when the aggregates of thoſe particles appear to be at reſt, by obſerving them when their ordinary motions happen to be diſturb'd, or to be extraordinarily excited by fit conjunctures of circumſtances. Thus we ſee the ſtrange force and effects of boiſterous winds and whirl-winds, which are but ſtreams and whirl-pools of the inviſible air, whoſe ſingly inſenſible parts are by accidental cauſes determined to have their motion made either in a ſtrait line, or as it were about a common centre. Thus when a mine of gun-powder is ſprung, the flame, or ſome ſubtile ethereal ſubſtance, that is always at hand in the air, tho' both are fluids, and the powder perhaps be kindled but by one ſpark of fire, inſtantly exerts a moſt rapid and furious motion.

The velocity of theſe diſcharged flames may appear from that which gun-powder impreſſes on a bullet, ſhot out of a well-charg'd gun; and this *Merſennus*, after ſeveral trials made to meaſure it, defines to be about 450 feet in a ſecond. If then we admit the probable opinion of the *Carteſians*, that the earth and planets are turn'd about their own axes by the motion of the reſpective ethereal vortices in which they ſwim; we ſhall eaſily grant, that the motion of the celeſtial matter, which moves, for inſtance, upon the remote confines of the earth's vortex, is by a vaſt exceſs more rapid than that of the ſurface of the earth. But if we chuſe the *Tychonic* hypotheſis, which makes the firmament, with all the vaſt globes of light that adorn it, to move about their common centre in four and twenty hours, the motions of the celeſtial matter muſt be allow'd a far greater, and indeed a ſcarce imaginable rapidity.

Hence we may have more enlarg'd conceptions of the power and wiſdom of the great Creator, who has both put ſo wonderful a quantity of motion into the univerſal matter, maintains it therein, and is able not only to ſet bounds to the raging ſea, but ſo to curb and moderate thoſe ſtupendouſly rapid motions of the mundane globes and intercurrent fluids, that neither the unweildineſs of their bulk, nor the celerity of their motions, have made them fly out, for many ages. And if the firmament itſelf, whoſe motion, in the vulgar hypotheſis, is by much the moſt rapid in the world, fail of exactly completing its revolution in four and twenty hours, that retardation is ſo regulated, that ſince *Hipparchus*'s time, who lived above 2000 years ago, the firſt ſtar in *Aries*, which was then near the beginning of it, is not yet come to the laſt degree of that ſign.

The wiſdom of God differently expreſs'd.

The wiſdom of God is expreſs'd in two different manners. Sometimes it is ſo manifeſtly diſplay'd in familiar objects, that even ſuperficial ſpectators may take notice of it. But there are many other things, wherein the "treaſures of wiſdom and knowledge" may be ſaid to lie deep.

In the various contrivances of animal bodies.

The contrivance of every animal, and eſpecially of a human body, is ſo curious and exquiſite, that 'tis almoſt impoſſible for any one, who has not ſeen a diſſection well made, and anatomically conſider'd, to conceive how much excellent workmanſhip is diſplay'd in that admirable engine. Nay, the meaneſt living creatures of God's making are far more wiſely contrived, than the moſt excellent pieces of workmanſhip that human heads and hands can boaſt. No watch or clock is any way comparable, for exquiſiteneſs of mechaniſm, to the body of an aſs or a frog.

But God's wiſdom is recommended as well by the variety, and conſequently the number of the kinds of living creatures, as by the fabric of each of them in particular. The skill of human artiſts is, for the moſt part, limited to one, or to a few ſorts of contrivances; whilſt the great author of nature has not only created four principal ſorts of living engines, beaſts, birds, fiſhes, and reptiles, which differ exceedingly from one another, as the ſeveral regions or ſtages where they were to act their parts, required they ſhould; but under each of theſe comprehenſive genus's are compriz'd many ſubordinate ſpecies of animals, which differ exceedingly from others of the ſame kind, according to the exigency of their particular natures; as the hog and the hare, the parrot and the batt, the whale and the ſtar-fiſh, *&c.*

And what greatly enhances the excellent contrivances to be met with in theſe automata, is the ſymmetry of all the various parts whereof each of them conſiſts. For tho' an animal, conſider'd in his ſtate of intireneſs, is juſtly look'd upon as one engine; yet, really, this whole machine is a complex thing, made up of ſeveral parts, which, conſider'd ſeparately, may paſs, each of them, for a ſubordinate engine, excellently fitted for ſome particular uſe. It ought, therefore, highly to recommend the wiſdom of the great "former of all things," that he has ſo framed each particular part of animals, as not to let the skill beſtow'd on that render the other leſs perfect: which manifeſts, that this great artiſt had the whole fabric under his eye at once, and did at one view behold all that was beſt to be done, in order to compleat the whole animal, and all its parts, at once; whilſt many excellent artificers, who can make a ſingle engine very compleat, may not be able to make it a commodious part of an aggregate of engines. Thus, tho' ſeveral can make pendulum-clocks go very regularly on ſhore, they cannot yet fit them to meaſure time exactly in a ſhip at ſea.

But how much more wonderful than the ſtructure of the grown body, muſt be the contrivance of the *Semen animatum*; ſince all the future parts, the functions, and many actions of the animal to be produced, muſt be durably delineated, and couch'd in a little portion of matter, that ſeems homogeneous, and is unqueſtionably fluid? And what much increaſes the wonder is, that one of theſe latent impreſſions, or powers, namely, the prolific, is to lie dormant, perhaps, above thirty or forty years, and then to be able to produce many more ſuch engines as is the animal itſelf.

In the mutual uſefulneſs of his productions to each other.

Another way whereby God manifeſts his wiſdom in his corporeal creatures, is their mutual uſefulneſs to one another, in a relation either of dependence, or co-ordination. This ſerviceableneſs may be conſider'd, either as the parts of the animal have relation to one another, and to the whole body they make up; or as intire and diſtinct bodies, which have a regard to, or dependence on each other. To the firſt ſort of utility belong the offices of the parts of the body, which ſome of them exerciſe for the good of the whole; as the ſtomach for concocting the aliment, the brain for ſupplying animal ſpirits, the kidneys to ſeparate the ſuperfluous ſerum of the blood. There are alſo many other particular parts that have a great ſubſerviency to one another; and ſeveral conſents of parts, and utilities, that accrue from one to the other, are farther diſcovered by diſeaſes; which primarily affecting one part or member of the body, diſcover that another has a dependance on it, or a particular relation to it. To the ſecond kind of utility belong thoſe parts that diſcriminate the ſexes of animals, which have ſuch a relation one to another in the male, and the female, that 'tis obvious they were made in order to propagate the ſpecies.

It were endleſs to obſerve the wiſdom and goodneſs manifeſt in the works of creation; the ſituation of the ſun, its motion in the ecliptic, trade-winds, ſtated rains, *&c.* with regard to the welfare of men and other animals. Thus, too, we ſee, that, according to the uſual courſe of nature, lambs, kids, *&c.* are brought into the world at the ſpring of the year, when tender graſs, and other nutritive plants, are provided for their food. The like may be obſerved in the production of ſilk-worms, whoſe eggs are hatch'd when mulberry-trees begin to bud, whereon theſe inſects are to feed; the aliment being tender whilſt the worms themſelves are ſo, and growing more ſtrong and ſubſtantial as the inſects increaſe in vigour and bulk.

And in forming and governing other ſyſtems beſides the ſolar.

I have hitherto taken notice only of the productions of God's power and wiſdom obſerved in the viſible world; but, perhaps, as the ſun, the neareſt fixed ſtar to us, has a whole ſyſtem of planets that move about him, ſo ſome of the other fixed ſtars may be centres of other ſyſtems of celeſtial globes; ſince we ſee, that even ſome planets, determined by aſtronomers to be much inferior in bigneſs to the fixed ſtars, have other globes, that do, as it were, depend on them, and move about them; the earth has the moon for its attendant; *Saturn* is not unattended; and *Jupiter* has no leſs than four ſatellites. But none of theſe ſecondary planets, that move about *Saturn* and *Jupiter*, are viſible to the naked eye, and were, therefore, unknown to the aſtronomers who liv'd before the invention of teleſcopes. Now in caſe there are other mundane ſyſtems, beſides this viſible one of ours; I think it may be probably ſuppoſed, that

God has given peculiar, and admirable inſtances of his inexhauſted wiſdom in the contrivance and government of ſyſtems, that, for ought we know, may be framed and managed in a manner quite different from what is obſerved in that part of the univerſe known to us.

However, we need not fly to imaginary ultra-mundane ſpaces, to be convinced, that the effects of the power and wiſdom of God are worthy of their cauſes, and not adequately underſtood by us; if, with ſufficient attention, we conſider that innumerable multitude, and unſpeakable variety of bodies, that make up this vaſt univerſe. For there being among theſe, a ſtupendous number, that may be juſtly look'd upon as ſo many diſtinct engines, and many of them very complex; to know that all theſe, with the reſt of the mundane matter, are every moment ſuſtain'd, guided, and govern'd, according to their reſpective natures, and with an exact regard to the general laws of the univerſe; to know that there is a Being, who, every where, manages all things, without either aberration, or intermiſſion; is a thing, that if we attentively reflect on, ought to produce in us great wonder, and adoration.

The *Epicureans* of old did, with ſome colour of reaſon, urge againſt the belief of a divine providence, that 'tis incredible the gods ſhould be ſufficient for ſuch differing and diſtracting employments, as, according to the exigencies of nature, to make the ſun to ſhine in one place; the rain to ſhower down in another; the winds to blow in a third; the lightning to flaſh in a fourth; the thunder-bolts to fall in a fifth; and other bodies to act and ſuffer, according to their reſpective natures. We, therefore, who, upon good grounds, believe God really does what they thought impoſſible; are much wanting in our duty, if we do not admire an all-pervading wiſdom, that reaches to the utmoſt extent of the univerſe; and, with eaſe, actually performs what theſe philoſophers profeſs'd they could not ſo much as conceive.

Still greater inſtances of power and wiſdom, in the formation and government of immaterial Beings.

We have ſeen God's wiſdom and power in his corporeal works; but ſome of the divine perfections could not be ſo well expreſs'd, or copied, upon corporeal creatures, as upon the rational and immaterial ſoul of man, and other intellectual Beings: as the picture of a plain ſimple thing is not capable of receiving, or containing, ſo much of an excellent painter's ſkill, as he could exhibit in a piece wherein the paſſions of the mind, and the laws of optics, &c. may be fully expreſs'd. And it may well be preſumed, that if we were as familiarly acquainted with God's incorporeal creatures, as with his viſible ones; we ſhould perceive, that as ſpirits are incomparably more noble than bodies; ſo the divine wiſdom employ'd in the government and conduct of them, is more glorious than that which we juſtly admire in the conduct of his corporeal works. And, indeed, let a portion of matter be ever ſo fine, and ever ſo well contrived, it will not be more than an engine deſtitute of underſtanding and will; and whoſe excellency, as well as its diſtinction from other bodies, even the groſſeſt, and moſt imperfect, can conſiſt but in mechanical properties; which neither excite themſelves into motion, nor regulate and ſtop the motion

Physics. tion they once are in; whereas, true ſpirits, or immaterial ſubſtances, have, by God's appointment, belonging to their nature, underſtanding, will, and an internal principle, both of acting, and of arbitrarily ceaſing from action. And tho' God, as the ſole creator of all ſubſtances, has, and may exerciſe, an abſolute dominion over all his creatures, as well immaterial as corporeal; yet, ſince he thinks fit to govern ſpirits according to the nature he has given them; to create ſuch intelligent, free, and powerful Beings, as good and bad angels; and to govern them on ſuch terms, as effectually to make them inſtruments of his glory, which multitudes of them ſubtily and obſtinately oppoſe; requires a wiſdom and providence tranſcending any that can be diſplay'd in the formation and management of merely corporeal Beings. For inanimate engines may be contrived, to act as we pleaſe; whilſt angels, and human ſouls, are endow'd with a freedom of acting, in moſt caſes, as they pleaſe themſelves. 'Tis far eaſier for a watch-maker to regulate the motions of a watch, than the affections and actions of his ſon.

Angels, whether good or bad, are very intelligent and active Beings; and each of them is endow'd with an intellect capable of numberleſs notions, and degrees, or variations of knowledge; with a will capable of no leſs numerous acts; of having various influences upon the underſtanding, and of being variouſly affected by the dictates of it. So that each particular angel, being ſucceſſively capable of ſo many differing moral ſtates, may be look'd upon, as, in a manner, a diſtinct ſpecies of the intellectual kind. And, the government of one dæmon, may be as difficult a work, and, conſequently, may as much declare the wiſdom and power of God, as the government of a whole ſpecies of inanimate bodies, whoſe nature determines them to a ſtrict conformity to thoſe primordial laws of motion, once ſettled by the great creator; and, from which, they have no wills of their own to make them ſwerve.

The ſcripture tells us, that, in the œconomy of man's ſalvation, there is ſo much of the "manifold wiſdom of God" expreſs'd, that the angels themſelves deſire to pry into the myſteries of it. When our ſaviour, having told his apoſtles, that the day and hour of his future coming was not then known to any, ſubjoins, no not to the angels of heaven, he ſufficiently intimates them to be endow'd with excellent knowledge, ſuperior to that of men: which, perhaps, may be one of the reaſons why the ſcripture ſtyles them "angels of light." It alſo teaches us, that the good angels are vaſtly numerous; and that, as they are of differing orders, God aſſigns them very differing, and important employments, both in heaven, and on earth; and, ſometimes, ſuch as oblige them, in diſcharge of their reſpective truſts, to endeavour the carrying on of interfering deſigns. The ſame ſcripture, by ſpeaking of the devil, and his angels, and of the "great dragon, that drew down with his tail the third part of the ſtars from heaven to earth;" and, by mentioning a whole legion of devils that poſſeſſed a ſingle man, *&c.* gives us ground to conclude, that there is a political government in the kingdom of darkneſs; that the monarch of it is exceeding powerful, whence he is ſtiled the Prince of this world; and ſome of his officers have the titles of principalities, powers, rulers of the darkneſs of this world, *&c.* that the ſubjects of it are exceeding numerous; that they are deſperate enemies to God and men; that they are very falſe and crafty; and that their malice is active, reſtleſs, and great. Theſe things being taught us in the ſcripture itſelf, we may rationally ſuppoſe, that if we were quick-ſighted enough to diſcern the methods of the divine wiſdom in the government of the angelical, and of the diabolical worlds; we ſhould be raviſh'd into admiration how ſuch intelligent, free, powerful, and immortal agents, ſhould, without violence offer'd to their natures, be made, in various manners, to conſpire to fulfil the laws, or, at leaſt, accompliſh the ends of that great theocracy, which not only reaches to all kinds of bodies, but comprizes the whole creation, or the great aggregate of all the creatures of God. And, indeed, to make the voluntary, and perhaps, the moſt crafty actions of evil men, and of evil ſpirits, ſubſervient to his wiſe, and juſt ends, no leſs recommends the wiſdom of God, than it would the skill of a pilot, to contrive and ſteer a ſhip, ſo as to ſail to the deſigned port, not only with a ſide-wind, but with one that was quite contrary, and tempeſtuous.

At that great decretory Day, when the whole off-ſpring of *Adam* ſhall, by the loud voice and trumpet of the arch-angel, be call'd together from the remoteſt ages and moſt diſtant climates in the world; when the fall'n angels, and all the human actors that ever liv'd, ſhall appear upon the ſtage at once; " when the dead ſhall be raiſed, and the books ſhall be open'd:" then the wiſdom of God will ſhine in its meridian luſtre and full ſplendor, and not only the occurrences which relate to the lives and actions of particular perſons, or of private families, and other leſs ſocieties of men, will be there found not to have been overlook'd by the divine providence; but the fates of kingdoms and commonwealths, and the revolutions of nations and of empires, will appear to have been order'd and over-ruled by an incomparable wiſdom. And thoſe great politicians, who thought to out-wit providence, by their refined ſubtilties, ſhall find themſelves " taken in their own craftineſs; " ſhall have their deepeſt " counſels turn'd into fooliſhneſs;" and not be able to keep the amazed world from diſcovering, that whilſt they thought they moſt craftily purſu'd their own ends, they really accompliſh'd thoſe of God. And the ſubtile hypocrites, who thought to make pretended religion the inſtrument of their ſecular deſigns, ſhall find thoſe deſigns defeated, and made truly ſubſervient to that advancement of religion, which they, in reality, never aim'd at.

To employ, and keep in order, a very complicated engine, tho' all the parts of it be inanimate, and deſtitute of purpoſes and ends of their own, is juſtly counted a piece of skill. And this task is more difficult, and, conſequently, recommends the conduct of the artiſt, in proportion to the intricacy of the ſtructure, and the number of pieces whereof the engine conſiſts. How aſtoniſhing, then, will appear that wiſdom and providence, which is able to guide, and over-rule many thouſand millions of engines, endow'd with wills, ſo as to make them all be found, in the final iſſue of things, ſubſervient to purpoſes worthy of divine providence, holineſs, juſtice, goodneſs?

In ſhort, when all the actors ſhall appear at once upon the ſtage; when all diſguiſes ſhall be ſtript off, all intrigues diſcovered, all hearts and deſigns laid open; then to find, that this whole amazing opera, that has been acting upon the face of the earth, from the beginning to the end of time, has been ſo contrived, and carried on, by the great author of the world, and of men, that their innumerably various actions, and croſs-deſigns, are brought to conſpire to the accompliſhment of a plot worthy of God; will appear an effect of ſo vaſt, and ſo all-pervading a wiſdom, that human intellects will with admiration confeſs, nothing but a divine omniſcience could compaſs.

Great inſtances of wiſdom in the redemption of man.

In the redemption of mankind, more of the divine attributes, than are commonly taken notice of, have their diſtinct Agencies; and their co-operation is ſo admirably directed by the divine wiſdom, that an apoſtle might very juſtly call it the "great myſtery of Godlineſs."

But many divines have largely treated of this ſubject; tho' I doubt whether moſt of them have not been more happy in avoiding errors about it, than ſucceſsful in unveiling the myſteries couch'd in it. There are, in the great work of man's redemption, ſome characters and footſteps of the divine wiſdom ſo conſpicuous and refulgent, that a believer, of mean parts, may eaſily diſcern them. But there are alſo, in this ſublime and comprehenſive work, ſome "depths of God," and ſo much of "the wiſdom of God in a myſtery, that I cannot think it eaſy to have a mental eye, ſo enlighten'd, and ſo piercing, as to treat largely and worthily of ſo vaſt and abſtruſe a ſubject. And, indeed, a man muſt know much of the nature of ſpirits in general, and even of the father of them, God himſelf; of the intellect, will, *&c.* of the ſoul of man; of the ſtate of *Adam* in paradiſe, and of the influence of his fall upon his poſterity; of the natural, or arbitrary vindictive juſtice of God; of the grounds and ends of God's inflicting puniſhments; of the admirable and unparallel'd perſon of Chriſt; of thoſe qualifications, and offices, that are required to fit him for being a redeemer; of the nature of covenants, and the conditions of thoſe which God vouchſafed to make with man; of the divine decrees, with regard to man's final State; of the ſecret and powerful operations of grace upon the mind, and the manner by which the ſpirit of God works upon the ſouls of men, which he converts, and brings, by ſanctification to glory; in ſhort, there are ſo many points, moſt of them of difficult ſpeculation, that are fit to be diſcuſs'd by him, who would ſolidly and fully treat of the world's redemption by Chriſt; that, when I reflect on them, I am ready to exclaim with St. *Paul*, "who is ſufficient for theſe things?" And, I am ſo far from wondering, that the generality of divines, and other writers on this ſubject, have not fully diſplayed the wiſdom which God has expreſs'd in this great work, that to have been able to accompliſh it in ſo admirable a way, as God has actually contrived, and made choice of; is one of the chief reaſons of my admiration of the wiſdom itſelf. And, I am perſuaded, that for God to reconcile his inflexible juſtice, his exuberant mercy, and all thoſe other attributes that ſeem'd to claſh, inevitably, about the deſign'd ſalvation of men, and make them co-operate to it; is a ſtupendous manifeſtation of wiſdom: there being no propoſition in *Diophantus*, or *Apollonius*, in algebra, or in geometry, near ſo difficult to be ſolv'd, or that requires a greater number of proportions and congruities, to be at once attended to, and made ſubſervient to the ſame ends; as that great problem, propounded by God's infinite goodneſs, to his divine wiſdom; the redemption of loſt and perverſe mankind, upon the terms declared in the goſpel: which are admirably fitted to promote, at once, God's glory, and man's felicity.

The immenſe difference between the creator and his creatures.

We have here, and that very imperfectly, only ſpoke to two of God's attributes, his wiſdom, and his power: tho' there are many others wherewith we are acquainted; and, perhaps, ſtill more whereof we are ignorant. Now, the natural and genuine reſult of all theſe divine perfections, muſt be a moſt glorious majeſty, that requires the moſt lowly and proſtrate veneration of all his intelligent works. And, accordingly, the angels, of all his mere creatures the moſt excellent and knowing, are repreſented in the ſcripture, as aſſiduouſly employing themſelves in obeying and ſerving, in praiſing and adoring the divine majeſty, with the utmoſt lowlineſs and ſubmiſſion.

This profound reſpect of the angels, is not to be wonder'd at; ſince, where eſteem ſprings not from ignorance, but knowledge; the greater the ability and opportunities are of having the knowledge clear and heighten'd, the greater veneration muſt be produced in an intelligent Being for the object admired; whoſe perfections are here ſuch as even an angelical intellect cannot fully reach: for, as a line, by being ever ſo much extended in length, cannot grow a ſurface; ſo neither can created perfections be, by any ideas, magnify'd into divine ones. And, indeed, ſpeaking in general, the creatures are but ſhadowy, and arbitrary pictures of the great creator; of many of whoſe perfections, tho' they have ſome marks; yet they are ſuch, as rather give the intellect riſe and occaſion to take notice of, and contemplate the divine originals, than afford it true images of them. The awful reverence paid to the ſupreme Being by thoſe excellent ſpirits, who "are greater in power and might than we," ought to admoniſh us of the ecſtatic reſpect we owe him; and teach us, that whenever we ſpeak either to God, or of him, we ought to be inwardly affected with the unmeaſurable diſtance there is between a moſt perfect and omnipotent creator, and a mere impotent creature.

The ſuperiority of the divine knowledge to that of man.

The diſtance betwixt the infinite creator and the creatures, which are but the limited and arbitrary productions of his power and will, is ſo vaſt, that all the divine attributes, or perfections, by unmeaſurable intervals tranſcend thoſe faint reſemblances of them, that he has been pleaſed to impreſs, either upon other creatures, or upon men. God's nature is ſo peculiar and excellent, that there are qualities, which, tho' high virtues in men, cannot belong to God, or be aſcribed to him without derogation. Nay, there are ſome virtues that belong to man himſelf only in his mortal ſtate.

But, whatever excellencies there be, that are ſimply and abſolutely ſuch, and ſo may, without diſparagement to his matchleſs nature, be aſcribed to God, we may be ſure that he poſſeſſes them; ſince he is the original author of all the degrees, or reſemblances, we men have of any of them. The pſalmiſt's reaſoning is good. "He that planted the ear, ſhall he not "hear? He that formed the eye, ſhall not he ſee? He that teacheth "man knowledge, ſhall not he know?" Since all the perfections communicated to, or to be found in the creatures, being emanations of the divine excellencies, belong as much to God, as, in a bright day, all the luminous rays found in the air, belong to the ſun. The vaſt difference, then, between the perfections of the great creator, and thoſe that are analogous to them in the creatures, reaches to all the perfections to be found in both: but the human underſtanding, as it values itſelf upon nothing more than wiſdom and knowledge; ſo there is nothing that it eſteems, and reverences, more in other Beings, and is leſs willing to acknowledge itſelf ſurpaſs'd in, than theſe. Now, 'tis certain, that God knows innumerable things, with which we are altogether unacquainted: he cannot but know all the creatures he has made, whether viſible or inviſible, corporeal or immaterial; and what he has enabled them to do. Nay, ſince he cannot but know the extent of his own infinite power, he cannot but know numberleſs things, as poſſible, that he has not yet made, or, perhaps, ever will make. He, alſo, knows thoſe things whereof we men have ſome knowledge, in a manner, or degree, peculiar to himſelf. As, what we know but in part, he knows fully; what we know but dimly, he knows clearly; and what we know but by fallible mediums, he knows moſt certainly.

But the great prerogative of God's knowledge, is, that he perfectly knows himſelf; that knowledge being not only too wonderful for man, but beyond the reach of an angelical intellect: ſince, fully to comprehend the infinite nature of God, no leſs than an infinite underſtanding is requiſite. And for the works of God, even in thoſe that are purely corporeal, our knowledge is incomparably inferior to his. For tho' ſome modern philoſophers have made ingenious attempts to explain the nature of things corporeal; yet their explanations generally ſuppoſe the preſent fabric of the world, and the laws of motion ſettled in it. But God knows, particularly, both why, and how the univerſal matter was firſt contrived into this admirable univerſe, rather than a world of any other of the numberleſs conſtructions he could have given it; and both why thoſe laws of motion, rather than others, were eſtabliſhed; and how ſenſeleſs matter, to whoſe nature motion does not at all belong, comes to be put into motion, and qualify'd to transfer it, according to determinate rules, which itſelf cannot underſtand. But when we come to conſider the particular, and more elaborate works of nature; ſuch as the ſeeds or eggs of living Creatures, &c. the ingenuous confeſs, and the confident betray their ignorance. 'Tis likely, that we men know ourſelves better than what is without us: yet how ignorant we are at home, if the endleſs diſputes of *Ariſtotle*, and his commentators, about the human ſoul, and of phyſicians and anatomiſts, about the mechaniſm of the human body, were not ſufficient to manifeſt, 'twere eaſy to ſhew, by the very conditions of the union of the ſoul and body: which, being ſettled, at firſt, by God's arbitrary inſtitution; and having nothing in all nature parallel to them; the manner and terms of that ſtrange union, is a riddle to philoſophers; but muſt needs be clearly known to him who alone inſtituted it, and preſerves it. There are ſeveral advantages of the divine knowledge, above that of man. For, we can perceive, and ſufficiently attend but to few things at once; but God's knowledge reaches, at once, to all that he can know; his penetrating eyes pierce quite thorough the whole creation at one glance; and "there is no creature that is not manifeſt in his ſight." He always ſees incomparably more objects at one view, than the ſun himſelf, endued with ſight, could do. For God beholds, at once, all that every one of his creatures, in the vaſt univerſe, either does or thinks. The knowledge of God is, alſo, not a progreſſive, or diſcurſive thing, like that acquir'd by our reaſoning; but an intuitive knowledge. Men, by reaſon of the limitedneſs and imperfections of their underſtandings, are obliged to make the notice they have of one thing, a ſtep and help to acquire that of another, leſs known: but God, whoſe knowledge, as well as his other attributes, is infinitely perfect, knows every thing in itſelf; and, all things being equally known to him, he can, by looking into himſelf, there ſee every thing that is knowable, moſt diſtinctly, yet all at once. But, further, God knows the moſt ſecret thoughts and intentions of men; whence he is call'd the "ſearcher of all hearts;" nay, he knows mens "thoughts afar off." And, by the way, how imperfectly muſt mere philoſophers know God, ſince they know him but by his works; and know his works themſelves but very imperfectly? Another conſpicuous prerogative of the divine knowledge, is the preſcience of future contingencies, that depend upon the determinations and actions of free agents. For we men are ſo far from being able to ſtretch our knowledge to the diſcovery of ſuch events, that the greateſt ſcholars in vain have try'd to diſcover how God himſelf can foreknow them; and, therefore, too many, even among chriſtians, deny that he can; tho' by ſeveral accompliſh'd predictions, recorded in ſcripture, it manifeſtly appears, that he does.

The obligations men are under to venerate, and contemplate God.

When I conſider the tranſcendent excellency, and the numerous prerogatives of the Deity, I cannot without wonder and concern obſerve, that rational men, profeſſing chriſtianity, ſhould wilfully neglect to acquire, or reflect on, thoſe notices that are apt to increaſe their knowledge of God, and conſequently their veneration for him. To aſpire to a farther knowledge of God, that we may the better adore him, is a great part both of man's duty and his happineſs. God, who has put into men an innate deſire of knowledge, and a faculty to diſtinguiſh the degrees of excellency in different notices; and to reliſh thoſe moſt, that beſt deſerve it; and has made it his duty to ſearch and inquire after God, and to love him above all things; would not have done this, if he had not known that thoſe who make a right uſe of their faculties, muſt find him to be the nobleſt object of the underſtanding; and that which moſt merits their wonder and veneration. And, indeed, what can be more ſuitable to a rational creature, than to employ reaſon to contemplate that divine Being, who is both the author of its reaſon, and the nobleſt object, about which it can poſſibly be employ'd? The knowledge of ſome dead language, or ſome old ruſty medal, or the opinions and cuſtoms of ſome nations or ſects, that did not, perhaps, reaſon or live any better than we do now, are thought worthy of curioſity, and even of the laborious induſtry of learned men; and the ſtudy of things merely corporeal gains men the honourable title of philoſophers. But whatever theſe objects of inquiry be, in themſelves, 'tis certain the greateſt diſcoveries we can make of them are but trifles, in compariſon of the excellency of the knowledge of God, which as much ſurpaſſes that of his works, as he himſelf does them. And 'tis the prerogative of his nature, to be infinitely above all that he has made, whether we contemplate the works of nature, or thoſe of art; the former whereof are under another name, his more immediate works; and the others, the effects of one of his works; and, by conſequence, originally his. And tho' it be moſt true, that God has been pleaſed to ſtamp on the corporeal world ſuch impreſſes of his power, wiſdom, and goodneſs, as have juſtly exacted the admiration even of philoſophers; yet the great author of the world is himſelf incomparably ſuperior to all his workmanſhip: ſo that tho' he could have made, and always will be able to make, creatures more perfect than thoſe he has made, by infinite degrees; yet the prerogative of his nature will keep him neceſſarily ſuperior to the moſt excellent creatures he can make; ſince the very condition of a creature hinders it from being ſelf-exiſtent and independent. 'Tis therefore, methinks, a ſad thing, that we men ſhould grudge to ſpend now and then a few hours in the contemplation and internal worſhip of that moſt glorious and perfect Being, who continually employs the devotion of angels themſelves.

I know 'tis ſuppoſed a dangerous thing to be inquiſitive about the nature of God; but the ſecret things of God, which are to be left to himſelf, ſeem to be his unrevealed purpoſes and decrees, and his moſt abſtruſe eſſence or ſubſtance; the ſcrutiny whereof, I readily acknowledge not to belong to us. And I think there is a great difference between contemplating God out of a bold curioſity, merely to know ſomewhat that is not common of him; and doing it out of an humble deſire, by a farther knowledge, to heighten our reverence and devotion towards him. 'Tis an effect of arrogance to endeavour, or ſo much as hope, to comprehend the divine perfections, ſo as to leave nothing in them unknown, but to aſpire to know them farther, that they may proportionably appear more admirable and lovely in our eyes, is not only an excuſable, but a laudable curioſity. The ſcripture in one place exhorts us "to grow" not only "in grace," but "in "the knowledge of Chriſt;" and in another, "to add to our virtue know-"ledge." And that we may aſpire to great degrees of knowledge, even as to thoſe ſupernatural objects we cannot adequately know, appears from St. *Paul*, who prays that his *Epheſians*, and all true chriſtians, may be able to comprehend what is "the breadth, and length, and depth, and height, "and to know the love of Chriſt, which, ſays he, paſſeth knowledge." Suppoſing it then lawful to contemplate God, not with deſign to pry into his decrees and purpoſes, nor to dogmatize in controverted points about his nature and attributes; but to excite in ourſelves the ſentiments which his indiſputable perfections are, by a more attentive view, qualified to produce; I take the devout contemplation of God, beſides other great advantages that it brings the mind, to be one of the moſt delightful exerciſes the ſoul is capable of, on this ſide heaven. 'Tis generally acknowledg'd, that admiration is one of the moſt pleaſing affections of the mind; which ſometimes, when the object deſerves it, is ſo poſſeſs'd thereby, as to forget all other things, or leave them unregarded. Now the pleaſure that admiration gives, being uſually proportionate to the uncommon nature and endearing circumſtances of the thing admired; how can any admiration afford ſuch a contentment, as that which has God himſelf for its object? The wonder produced in us by an humble and attentive contemplation of God, has two principal advantages, above the admiration we have for any of his works, or of our own. For, firſt, when we admire corporeal things, how noble and precious ſoever, the contentment that accompanies our wonder is allay'd by a kind of ſecret reproach, grounded on that very wonder; ſince it argues a great imperfection in our underſtandings, to be unacquainted with things that are but creatures, as well as we; and, what is worſe, of a nature much inferior to ours: whilſt 'tis no diſparagement, for a human intellect to be poſſeſs'd with wonder, tho' heighten'd to amazement, or aſtoniſhment, by the contemplation of that moſt glorious and infinitely perfect Being, who muſt neceſſarily exceed the adequate comprehenſion of any created intellect. But there is a farther and much greater advantage of the admiration of God, above that of other things; for other objects having a bounded nature, and commonly but one thing to deſerve our wonder, our admiration of them is ſeldom laſting; and after a little familiarity, firſt languiſhes, and then ceaſes: but God is an object ſo very ſingular, whoſe perfections are ſo immenſe, that no diligence of conſidering him, can make him ceaſe to be admirable; and the more we know of him, the more reaſon we find to admire him. So that there may here be a perpetual viciſſitude of our happy acquirements of farther degrees of knowledge, and our eager deſires of new ones. God is ſo fertile an object, that we need not fear our admiration of him ſhould expire, for want of variety to keep it up. To the wonderful excellence of God, may be juſtly applied what *Ariſtotle* lays down as a definition of *Infinite*; *viz.* "'tis that, of which "how much ſoever one takes, there ſtill remains more to be taken." If the intellect ſhould for ever make a farther progreſs in the knowledge of the wonders of the divine nature, attributes, and diſpenſations; yet it may ſtill make diſcoveries of freſh things worthy to be admired: as in an infinite ſeries, or row of aſcending numbers, tho' you may ſtill advance higher and higher, yet all that you can do by that progreſs, is to go farther and farther from the firſt term of the progreſſion, without ever reaching, or ſo much as approaching to an infinite number. Nu-

Numerous other arguments might be brought, to ſhew the immenſe inferiority of man's intellect to God.

I think, then, that it becomes us to uſe an awful circumſpection, not only when we make philoſophical inquiries about God, that is, when we preſume to diſcourſe of him, but when we ſolemnly deſign to praiſe him; for 'tis one thing to ſay true things, and another to ſay things worthy of God. Our ideas of him may be the beſt we are able to frame, and yet may far better expreſs the greatneſs of our veneration for him, than the immenſity of his perfection; and even thoſe notions that may be worthy of the moſt intelligent of men, will fall extremely ſhort of being worthy of the incomprehenſible God. The brighteſt and leaſt unlike idea we can frame of God, is infinitely more inferior, with regard to him, than a parhelion is with regard to the ſun. He has not, in my opinion, the trueſt veneration for God, who can ſet out his excellencies and prerogatives in the moſt high and pompous expreſſions; but he who willingly has a deep and real ſenſe of the unmeaſurable inferiority of himſelf, and his beſt ideas, to the unbounded and unparallel'd perfections of his maker. And as even our hymns and praiſes of the ſupreme Being deſerve our bluſhes, and need his pardon; what confuſion will one day cover the faces of thoſe who not only ſpeak ſlightly and careleſly, but often contemptuouſly of that ſupreme and infinitely perfect Being, to whom they owe thoſe very faculties which they ſo ungratefully and impiouſly miſemploy? Indeed ſuch tranſcendent excellencies as are the divine, might juſtly diſcourage us from offering ſo much as to celebrate them, if infinite goodneſs were not one of them. I ſhall not therefore allow myſelf the preſumption of pretending to make a panegyric of God, but content myſelf with an humble adoration of thoſe perfections whereof my utmoſt praiſes would rather expreſs my own weakneſs, than their excellence; ſince of this ineffable object the higheſt things that can be expreſſed in words muſt fall ſhort; for words cannot expreſs him.

STATICS.

THE
PREFACE.

STATICS is that part of univerſal mechanics, which conſiders the gravity of bodies, in all ſorts of mediums; tho' 'tis, ſometimes, taken for the ſame as particular mechanics, or the doctrine of motion in bodies, which depends upon their gravity. But, hydroſtaticks, in general, regards the weight of bodies in all manner of fluids; and, therefore, differs but little from ſtatics, unleſs when bodies are weighed in vacuo; *or, where there is no ſenſible reſiſtance. There is, alſo, a more limited ſignification of the word, hydroſtatics; which reſtrains it, as its derivation implies, to the weighing of bodies in water; and 'tis hydroſtatics in this ſenſe, wherewith we are here principally concern'd: a doctrine which, before Mr.* Boyle *undertook to improve it, conſiſted chiefly in theory and ſpeculation; having only been treated by meer mathematicians: but, that noble philoſopher ſoon reduc'd it to practice, and applied it to uſeful purpoſes. Some of its propoſitions, formerly demonſtrated in a mathematical manner, he proved by the direct, obvious way of experiment; that is, by the proper way, or hydroſtatically. He, alſo, made many new diſcoveries in this part of knowledge, which are highly uſeful in life, and teach us to examine the goodneſs of drugs, of metals, minerals, and other bodies, both ſolid and fluid.*

The firſt of the following pieces appears under the title of paradoxes; and, truly, the propoſitions it contains, were paradoxes, even to ſome mathematicians, at the time wherein they were publiſhed. Nor is this ſtrange, ſince, as the learned Wolfius *obſerves, many perſons taking gravity for a permanent power in matter, which muſt remain unalter'd, ſo long as it continues its ſtate; and ſuppoſing fluids, whilſt confin'd, and at reſt, wholly unable to act upon bodies; no reaſon appears to them, why they ſhould, as it were, take away a part of the gravity of the bodies immerſed in them; and, much leſs, why they ſhould, ſometimes, throw them upwards with a great force. This was remarkably the* *caſe with that great Scholar, Dr.* More; *who, to ſolve ſuch an appearance, would fain have introduced a new, and an immaterial principle into phyſics. And this gave birth to another of the enſuing pieces; which tho' it alſo relates to ſome experiments, that appear under the head of pneumatics, we thought fit to range entire, under this of ſtatics; becauſe, in ſtrictneſs, 'tis wholly hydroſtatical; taking that term in its larger ſenſe; becauſe we would not break in upon the order of the pneumatical pieces; and, laſtly, becauſe hydroſtatics ought, always, to precede pneumatics; ſince the latter cannot be underſtood without a knowledge of the former.*

Hydroſtatical Paradoxes,

Proved and Illuſtrated by

EXPERIMENTS.

POSTULATA, & LEMMATA.

1. SUppoſe a tube, open at both ends, held, with one of them, perpendicularly, under water; the lower orifice may be conceived to terminate in a plain parallel to the horizon, or the upper ſurface of the fluid.

2. All aſſignable equal portions of this ſurface, will be equally preſſed by the water perpendicularly incumbent thereon.

For the fluid being here ſuppoſed homogeneous, as to its gravity, and to ſtand, at the ſame height, upon all the parts of the imaginary plain; no equal part can poſſibly be more preſſed than another, in the ſame ſurface.

3. If any part of this imaginary plain be preſſed with a greater weight than another, the former will be either diſplaced, or depreſſed.

Thus, whilſt an heavy body ſinks in water, that part of the imaginary plain, contiguous to the lower part of the body, being preſſed by a greater weight than the other portion of the ſame ſurface, muſt needs give way, ſucceſſively, till the heavy body arrives at the bottom.

4. On the other hand, if any part of the imaginary ſurface be leſs preſſed upon than the remainder, it will, by the weight on the remainder, be impell'd upwards, till the preſſure there be equal to that upon the other parts of the ſame ſurface.

If this be doubted, the following experiment will prove it. If a cylindrical glaſs tube, open at both ends, be ſteadily held in a perpendicular poſture, with one of them immerſed, two or three inches below the ſurface of a proper quantity of water, in a glaſs veſſel; the ſurface of the water within-ſide the tube, will be nearly level with the ſurface that is without-ſide the ſame; becauſe the water, in both caſes, has a free communication.

If, now, a convenient quantity of oil, be gently pour'd upon the water that is external to the tube, that within the tube, which we ſhall call internal, will gradually riſe, and continue to do ſo proportionably; becauſe the imaginary plain, cutting the immerſed orifice of the tube, is every where preſſed by an additional weight, except in the orifice itſelf: which preſſure muſt neceſſarily be increas'd, as more oil is pour'd upon the external water; whilſt a circle of our imaginary plain, equal to the lower annulus of the tube, is, by the ſides thereof, guarded from the immediate weight of the oil; whence the external water, being more preſſed than the internal, is, conſequently, forc'd up thro' the tube, where there is the leaſt reſiſtance, till the cylinder of water within the tube, gravitates upon the ſubjacent part of the imaginary plain, equally with the ſum of the water and oil, upon every other equal portion of the ſame ſurface.

5. The air is a ponderous body.

This has been ſhewn by ſeveral experiments; ſome whereof, indeed, are excepted to; but the following is unexceptionable, and was often repeated.

Having obtain'd a thin, and large glaſs bubble, with a ſlender ſtem, and gradually rarified the air it contain'd, as much as I conveniently could, by the flame of the lamp, whereat the bubble was blown; the ſtem, whilſt the bubble remain'd exceeding hot, being nimbly put into the flame, thereby became hermetically ſealed, in an inſtant. This glaſs we permitted to cool leiſurely, and afterwards weigh'd it in a very exact balance; then carefully breaking off the ſealed end, and preſerving the fragments, we weigh'd the glaſs again, and found it conſiderably heavier than before; which could be owing to nothing but the natural air, that ruſhed into the bubble, upon breaking off its ſtem, whilſt it contain'd air greatly expanded. For, the noiſe hereof, in entring, may be plainly heard by an attentive ear; and as the rarifaction is here great, ſo the bubbles will ſometimes break, by the bare force of the external air; and, laſtly, if the ſealed end of the glaſs be broke off, under water, that fluid will, by the preſſure of the atmoſphere, be forced into the bubble, like an artificial fountain, till it be about three quarters full: whence the weight of the air appears to be very conſiderable; ſince the fourth part of what the bubble would contain, remains therein not taken notice of by the balance. In one repetition of this experiment, I found the air freſh admitted into the bubble, weighed near $\frac{3}{4}$ of a grain, and its full content of water 906 grains; whence, if the re admitted air only filled $\frac{3}{4}$ of the bubble, the whole air it contain'd, may be reaſonably ſuppoſed at a grain. And thus the water weigh'd about 900 times as much as the air of the bubble; which, all things conſider'd, comes near enough to ſome other trials, whereby we determin'd the ſpecific gravity of air, to be to that of water, as 1 to 1000.

PARADOX I.

In all fluids, the upper parts gravitate on the lower.

Suppoſe one end of a ſmall, cylindrical, open glaſs tube, plunged into oil of turpentine, and that liquor to be raiſed, by ſuction, to a convenient height therein; when the lips being removed, and the upper orifice of the tube nimbly ſtopp'd with the finger, to prevent the fluid from falling back, imagine it thus placed perpendicularly in the glaſs A B C D almoſt fill'd *Fig.* 2. with water, ſo that the ſurface of the oil may ſtand ſomewhat higher than that of the water. This done, and the finger removed from the upper orifice of the tube, the oil will not fall out at the lower, but remain ſuſpended near its former altitude. But oil of turpentine, being an heavy fluid, has a tendency downwards; and as the lower orifice of the tube is open, it muſt neceſſarily fall out thereat, did not the preſſure of the water underneath ſuſtain it. No contrariety in the nature of theſe two liquors can be ſuppoſed the cauſe of this phenomenon; for, if the finger be removed, before the pipe is ſufficiently immerſed, the oil will ſubſide till it becomes a balance to the water; but the reaſon of it is manifeſtly this: Suppoſe the plain wherein the extremity Q, of the pipe P Q, reſts, to be G H; if that part thereof whereon the oil reſts at Q, be equally preſſed by the incumbent cylinder of oil Q X, with the other parts of the ſame ſurface G H, by the water incumbent upon them; the part Q can neither be depreſſed by the cylinder of oil Q X, nor raiſed by an equal preſſure of water upon the other parts of the plain G H. That this equilibrium, betwixt the oil and water, is juſtly aſſign'd for the cauſe of the phenomenon, appears farther, 1. From what is already obſerved, *viz.* that if the cylinder of oil reach much higher than the ſurface of the water, the oil will deſcend: the reaſon whereof is, that the plain G H, being more charged at Q than in any other part, it is there unable to make reſiſtance; whence it muſt neceſſarily be thruſt out of its place, by the deſcending oil. 2. Becauſe the oil will continue ſubſiding only till its ſurface becomes almoſt level with that of the water; when the part Q is preſſed by the oil, equally with the other parts of the plain G H, by the water incumbent on them. 3. If, while the oil and water are balanced, the pipe be gently raiſed from Q to S, the oil will preponderate, and conſequently fall out in drops, which by the ſuperior ſpecific gravity of the water, will be buoy'd up, and ſo float on the ſurface thereof; and as the pipe is gradually elevated towards the ſurface L M, more and more of the oil will drop out: but if the tube be ſtop'd any where in its aſcent, at S, for inſtance, the oil will ceaſe to flow out: and, as at the plain I K the preſſure of the water, by reaſon of its diminiſh'd depth L I, upon the other parts of the ſurface, is not near ſo great, as upon the plain G H; ſo the remaining cylinder of oil incumbent on S, is, by reaſon of its proportionable efflux, unable to preſs that part more forcibly than the others of the ſame plain I K, are preſs'd by the incumbent water. And, if the lower orifice be raiſed almoſt to V, or near the upper ſurface of the water L M, ſcarce any oil, for the ſame reaſon, will remain in the pipe T V. 4. But if, when the end of the tube reſts in the plain G H, where the oil is a balance for the water, it be let down to O, the external water will cauſe the oil to aſcend in the tube, the height of E G; ſo that the pipe will contain, beſides a cylinder of oil α W, a ſhorter one of water α O; for now a new plain E F, cuts the lower orifice of the tube, whilſt that part of it at O, being, by the incumbent oil, preſſed leſs than the others by the incumbent water, the oil will be buoy'd up, till the water O α, and the cylinder of oil α W, taken together, gravitate as much upon O, as the reſt of the incumbent water does upon the other equal parts of the ſame plain E F. Since then the oil, in this caſe, may be kept ſuſpended at any point, under water, as at Q, being there in an equilibrium with the external fluid; ſince, when elevated, as from Q to S, it falls by its own gravity; and, ſince the deeper it is plunged, the greater weight and preſſure are required in the cylinder of oil, to balance the preſſure of the water; it follows, that the parts of the water incumbent on the plain G H, preſs that ſurface more than the plain I K is preſſed by the parts of the water contiguous to it; and conſequently, that all the parts of the water which lie under the upper ſurface, are preſſed by thoſe which ſtand directly over them; as the upper parts of the oil preſs'd down the lower, whilſt the pipe was drawn up from Q to S; ſo that the oil flow'd out at the orifice S: for ſince the lower parts of a liquor preſs proportionably to the height of the fluid, they muſt neceſſarily derive their force from the incumbent parts, which conſequently gravitate upon them.

A glaſs bubble, about the bigneſs of a pullet's egg, was purpoſely blown at the flame of a lamp, with a long ſtem turn'd up at the end, that it might be the more conveniently broken off. This bubble being well heated, to rarify the air, and thereby drive out a large part of it, was preſently ſeal'd at the end, and, by the help of the figure of the ſtem, was, by a convenient piece of lead, ſunk under water, the weight and glaſs being tied by a ſtring to one ſcale of a good balance, in the other whereof was placed a counterpoiſe to the bubble, as it hung freely in the midſt of the water. Then, with a pair of forceps, I carefully broke off the ſealed end of the bubble under water, ſo that no air appear'd to emerge, or eſcape thro' the water; but the liquor, by the weight of the atmoſphere, ſprung into the empty part of the bubble, and about half fill'd the whole cavity; whereupon the bubble immediately ſubſided, and made the ſcale, whereto 'twas faſten'd, require 4 drams, and 38 grains, to bring it horizontal. Then taking out the bubble, with the water it contain'd, we, by means of the flame of a candle warily applied, drove out the water, which otherwiſe is not eaſily excluded at a very narrow ſtem, into a glaſs counterpois'd before-hand, and found it to weigh about 4 drams, and 30 grains, beſides a little that remain'd in the bubble, and a ſmall matter that might have been rarify'd into vapour; which, added to the piece of glaſs broken off under water, and loſt there, might very well amount to 7 or 8 grains. By which it appears, not only that water has ſome weight, or gravitates in water, but that it weighs very near, or altogether as much in water, as in the air. We repeated the experiment with another ſealed bubble as large as a great hen-egg, and with the like ſucceſs.

COROLLARY.

From hence it appears why the ſurface X, of the oil in the pipe P Q, riſes a little above the level L M of the external water. The ſlenderneſs of the pipe may indeed contribute hereto, but there would, otherwiſe, be an inequality. For ſince oil of turpentine is ſpecifically, that is, bulk for bulk, lighter than water; and ſince an equilibrium between them is here conſtantly obſerv'd; the height of the oil incumbent upon Q muſt of neceſſity be greater than that of the water incumbent on the other parts of the ſame plain G H, to make the preſſure on both ſides equal. And were the difference between the ſpecific gravities of theſe two liquors greater, the difference in the heights of their ſurfaces X and L M, would be greater alſo; as may be try'd, by ſubſtituting oil of tartar *per deliquium*, a heavy ſaline liquor, for water.

SCHOLIA.

1. What we have here ſaid, or ſhall hereafter ſay, of the properties of water, is applicable to all ponderous fluids, unleſs where cauſe of exception appears in particular caſes.

2. 'Twas before hinted, that in very ſmall glaſs pipes, many fluids (I except quick-ſilver) will appear with their ſurfaces higher than that of the liquor wherein they are immerſed; and that, as far as I have obſerv'd, in proportion to the ſmallneſs of the bore: but this being of ſmall conſequence in our preſent experiments, we ſhall take no farther notice thereof.

3. Notwithſtanding this inconvenience of ſmall tubes, we chuſe to make uſe thereof, becauſe thoſe of a wider bore are apt, upon any ſmall inequalities of preſſure, to ſuffer the oil and water to paſs by each other in their cavity, and ſo diſturb the experiment.

4. Common oil and water, or any two liquors that will not mix, may ſerve in moſt of our experiments. I made choice of oil of turpentine, becauſe 'tis light, thin, clear, colourleſs, eaſily obtainable, not apt to ſpot apparel, or greatly to adhere to porous bodies; and its offenſive ſcent is eaſily corrected by oil of rhodium, *&c.*

5. Oil of turpentine will corrode copper; whence, by digeſting it upon the crude filings of that metal, a deep green liquor is obtainable, which uſed inſtead of the colourleſs oil, will render the diſtinction of the fluids more conſpicuous.

6. For the ſame purpoſe I frequently employ a decoction of brazil, logwood, or the common red ink; which muſt be made very ſtrong and deep, otherwiſe their colour in a ſlender pipe will be ſcarce diſcernible.

7. As to the ſhape of the glaſs-veſſel, we need not be ſolicitous, tho' that of a wide-mouth'd jar be for ſome uſes the moſt convenient. The depth hereof, and the length of the tubes, muſt be determin'd by the experiments to be made.

8. It ought to be no diſcouragement, if the oil of turpentine ſuck'd up do not remain in the glaſs upon the firſt or ſecond attempt; 'tis beſt to draw up more than is neceſſary, and afterwards to let in the air gradually between the orifice of the tube and the finger, which will ſuffer the liquor to deſcend to the height required.

9. Where a conſiderable difference is required between the two fluids made uſe of, oil of tartar *per deliquium*, as we ſaid, tinged with cochineal, may ſupply the place of water, and highly rectified ſpirit of wine that of oil of turpentine: for theſe two liquors, tho' they readily unite with water, will not eaſily mix with each other. Or a very ſtrong filtred ſolution of ſea-ſalt may be ſubſtituted for water, occaſionally. I have ſometimes added oil of turpentine to ſpirit of wine and oil of tartar, and thereby obtain'd three liquors of different gravities, which will not eaſily mix; by plunging a pipe, with water in the bottom of it, below the ſurface of the loweſt of theſe liquors, and raiſing and depreſſing it in a proper manner, the experiment may be agreeably varied, but ſtill continue explicable by the principles we deliver.

10. This propoſition has been thought liable to the following objections. 1. If the upper parts of water gravitate upon the lower, the latter muſt be more denſe than the former. 2. Divers are ſenſible of no compreſſure under water. 3. Tender plants grow at the bottom of the ſea. 4. An immerſed heavy body is ſupported and drawn out with greater eaſe, than if no water were incumbent thereon; for it weighs leſs in water, than out of it. 5. A bucket of water is lighter when immerſed, than before, and weighs leſs in water, than, when empty, out of it; therefore the water in the bucket does not gravitate, either on the bucket, or the water below it: whence the ſchools have determin'd, that fluids never gravitate in their proper places. 6. Horſe-hairs preſerve any place aſſign'd them in water, and do not ſink by the incumbent weight thereof. 7. Were the propoſition true, all the lower parts of water would be in perpetual motion, and conſtantly expel the upper.

To anſwer each of theſe objections in their order. 1. If the component parts of water be perfectly hard and ſolid, the upper may preſs upon the lower without condenſing them, as would be the caſe in a heap of diamond duſt. And poſſibly the parts of water will not ſenſibly yield to ſo ſmall a weight as that of the incumbent fluid, at leaſt in thoſe heights whereat experiments are uſually made; for, in fact, water is not compreſſible by an ordinary force: nor would any abſurdity follow from our doctrine, were the lower parts of the ſea condenſed by the upper. 2. That divers are inſenſible of any compreſſure under water, proceeds from the ſurprizingly ſtrong reſiſtance of an animal body to an uniform force, at once applied to all the external parts thereof; but that, if a proportionable part of this preſſure was ſuſtain'd by one limb, and not by all the reſt at the ſame time, 'twould be greatly pain'd thereby, appears ſenſibly from covering the mouth of an open cylindrical veſſel with the palm of the hand, whilſt applied like a receiver, to be exhauſted by the air-pump: for as ſoon as the included air is withdrawn from under your hand, it will be vehemently preſs'd from without, againſt the edge of the veſſel; as if ſome weight were laid upon the back of your hand, and forced it into the hollow of the cylinder. But that ſo vaſt a weight of water, as muſt compreſs a human body at the depth of near 100 feet below the ſurface of the ſea, (where, a diver told me, he had remain'd without pain) will not ſenſibly affect it, may appear from the following experiment. To a ſmall, cylindrical, glaſs ſyphon, ſealed at one end, we exactly fitted a rammer, by means of ſoft *Fig.* 3. leather, ſo that neither air nor water could paſs between it and the ſides of the glaſs; then pouring in a proper quantity of water, and leaving ſome inches height of air above it, we let a young tadpole, the body of which animal is exceeding ſoft and tender, ſwim about in the glaſs; when a man forcing the rammer down the tube with his utmoſt ſtrength, the bulk of the tender creature ſeem'd ſomewhat diminiſh'd by the preſſure; however, it continued to move freely about in the water, and often aſcended to the top thereof; tho' the inſtrument ſtood peependicular to the horizon: nor could we perceive that the animal ſuffer'd the leaſt injury hereby. This experiment we frequently repeated, with tadpoles of different ages; whence it may fairly be concluded, that the texture of animals is ſo ſtrong, that tho' water actually preſs on water, the body of a diver, at a great depth therein, ought not to be ſenſible of its weight: for the air being here compreſs'd into an eighth part of its natural dimenſions; the tadpole ſuſtain'd a preſſure of water equal to that of a cylinder of the ſame near 300 feet high. 3. That plants ſhoot up under water, is owing to the ſubjacent preſſure of the ſame; the force whereof, we ſee, is able, when they are detach'd from their roots, to throw them up to the ſurface, notwithſtanding the direct weight of the incumbent fluid. 4. Tho' the water incumbent on a heavy body actually tends to depreſs it, yet that tendency may not cauſe it to ſink deeper; by reaſon of an equal or greater preſſure upon all the other parts of the ſame plain, wherein its bottom reſts; in which caſe, the hand that ſuſtains it will not feel the weight of the incumbent fluid, but be able to raiſe the body more eaſily here, than in the air: yet if the incumbent water be removed, as in the proof of our eleventh paradox, a great preſſure will appear to have been there ſuſtain'd by the body. 5. To paſs over that ſenſeleſs expreſſion of fluids not gravitating in their proper places, I thus ſolve the phenomenon of the immerſed bucket. Suppoſe A B C D the well, E F the rope whereby the bucket is ſuſpended *Fig.* 4. under water, with its bottom reſting on the plain I K; if the bucket wholly conſiſt of wood ſpecifically lighter than water, it muſt be ſo far from weighing upon the hand at E, that its upper parts muſt needs riſe above the top of the water: but if this veſſel be a compoſition of wood and iron, it may be ſpecifically heavier than water; in which caſe, the hand that draws it out, will feel only that weight, whereby it exceeds an equal bulk of the fluid; the water in the cavity G, and ſo much as reſts thereon, being balanced by the preſſure of the other water above the plain I K: whence the bottom of the bucket is preſſed as forcibly upwards, as the weight of the incumbent water, in the bucket and above it, tends to depreſs it; ſo that the hand has no more to lift, than the exceſs of weight of the wood and iron of the bucket, above that of an equal bulk of water. And farther to ſhew the abſurdity of that ſcholaſtic doctrine, of water's not weighing in its own element, drawn from this experiment of the bucket, I perform'd the ſame thing in water, with a ſmall box fill'd with butter, and ſome pieces of lead, to add to its weight; and I hope, it will not be pretended, that the butter and lead did not gravitate here, becauſe they were in their own element*. 6. That horſe-hairs, ſuppoſing them of the ſame ſpecific gravity with water, ſtand ſuſpended at any depth in that fluid, is eaſily accounted for upon our principles. *Fig. 4.* Suppoſe the body R, to be of the ſame ſpecific gravity with water, it cannot preſs that part of the plain S, wherein it reſts, either more or leſs than an equal bulk of water; and conſequently, tho' the whole column that ſtands above it, directly preſſes thereon, that does not ſink it; being balanced by the weight of the other water above the plain I K; whence the body will neither be depreſs'd nor rais'd. And as this muſt be the caſe at what depth ſoever the body be immers'd, it muſt neceſſarily keep in the place aſſign'd it. And I once procured glaſs bubbles ſo nicely poiſed, as to exhibit this phenomenon with a ſurprizing exactneſs. 7. If the laſt objection ſuppoſes the lower parts of water to be of any conſiderable bulk, 'tis already anſwer'd, when we ſhew'd that the body R, would retain its place aſſign'd, any where in the water. And unleſs we knew the particular magnitude and figure of the component particles of water, 'tis to no purpoſe to diſpute this point. I am, however, ſo far from thinking it abſurd, that the lower parts of water ſhould be in conſtant motion, that I take this to be the very property that conſtitutes a fluid; which, tho' not viſible, is eaſily diſcoverable by its effects. Thus, when ſalt is thrown into water, the parts near the bottom ſoon raiſe ſaline corpuſcles to the top, where they afterwards ſhew themſelves in ſmall, floating, cubical grains.

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* This matter cannot be ſet clearer than in the words of Sir *Iſaac Newton*; who obſerves, that bodies immerſed in fluids, have two kinds of gravity; the one true and abſolute, the other apparent, vulgar and relative. "Abſolute gravity (ſays "he) is the whole force wherewith a "body tends downwards; but the relative "and vulgar is that exceſs of gravity, "whereby a body tends downwards, more "than the fluid which ſurrounds it. By "the former kind, the parts of fluids, and "of all bodies, gravitate in their proper "places; and, by their joint weights, "compoſe the weight of the whole. For "every whole has weight, as is evident "in veſſels fill'd with liquids; and the "weight of the whole being equal to the "weights of all the parts, muſt of neceſſity be compoſed of them. But bodies, "by the latter kind of gravity, do not "gravitate in their own places; that is, "do not, when compared with one another, pregravitate; but mutually hindering each other's endeavour to deſcend, they remain in their places, as "if they had no weight. Bodies in the air "which do not pregravitate, are thought "by the vulgar not to be heavy; but thoſe "which pregravitate, they judge to be "heavy, ſo far as the air does not ſupport "them: ſo that the weight of bodies among "the

PARADOX II.

A lighter fluid will gravitate upon a heavier.

If you ſuck up fair water in a ſlender glaſs pipe, to the height of three or four inches, nimbly ſtop the upper orifice with your finger, plunge the lower into a glaſs of oil of turpentine, till the ſurface of the oil in the veſſel riſe ſomewhat higher than that of the water in the tube, and then, laſtly, remove your finger; the water will not fall out, but continue ſuſpended by the preſſure of the oil; as appears evident from the foregoing propoſition, whereof this is only the converſe. And if either more oil be poured into the veſſel, or the tube be immerſed deeper therein, the water will be buoy'd up towards the top of the pipe; that is to ſay, a heavier fluid will be raiſed by a lighter. Since then, by the preceding propoſition, the liquor riſes in the pipe by the gravity of the external fluid, it follows, that a lighter liquor will gravitate upon a heavier.

COROLLARY.

As the ſurface of the oil in the tube, was always, in the former experiment, higher than that of the external water, ſo here, the ſurface of the water in the pipe, will conſtantly be lower than that of the oil; becauſe in the plain E F, the cylinder of water I G, contain'd in the pipe I H, will *Fig.* 5. upon account of its greater gravity, preſs as much upon the part I, as the oil K I, I L, which is ſpecifically lighter than it, does upon the other parts of the ſame plain E F.

SCHOLIA.

1. This ſecond paradox may be farther confirm'd from the common phenomena of glaſs bubbles, the explication whereof is as follows. The bubble X, conſiſting of glaſs, which is ſpecifically heavier than air, which is *Fig.* 6. lighter than water, and of water itſelf; whilſt the whole compoſition is lighter than an equal bulk of water, it will float; but if it grows heavier than ſo much water, it muſt, according to the laws of hydroſtatics, ſink. When, therefore, there happens any conſiderable preſſure upon the water wherein the bubble is generally immerſed; ſince glaſs is a ſolid body, and

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" the vulgar, is only the exceſs of their " real weight above that of the air. And, " therefore, they call thoſe things light, " which being leſs heavy than the air, " and yielding to its greater gravity, " mount upwards. But theſe bodies are " only comparatively light, not really ſo; " for they will deſcend *in vacuo*. Thus " bodies, which by reaſon of their greater " or leſs gravity, deſcend or aſcend in " water, are but comparatively and apparently heavy or light; and their comparative and apparent levity, is the exceſs or defect, whereby their real gravity either exceeds or falls ſhort of the " gravity of water. But whatever bodies " neither deſcend by pregravitating, nor " aſcend by yielding to one that pregravitates; tho' they ſtill, by their real weights, " increaſe the weight of the whole; yet " comparatively, and in a popular ſenſe, " they do not weigh in water." *Newton. Princip.* p. 264.

water incompreſſable, but the air included in the bubble, elaſtic, and eaſily compreſſed; this will ſhrink, and ſo poſſeſſing leſs ſpace than before, the contiguous water will ſucceed in its place; which being a body that is a thouſand times ſpecifically heavier than air, the bubble thereby becomes heavier than an equal bulk of water, and conſequently ſinks: but, upon the removal of this preſſure, the pent-up air will, by its own ſpring, free itſelf from the intruding water; whence the compoſition of the bubble growing lighter than an equal bulk of water, it will, from the bottom, immediately emerge to the top.

2. Into a long glaſs pipe, ſealed at one end and open at the other, pour common water, till about half a yard thereof remains unfill'd. In the next place poiſe a glaſs bubble, with a ſlender neck, ſo that a ſmall addition of weight would ſink it; and place it on the ſurface of the water in the tube; then continue pouring oil of turpentine gently thereon, till it riſes to a ſufficient height; when the bubble will deſcend to the bottom, and there continue as long as the oil remains at that height above the water. The reaſon whereof is, that the oil, tho' lighter than water, gravitates upon that fluid below it, and thereby forces a part thereof into the open orifice in the neck of the bubble, which thence becomes heavier than an equal bulk of water, and conſequently deſcends to the bottom; and the cauſe of this ſubmerſion, the preſſure of the oil, continuing, it muſt neceſſarily remain there. In confirmation hereof, it may be added, that if the cylinder of oil be diminiſh'd, or taken away, the bubble will again aſcend to the top of the water; becauſe the water being reliev'd from the preſſure of the oil, the air, by virtue of its own elaſticity, recovers its former dimenſions, and thereby renders the bubble as light as before.

3. The firſt paradox may, alſo, be confirmed by the following experiment. Having provided a tube and bubble, as juſt mention'd, pour water into the tube, to the height of 6 inches, then caſt in the bubble, and keep it conſtantly plung'd below the ſurface, whilſt more water is ſlowly pour'd into the tube, to a conſiderable height; when, the bubble, which before attempted to emerge, will by the additional weight of the incumbent water, be depreſſed to the bottom of the tube, and there continue, whilſt the ſame weight of water remains.

4. Tho' the weight of the incumbent water alone here cauſes the bubble to ſink, yet, if it were not kept immerſed, a ſtill greater weight of water would not depreſs it; becauſe, being left to itſelf, it would riſe with the fluid, and never permit the ſame to come ſufficiently above it for that purpoſe. And that the weight of the incumbent water really ſinks the bubble, and keeps it at the bottom, appears from taking away a ſufficient part thereof; for, when that is done, the bubble will preſently, and of itſelf, begin to float up towards the ſurface, from whence it may eaſily be made to ſubſide, by pouring back the ſame water that was taken out.

5. The reaſon why oil of tartar *per deliquium*, was not here made uſe of, inſtead of fair water, is, that in ſuch ſmall pipes, as thoſe employ'd about the firſt experiment, I found that liquor ſo ponderous, as to flow

down into the oil of turpentine, whilſt the oil paſſed upwards by it, along the other ſide of the tube: whence I am ſurprized, that M. *Paſchal* ſhould ſay, that if a tube about 14 feet long, with one orifice placed 14 feet under water, be full of quick-ſilver, it will not all run out thereat, tho' the top be expoſed to the air, but ſtop at the height of one foot; becauſe the moment it acquires in falling, muſt, probably cauſe the whole to come out. My own trials with quick-ſilver, and ſmall tubes, with this more favourable circumſtance of oil of turpentine, and oil of tartar, tempt me to ſuſpect, that M. *Paſchal* never made the experiment, at leaſt, not in a tube ſo large as his ſcheme ſuppoſes. Experiments, that are only true in ſpeculation, ſhould be propoſed as ſuch, for they may often fail in practice.

6. This ſecond paradox, I am ſenſible, contradicts the current opinion of the ſchools, and even of many modern mathematicians, and writers in hydroſtatics; but whether the cauſe of gravity be the truſion of any ſuperior ſubſtance; or the magnetic attraction of the earth, *&c.* there is in every heavy body, a conſtant tendency towards the centre of the earth, that cannot be deſtroy'd by the interpoſition of another heavier body, tho' its nearer approach towards the centre, may be hindred thereby; the latter, from its greater gravity, obtaining the lower place. But then, the lighter body tending downwards, muſt needs preſs upon the heavier that lies in its way; and jointly therewith, preſs upon whatſoever body ſupports them both, with a weight made up of their united gravities. Some men of learning are kept from acknowledging this truth, by finding that a lighter fluid, ſurrounded by a heavier, does not deſcend, but emerge to the top; whence they conſider the former, not as a heavy body, but a light one. 'Tis true, in reſpect of the heavier liquor, the leſs heavy may be called light; yet it ſtill retains its own abſolute gravity, or tendency downwards, as ſtrongly as before; tho' by a contrary and more powerful tendency of the contiguous liquor upwards, ſuch its endeavour does not appear. Thus whilſt a piece of light wood emerges from under water, it retains its gravity in its aſcent, being only buoy'd up by the water whoſe ſpecific gravity is the greater; ſo that the ſum of the water, and the aſcending wood. weighs more than the water alone would do; and therefore, when this wood floats, with ſome part above the ſurface of the water, a lighter body actually gravitates upon a heavier. Thus, alſo, were a man weigh'd in a balance, with a ſtone in his hand, which he lifts above his head; tho' that may ſeem light, in reſpect of his body, yet it loſes nothing of its natural weight, neither during its aſcent, nor afterwards: for the hand that raiſes it up, will conſtantly perceive its tendency downwards, and the ſtone; and the man together, will weigh no leſs in the ſcale, than if he did not ſupport it, but both were weigh'd a-part, and their reſpective weights afterwards added together. And thus, laſtly, if a glaſs of water, with a large quantity of common ſalt, in powder, be balanced in the ſcale, whilſt the ſalt gradually diſſolves and becomes one body with the water; yet the weight here will not be diminiſh'd as the ſalt comes to be ſupported by the fluid; but both together will gravitate, as at firſt, on the ſcale that ſupports them.

PARADOX III.

If a body be wholly, or in part, immerſed below the ſurface of water, its lower part will be preſſed upwards by the water contiguous to it, from beneath.

This appears from the firſt experiment; for where-ever the loweſt part of the body be, in either caſe, ſuppoſed, all the parts of the plain, wherein it reſts, are preſſed upwards. And if this force under the body be greater than that wherewith it tends downwards, 'twill buoy it up; but if the body be ſpecifically heavier than water, the tendency of the contiguous fluid from beneath upwards, proves only ineffectual to raiſe or ſupport it, but is by no means deſtroy'd; the heavy body being conſtantly reſiſted and retarded by the water, as it would be in a pair of ſcales, were it hung at one end, and an equal bulk of water at the other end of the beam.

Fig. 5. The ſame truth is farther manifeſt from the third experiment; for if the tube, wherein the water is ſuſpended, be plunged deeper into the oil, or if more oil be poured into the veſſel, the water in the tube will riſe the higher; which could not happen, unleſs the oil, tho' ſpecifically lighter, preſſed againſt the lower ſurface of the water more forcibly than the water tends, by its gravity, downwards. And even when the two fluids balance each other, the oil continually preſſes upwards againſt the lower ſurface of the water; for herein conſiſts its conſtant reſiſtance to the perpetual endeavour of the water to deſcend. And ſince the ſame phenomenon happens, whether the water be ſuſpended in oil, or *vice verſa*, the propoſition holds equally true, let the body be ſpecifically heavier, or lighter than the fluid wherein it is immerſed.

SCHOLIA.

Fig. 7. 1. That water makes a reſiſtance to bodies which ſink therein, is thus demonſtrated. Suppoſe the pipe E F contains oil of cloves, or any other oil, that is ſpecifically heavier than water; that the oil in the tube, and the external water, are in equilibrium; and, laſtly, the pipe to be ſlowly raiſed towards the top of the veſſel: 'tis evident, that drops of oil will, during the tube's aſcent, fall from the bottom thereof to that of the glaſs; but much more ſlowly than if they fell in the ſame manner thro' air.

2. To compute the quantity of preſſure againſt the lower parts of a drop, ſuppoſe it to be G, touching the plain H I; we muſt obſerve, that if the drop of oil were not there, its place would be ſupplied by an equal bulk of water, which being of the ſame ſpecific gravity with the reſt of the fluid in the veſſel, the ſurface H I would be every where equally preſſed, and conſequently no part thereof be diſplaced. But the drop of oil being heavier than an equal bulk of water, that part of the ſurface whereon it reſts is more preſſed than any other equal part of the ſame; and therefore will give way to the deſcending drop. And as this muſt be the caſe in all the other ſurfaces, as well as in H I, the drop will conti-

nue falling, till it arrives at the bottom of the glaſs. And if the drop G were not ſpecifically heavier than water, but barely equal thereto, the contiguous part of the ſurface H I would be no more preſſed than the other equal parts thereof, and conſequently it would be neither depreſſed nor raiſed, but the drop G continue in the ſame place: whence we conclude, with *Archimedes*, and others, that a body immerſed in a fluid, of the ſame ſpecific gravity with itſelf, will reſt at any aſſignable depth therein. Since, then, if the drop G be of the ſame ſpecific gravity with water, it will neither ſink, nor emerge; if it be heavier, 'twill only fall by the weight whereby it exceeds an equal bulk of water; for this alone is the cauſe of its deſcent, the other being wholly reſiſted by the water; and conſequently it loſes in the water the weight of an equal bulk of water, weighed in the air.

3. This grand theorem is thus eaſily confirm'd by experiment. Hang a piece of lead, tied to an horſe-hair, from one ſcale of an exact balance, counterpoiſe it, and ſuffer the lead to ſink down, and hang freely, in a veſſel of water; and you will find the former counterpoiſe much too heavy for the lead, part of it, therefore, being taken away, till the balance be reduced to an equilibrium, 'tis eaſily ſeen what proportion of weight is loſt in the water. If, then, another piece of lead be weigh'd in air, where it equals, ſuppoſe, 12 ounces, and afterwards be hung to the ſcale by an horſe-hair, as the former, and weigh'd in water, you may conclude that 11 ounces, (ſuppoſing lead to be to water as 12 to 1) put in the oppoſite ſcale, will now reduce the beam, how uneven ſoever it might hang in the air, to an horizontal poſition.

COROLLARIES.

1. From this preſſure of water againſt the lower ſurface of bodies immerſed therein, may be deduced the cauſe why light ſubſtances emerge to the top thereof, which has hitherto remain'd an inexplicable phenomenon. Suppoſing, then, a pipe almoſt filled with oil of turpentine; if, as in our firſt experiment, the lower orifice be plunged under water, tho' not near ſo deep as the oil is high, and the upper end be gradually unſtopp'd, the oil will fall in drops from the bottom of the tube, and preſently emerge to the ſurface of the water, notwithſtanding they are preſſed as well downwards as upwards: the reaſon whereof is, that the upward tendency of the water, contiguous to the lower ſurface of the oil, is ſtronger than the tendency of the oil, and the incumbent water downwards. Thus, when the drop G touches the two parallel plains H I, K L, 'tis evident, that the lower part of the drop G is more preſſed by the water than the upper part M, becauſe upon the whole ſurface K L there is only an uniform preſſure of the water A K B L, whilſt upon all the parts of the ſurface H I there lies a greater quantity of water, A H B I, except upon the part N, where the oil G being lighter than an equal bulk of water, and expoſed to a greater preſſure from beneath, than its own gravity, with that of the incumbent water, can reſiſt; it muſt neceſſarily give way, and

Fig. 7.

STATICS.

be impelled upwards. And as the ſame reaſoning holds for all the points in its aſcent, it muſt conſequently be driven gradually to the ſurface, and there float. Or, in other words, when any body, ſpecifically lighter than water, is immerſed in that fluid, it ſuſtains the preſſure of two columns of water, one againſt its upper, and the other againſt its lower part; and as the height of theſe columns muſt be computed from the top of the water, the lower part of the immerſed body is preſſed upon by a column longer than that which preſſes upon the upper, by the thickneſs of the body, which therefore will be preſſed more upwards than downwards. And the greater difference there is in the ſpecific gravities of the water and the body made uſe of, the ſwifter, *cæteris paribus*, will be the aſcent, becauſe of the greater preſſure upon all the other parts of our imaginary plain, than upon that which is contiguous to the bottom of the aſcending body.

2. Hence alſo we may ſolve that hydroſtatical problem; why if two cylindrical pieces of wood, one of them twice the length of the other, be plunged to an equal depth under water, and thence ſuffer'd to emerge, at the ſame time, the longer gains the top before the ſhorter: for ſuppoſe O P to be one of theſe bodies, and two feet long, Q R the other, and one foot, and that they both reſt upon the ſame plain, parallel to the upper ſurface of the water, and three feet diſtant therefrom; there will be a lateral preſſure of the incumbent water upon the lower ends of each of theſe bodies, as great as that upon the equal parts of the plain whereon they reſt; but upon the upper end of the ſhorter body Q R there will preſs a column of water two feet high, whilſt the taller P O ſuſtains only a pillar of one foot; ſo that both being ſpecifically lighter than water, they will be impell'd upwards: but the column conſiſting of one foot of wood, and two feet of water, will, by its gravity, reſiſt this impulſe more than that which is made up of two feet of wood, and but one of water; whence the reaſon of their unequal velocities is evident.

Fig. 7.

3. From hence alſo we may probably deduce the reaſon why, in diſtilling aromatic oils, with water, the whitiſh liquor, which frequently comes over, long continues turbid; for this chiefly proceeds from the exceeding minuteneſs of the drops whereof it conſiſts, allowing the height of the water, preſſing upon the upper part thereof, to be almoſt as great as that which preſſes againſt the lower, ſo that the drops are raiſed but very ſlowly, tho' they will, at length, emerge; eſpecially when, by their frequent occurſions, they have form'd drops of a larger ſize.

4. From this propoſition it likewiſe follows, that all bodies floating upon a fluid, ſink till the irimmerſed part be exactly equal in bulk to ſuch a quantity of the fluid as equals the whole weight of each body. For inſtance, if V be a cubic yard of wood, 6 pounds in weight; ſince 'tis heavier than air, it will ſink in water, ſuppoſe to the plain X W, where it muſt neceſſarily reſt; for all the other equal parts of that plain X W being preſſed upon by columns of water, equal in height to X A, if the whole weight of the cube be greater than that of as much water as equals the

Fig. 8.

immerſed part thereof, it muſt ſink lower, becauſe the ſubjacent part at V, would then be more charged than the reſt. On the contrary, if the cube were lighter than the water, whoſe place the immerſed part poſſeſſes, it muſt, by the greater preſſure of the water upon the other parts of the plain X W, than upon that contiguous to the wood at V, be impell'd upwards, till the preſſure of the whole cube upon the part that ſuſtains it be equal to that of the water upon the reſt of the plain, and conſequently be the ſame with the water whoſe place the immerſed part poſſeſſes; the lightneſs of that part, in reſpect of ſo much water, being compenſated by the weight of what remains unimmerſed above the level of the water. Thus, when a piece of wood is thrown into water, tho' the moment it acquires in falling carries it thro' many imaginary plains below its proper ſtation; yet the ſuperior preſſure to which each of thoſe plains is expoſed in all other parts, but in that contiguous to the bottom of the wood, preſently impels it up again, till at length it reſts in the place we have aſſign'd it.

STATICS.

SCHOLIUM.

This ingenious and uſeful propoſition, relating to floating bodies, is mathematically demonſtrated by *Archimedes* and his commentators, and may be farther manifeſted to the eye by the following experiment. Having poured a convenient quantity of water into a large deep glaſs, and placed another deeper, that was properly ſhaped for ſwimming, within it; we furniſh'd the latter with balaſt, a deck, and other parts of a ſhip, till it ſunk down to ſome particular marks we had placed on the outſide. Then obſerving how high the water reach'd in the larger veſſel, we carefully placed two or three marks in a level with the horizontal ſurface of the water; and taking out the floating glaſs, we wiped the outſide dry, and weigh'd it, with all its rigging, in an exact pair of ſcales, and afterwards pour'd an equal weight of water into the large glaſs, which we found reach'd up to the marks whereto our counterfeit ſhip had raiſed it. This experiment we ſeveral times repeated in veſſels of different ſizes, ſhapes, and lading. From whence it appears, that the floating veſſel, with all it contained and ſupported, was equal in weight to the water whoſe bulk equall'd the immerſed part of the veſſel, had it been cut off from the other, by a plain continu'd from the horizontal ſurface of the water. This theorem, with what we have brought to confirm or illuſtrate it, may be apply'd, *mutatis mutandis*, to a ſhip with all her rigging and lading; for 'tis univerſal, that the weight of a floating body equals that of the water whoſe ſpace the immerſed part poſſeſſes. And hence the learned *Stevinus* ſomewhere ſays, that a whole ſhip, with whatever belongs to it, preſſes no more upon the bottom it floats over, than a bulk of water equal to the immerſed part of its hull.

PARADOX IV.

A competent preſſure of an external fluid is, alone, ſufficient to raiſe the water in pumps.

The truth of this propoſition may appear from the preceding experiments; however, we ſhall give it a farther illuſtration and proof, as follows. Suck deeply tinged water into a ſlender glaſs-pipe, to the height of an inch; and nimbly ſtopping the upper orifice, immerſe the lower end into a glaſs half fill'd with the ſame tinged liquor, till the ſurface of that in the pipe be an inch below the top of the external fluid; next, pour oil of turpentine thereon, till it ſwim three or four inches above the water; when eaſing your finger, gently, from the upper orifice of the pipe, to admit a communication between the internal and external air, the liquor in the tube will be impell'd up almoſt as high as the ſurface of the external oil. Now, it cannot here be pretended, that the aſcent of the liquor, in the pipe, is owing to nature's abhorrence of a *Vacuum*; ſince, as the pipe is full of air, and its orifice open, there's no danger of a *Vacuum*, tho' the water did not riſe; the air and water remaining contiguous as before. The true reaſon, then, of this aſcent, is, that upon all the other parts of the imaginary plain cutting the immerſed end of the tube, there is a preſſure of water and oil ſwimming thereon, equal to four or five inches height of water; whilſt, upon that part where the liquor contain'd in the pipe reſts, there is only the preſſure of one inch of water; whence the parts near the immerſed orifice, muſt be neceſſarily thruſt out of their places by thoſe of the water that are more preſſed, till ſo much liquor be forced up into the pipe, as makes the preſſure on that part of the imaginary plain equal to that of every other ſuch part; when the water will riſe no farther, but by reaſon of the equilibrium, reſt a little below the ſurface of the oil; the ſpecific gravity of this being leſs than that of water. In like manner, it may happen in pumps; for as the oil of turpentine, tho' lighter than water, by reſting upon the ſurface of the external fluid, forces up the liquor, within the pipe, far above the external water; ſo the air, tho' lighter than oil of turpentine, reaching to the height of many miles, and reſting upon the water in a well, will preſs that water up the cylindrical cavity of a pump much higher, if all impediments were removed, than the ſurrounding water. Now the obſtacle here is either the ſucker, or the preſſure of the external air; the whole uſe of the ſucker is, therefore, to free the water in the pipe from any impediment to its aſcent; as, in our experiment, the ſides of the tube ſufficiently defend the water therein from any preſſure of the external oil that might obſtruct its riſing. Laſtly, as the liquor in our pipe was forced up, till the cylinder raiſed, became a balance to the preſſure of the water and oil on the outſide; ſo in pumps, the water riſes but to a certain height, *viz.* thirty-three, or thirty-four feet, beyond which it cannot be impell'd; becauſe at that ſtandard the preſſure of the water in the pump upon that part of the imaginary plain wherein its lower orifice lies, is the ſame with that the other equal parts thereof ſuſtain, from ſo much of the external water and atmoſphere as reſts thereon. And this may ſuffice to ſhew, that nothing more than a competent weight of an external fluid, ſuch as the air, is requiſite to raiſe the water in pumps.

SCHOLIUM.

For a farther confirmation of this propoſition, and, at the ſame time, to ſhew that a ſmall quantity of quick-ſilver may be ſuſpended in an open tube; I ſhall here offer another experiment. Having ſuck'd into the lower end of an open glaſs-tube, the diameter of whoſe bore was above the ſixth of an inch, near half an inch height of quick-ſilver, and nimbly ſtopped the upper end with my finger; I plunged the mercury into a deep glaſs of oil of turpentine, being careful not to open the upper orifice, till the quick-ſilver was ſunk eighteen or twenty times its own height below the ſurface of the oil; after which, it remain'd ſuſpended, while the tube continued unſtopped: and as if I raiſed it towards the upper ſurface of the oil, it would drop out; ſo if I depreſſed it deeper into that fluid, it would be impelled higher up into the pipe: and, by this means, ſo ponderous a body as mercury was made ſometimes to riſe, and ſometimes to fall; but ſtill to continue ſupported by a liquor ſpecifically lighter than common ſpirit of wine. This experiment, however, may eaſily miſcarry, unleſs great care be uſed; and, after all, the oil will ſoon inſinuate itſelf between the ſides of the pipe, and ſo ſhort a cylinder of mercury; for which reaſon, I here prefer water to oil of turpentine.

COROLLARY.

Since a cylinder of mercury, about thirty inches high, is equal in weight to one of water, about the height of thirty-three, or thirty-four feet; 'tis evident, that the preſſure of the external air, which can raiſe and ſuſpend that height of water, will do the ſame to the proportionable one of mercury, in the *Torricellian* experiment.

PARADOX. V.

The preſſure of an external fluid will keep an heterogeneous liquor ſuſpended at the ſame height, in tubes of very different bores.

Pour a ſufficient quantity of deeply tinged water into a clear, deep, wide-mouth'd glaſs, and cloſe it with a broad, thin cork, wherein various round holes, of different magnitudes, have been burnt; into each of which you may thruſt an open glaſs-tube of a correſponding ſize, ſo that they may ſtand parallel to one another, and perpendicular to the ſurface of the fluid, wherein they are immerſed. Beſides theſe holes, another aperture muſt be made in the cork, to receive the ſmall end of a glaſs-funnel, by means whereof, oil may be conveyed into the veſſel, when ſtopped; and in the ſlender part of this funnel, 'tis proper to place ſome cotton, to break the force of the deſcending oil, which might otherwiſe fruſtrate the experiment.

riment. Things being thus prepared, pour oil of turpentine thro' the funnel, till the tinged water be preſſed up into all the pipes, and riſe to a conſiderable height therein, above the lower ſurface of the oil; when you'll plainly perceive the liquor to ſtand at the ſame level, both in the greater and ſmaller tubes, while in each it is plainly ſuſtain'd at that height above the other water, by the preſſure of the external oil; which being ſpecifically lighter than water, its ſurface will remain ſomewhat higher on the outſide of the tubes, than the tinged water they contain. And if, by means of a ſyphon, apply'd in the place of the funnel, the oil be gradually drawn off from the water; as the depth and preſſure of the ſame decreaſes, the liquor in all the pipes, both great and ſmall, will gradually ſubſide. Fig. 9. For, ſuppoſe E F, the ſurface of the water within-ſide of the tubes, as well as without them, before any oil ſwims upon it; the lightneſs of that fluid, compared to water, will prevent it, when pour'd thereon, from getting into the cavity of the pipes L, M, N: and, therefore, riſing on the outſide thereof, it muſt, by its gravity, preſs the water, and impel it up into them. And if the upper ſurface of the oil reach to G H, and that of the water deſcend to I K; by means of the quantity of oil made uſe of, the liquor in the pipes ought to have its ſeveral upper ſurfaces in the ſame level with each other, notwithſtanding the great inequality of their bores; ſince that part of the plain I K, whereon reſts the circular orifice of the largeſt tube L, is no more preſſed by the incumbent water, than any other equal part of the ſame is by the oil it ſuſtains; the oil having only an additional height on the plain I K, to balance the preſſure of the liquor in the pipe. And tho' the tube L were twice its preſent bigneſs, 'twould but preſs the ſubjacent plain I K, equally with the oil on the other equal parts. Nor ought this preſſure of the external oil to raiſe the water in the ſlender pipe N, above the ſurface Q, in the ſame level with O; for were the liquor higher in the ſmall pipe, it muſt preſs that part of the plain I K, whereon it reſts with a force ſuperior to that of the oil upon the other parts of I K, which is greater than the oil could reſiſt; whence, conſequently, the liquor in the ſlender pipe muſt ſubſide, till its ſurface fall below that of the external oil; for, till then, the different fluids cannot reſt in equilibrium. In a word, it matters not how wide the cylinder of water be, which the oil is to ſupport, provided it riſe no higher than the difference in the ſpecific gravities of the two fluids will admit of; for in this caſe, the preſſure of water on the ſuſtaining part of the plain, will be equal to the preſſure of the external oil on all the other parts thereof; and, conſequently, the two liquors muſt remain perfectly balanced.

SCHOLIA.

1. It may here be obſerv'd, that there's no neceſſity the glaſs-tubes L, M, N, ſhould be of the ſame length; ſince, as their lower orifices are open, the liquor will riſe to the ſame height within them, tho' they lie not in the ſame plain below the water.

2. We all along ſuppoſe, and particularly in the laſt experiment, that the ſmalleſt glaſs tubes employ'd are of a moderate ſize, and not exceeding narrow; or, in caſe they are, that allowance be made for the property of water's riſing in them to a greater height, than can be attributed to the balance either of water or oil, that keeps it there ſuſpended.

PARADOX VI.

The direct preſſure ſuſtain'd by a body, placed any where under water, with its upper ſurface parallel to the horizon, is that of a column of water, whoſe baſe is the horizontal ſuperficies of the body, and height the perpendicular depth of the water; and if the water preſſing upon a body, be contained in open tubes, its preſſure is to be eſtimated by a column of the ſame, the baſe whereof equals the lower orifice of the pipe, and height a perpendicular from thence to the top of the water: and this, tho' the pipes ſtand obliquely, be irregularly ſhaped, or wider in ſome parts than the ſaid orifice.

The former part of our propoſition we might, with *Stevinus*, make more general, and thus demonſtrate. Suppoſe A B C D, a ſolid rectangular figure of water, whoſe baſe D C, is parallel to the horizon, and height G E, a perpendicular let fall from the upper to the lower ſurface of the water; if that part of the baſe E F, be charged with more water than G E F H, the overplus muſt come from the adjacent water; ſuppoſe it then, if poſſible, to come from A G E D, and H B C F: then, for the ſame reaſon, the baſe D E, will have more water incumbent thereon than A G E D; and the like may be ſaid of F C, whence the whole baſe D C will ſuſtain a greater weight than that of the whole water A B C D, which is abſurd. By the like reaſoning we prove that the baſe D C, ſuſtains no leſs a weight than that of the water G H F E; ſince, then, it ſuſtains neither a greater weight nor a leſs, than the column of water G H F E, it muſt exactly ſuſtain the ſame. *Fig.* 10.

To this demonſtration we ſhall annex ſenſible experiments, to confirm and illuſtrate each part of our propoſition. Firſt then, ſuck oil of turpentine into a ſlender glaſs ſyphon, till it fill the ſhorter leg, and riſe two or three inches high in the longer; then nimbly ſtop the upper orifice with your finger, and plunge the lower part of the ſyphon into a deep glaſs of water, till the ſurface of the oil in the longer leg riſes but little higher than that of the external water, and upon removing your finger, the ſurface of the oil will either entirely, or almoſt, preſerve its former ſtation; when if the ſyphon be immerſed deeper, the oil in the ſhorter leg will be depreſs'd; but upon gently raiſing the ſyphon towards the top of the water, it will exceed its former ſtation, gradually flow out in drops, and emerge to the ſurface. Now, ſince the water, at firſt, kept the oil in the longer leg, ſuſpended no higher than it would have been by a cylinder of water, equal in baſis to the orifice of the ſhorter leg, and reaching, in length, from thence to the top of the water, as may be eaſily ſeen in a ſyphon with legs ſufficiently long; and ſince, when by raiſing the ſyphon, *Fig.* 11.

the height of the incumbent water was diminiſh'd, the oil overbalanced the water, and run out; it may be fairly concluded, that tho' much of the water in the veſſel lay higher than the immerſed orifice of the ſyphon, yet no more gravitated thereon, than what ſtood directly over it; that is, ſuch a column as our paradox requires.

COROLLARY.

It follows from hence, that water may preſs as regularly upon an immerſed body, when not reſtrained by pipes, as when included in them; we may, therefore, properly conceive a particular column of water, tho' it has no actual bounds, in an unlimited quantity of the ſame fluid.

The firſt part of our paradox holds equally true, whether the incumbent water be free or confined in veſſels of any irregular ſhape. Thus, tho' the ſhorter leg of the ſyphon be faſhion'd into a funnel, and filled with water, the oil in the other leg will reſiſt the preſſure thereof, ſo that the ſurface of the oil in the longer leg will riſe but little above that of the water in the funnel. For farther confirmation hereof, we try'd the experiment in a ſyphon, in one leg whereof, a glaſs ſphere was made to communicate with the upper parts of the ſame. Into the uniform leg of this ſyphon, we poured a proper quantity of oil of turpentine, and fill'd the lower and globular part of the other with water; which proved inſufficient to ſuſtain the oil at a greater height, than if this leg alſo had been uniform: all the water in the ſpherical cavity, which fell not directly over the lower orifice thereof, being ſupported by the ſides of the ſame. And when the irregular leg was fill'd with oil, and the other with water, the former fluid would not ſuſtain the latter to an equal height, notwithſtanding its exceſs of quantity contain'd in the concave ſphere. To clear up this matter ſtill farther, we poured mercury into the ſyphon A B C D, till it almoſt reach'd the bottom of the globular part, in the ſhorter leg, and to an equal height in the longer; then letting a proper quantity of water run into the longer leg, it impell'd the mercury in the ſhorter leg, till it more than half fill'd the cavity of the ſpherical part E, the tube A B, not proving ſufficiently long to allow of its being quite fill'd; when we obſerv'd the ſurface of the quick-ſilver H G, to lie as high as the different ſpecific gravities of the two fluids requir'd: ſo that notwithſtanding the great weight of the mercury contain'd in the concave ſphere E, no more preſſed upon the ſubjacent ſlender part of the leg E C, than ſtood directly over its lower orifice: whence the water in the leg A B, appear'd preſs'd only as much as if the leg C D had been uniform, and without the ſpherical cavity E. And thus, if the ſphere had been made larger, a ſmall quantity of water in the leg A B, would have ſomewhat raiſed a much greater weight of mercury than its own.

Fig. 12.

Fig. 13.

The ſecond part of our preſent propoſition is proved thus. We took three open glaſs pipes, of the irregular figures in the ſcheme, and plunged them in a glaſs veſſel of water, the two crooked ones ſtanding very oblique to the upper ſurface thereof, and the higher ends of them all coming

Fig: 1: p: 56.

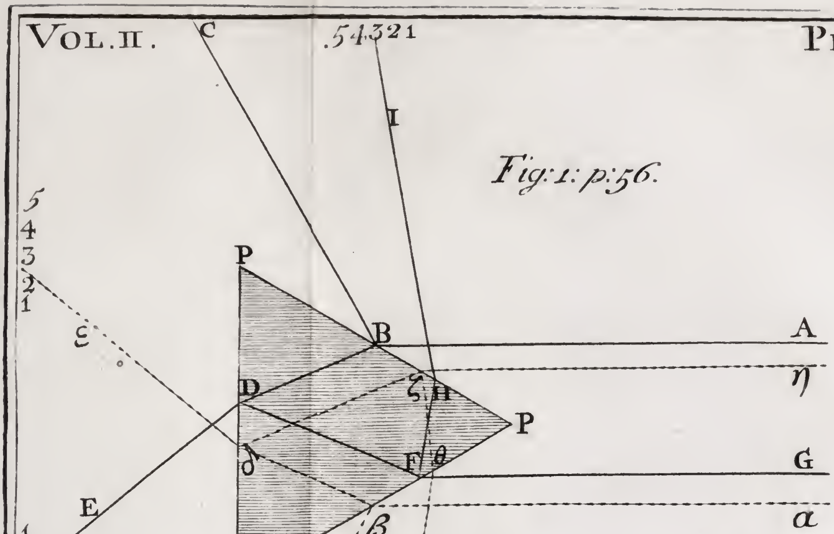


Fig. 2. p. 287.

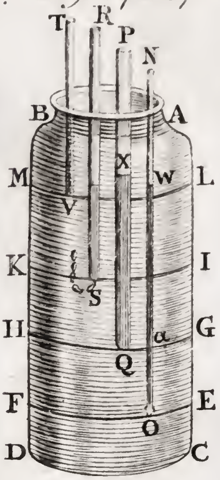


Fig 3. p. 291.

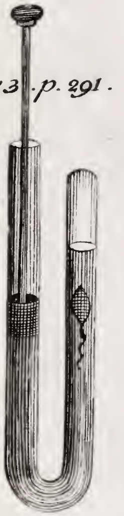


Fig. 4. p. 291.

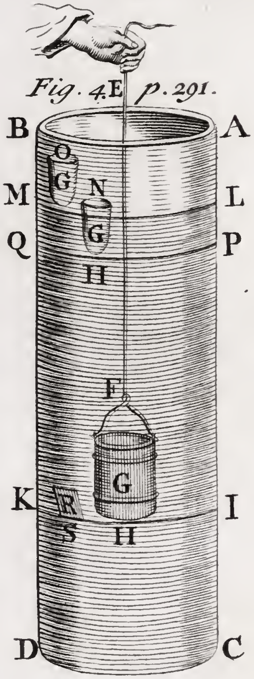


Fig. 5. p. 293.

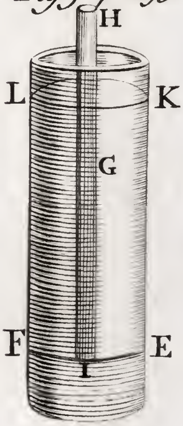


Fig. 6. p. 293.

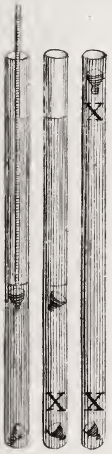


Fig. 7. p. 296.

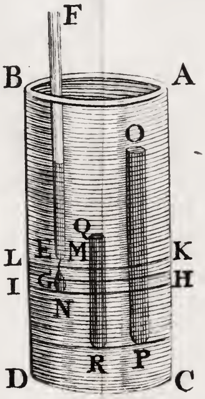


Fig. 8. p. 298.

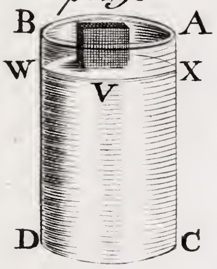


Fig. 9. p. 302.

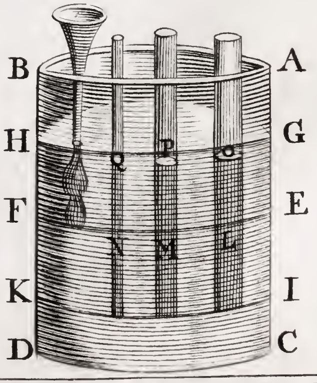


Fig. 10. p. 303.

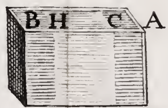
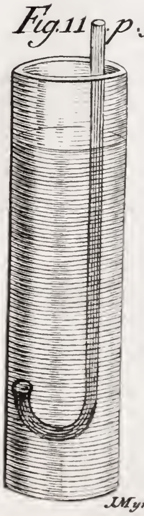


Fig. 11. p. 303.



J Mynde

thro' particular ſtrait holes made in a cork fitted to the veſſel's mouth, whilſt the water aſcended to a certain height, thro' the lower orifice of every pipe; then pouring a conſiderable quantity of oil of turpentine thro' a funnel, into the veſſel, the water was thereby impelled to the ſame height, of two or three inches, into the ſeveral tubes: whence 'tis apparent, that notwithſtanding the irregularity of ſhape in the pipes, and the different wideneſs in each of their bores, the preſſure of the contain'd water upon their lower orifices did not exceed that of five cylinders of the ſame, with each of thoſe orifices for baſes, and the perpendicular depth of the water and oil therefrom for their height. For had each of the pipes contain'd only ſuch a cylinder, its upper ſurface would have ſtood at the ſame height; and if it did not depreſs the ſubjacent fluid as much as the external force tended to raiſe it, that exceſs of preſſure muſt have impelled it higher. Since then the water roſe to the ſame height in the ſeveral pipes, tho' two of them, being greatly inclined, contain'd much more water than if they had ſtood erect; by a like way of reaſoning, we may conclude, that the imaginary plain cutting their immerſed ends, ſuſtain'd no greater preſſure than that of a ſhorter erect cylinder of water. For in all caſes where the pipe is either inclined, or wider in one part than in another, the weight of the liquor it contains is not wholly ſupported by the body contiguous to the lower orifice, but in ſome meaſure by the ſides of the pipe. Thus when oil of turpentine, in a ſlender tube, balances the preſſure of the external water, if the tube be barely inclined to the ſides of the containing glaſs, a conſiderable quantity of water will get up into the pipe; becauſe the oil no longer reſting wholly upon the water, but partly upon the tube, its preſſure on the water is diminiſhed, while that, continuing its force, impels up the oil, and intrudes it into the pipe; which being reſtored to a perpendicular poſition, the oil will again depreſs, and drive the water out of its cavity. We farther cauſed three pipes to be *Fig.* 14. blown differently from the former, and filled their winding and irregular parts, as alſo their uniform ſtems, to a proper height, with oil of turpentine; we then plung'd them to a due depth under water, and there unſtopping their upper ends, the ſurface of the oil they contain'd appear'd at the ſame height above the water, it would have done had the tubes been ſtrait; as we found by a cylindrical one fill'd and immerſed as the reſt; tho' the quantity of oil in one of theſe pipes were perhaps thrice as great as that in the ſtrait one. Hence we may fairly conclude, that the preſſure of fluids contain'd in pipes, muſt be computed by their perpendicular height, how wide, long, or irregular ſoever they are.

SCHOLIUM.

The learned *Stevinus* has a corollary from the preceding propoſition, to this purpoſe. If thro' the upper ſurface of a concave cylinder, an open cylindrical tube be erected perpendicularly, and the whole united cavity of both fill'd with water, the bottom circle will ſuſtain a preſſure equal to that of a cylinder of water whoſe baſis is that of the veſſel, and height that of

Fig. 15.

the tube therefrom. The truth of this paradox we attempted to try in the following manner. Having provided a latton veſſel A B E H G F, furniſh'd with a falſe wooden bottom C D, which was cover'd with a fine bladder, and oiled about the lower edges, to facilitate its riſing from the rim of wood H G, that lay every where contiguous to the inſide of the metal, and kept the water from paſſing between; to the middle of this looſe bottom we alſo faſten'd a ſtrong ſtring, which came out at the orifice A. Thro' this orifice we then pour'd in water, which preſſing the falſe bottom C D, help'd to tighten the veſſel, and keep its parts cloſe. When the whole cavity was fill'd, we faſten'd the upper end of the ſtring A, to the beam of a balance, and gradually placed weights in the oppoſite ſcale, till they elevated the falſe bottom C D from the rim H G, and conſequently lifted the incumbent water, which ſoon ran down between them. Now we had before-hand found what weight ſufficed to raiſe the bottom C D alone; deducting that, therefore, from the weight in the ſcale, and comparing the remainder with the weight of as much water as the ſhallow cylinder B E C H G D F would alone, without taking notice of that in the pipe A B, contain; we found the preſſure upon C D ſo vaſtly greater than could be attributed to the whole quantity of water made uſe of, had it been contain'd in an uniform cylinder of the ſame baſis with our inſtrument, that we thought it ſome ſmall confirmation of *Stevinus*'s doctrine; tho' the paradox itſelf be greatly queſtion'd by ſome men of learning.*

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* 'Tis now an eſtabliſh'd propoſition, that fluids preſs not according to their quantity, but perpendicular altitude; and may be demonſtrated thus.

Fig. 16.

Let ABCDFE be a veſſel full of water. Now the column B F being heavier than the column H G, 'tis plain that if the veſſel were open at H, the column G H would aſcend till it balanced the column B F; but ſince it cannot aſcend at H, the water there muſt be preſs'd back by the obſtacle, with a force equal to the weight of B L, for action and re-action are equal; and all preſſure here being reciprocal, the water at G will preſs againſt the bottom of the veſſel with the ſame force. Now as the weight of the column G H, is added to the former, the force of the water's preſſure at G will be the ſame as if the column G H were of equal height with the column F B, that is, as if it reach'd to M. And the ſame reaſoning is applicable to all the other columns in the veſſel; conſequently the bottom of it, E D, will ſuſtain the ſame preſſure as if the veſſel were fill'd up to N O.

This ſurprizing property in fluids ſeems applicable to conſiderable purpoſes, as it ſhews us how an exceeding ſmall quantity of matter may be made to ſupply the place of one infinitely larger.

Fig. 17.

Thus, for inſtance, if the tube A B, of the inverted ſyphon A B C D, were cloſe ſhut, and the ſmall pipe C D, fill'd with water to D, that little quantity C D would exert ſuch a force upon the larger A B C, that if the cover of the great orifice were perforated, the water would be made to fly out thereat, as violently, as if part of the little tube C D contain'd as much water as the great one A B.

Again, if two cylindrical emboli were fitted to the tubes A B, and C D; the weights laid upon them would be balanced, provided they were proportionable to the capacity of the tubes; that is, ſuppoſe the capacity of the tube A B was four times greater than that of C D, one pound laid upon the little embolus, would balance the force of four pounds upon the greater. And theſe experiments might be varied infinite ways. See *Clark. Annot. in Rohault. p.* 41.

PARADOX VII.

A body, immerſed in a fluid, ſuſtains a lateral preſſure therefrom; which increaſes with the depth whereto 'tis plunged.

Bend a ſmall glaſs tube at right angles, into a longer and a ſhorter part; *Fig.* 18. then, by ſuction, draw up oil of turpentine enough to fill the ſhorter, and to riſe two or three inches high in the longer; nimbly ſtop the orifice, and immerſe the lower end of the tube under water; ſo that the longer leg, EF, may ſtand perpendicularly to the ſurface AB, and the ſurface of the oil IK, but a little above the ſame AB; when, if the tube be unſtopp'd at E, the oil will continue as it was, and, conſequently, there's a lateral preſſure of the water againſt the oil contiguous to G; for, nothing elſe could hinder the perpendicular preſſure of the oil in the longer leg, from forcing it out; as will farther appear, by gently raiſing the tube, in the ſame poſture toward the top of the water; for, as the lower leg aſcends, the oil will be driven out thereat, by the preſſure in the other. And that the lateral preſſure of the water againſt the lower orifice is, before the tube be raiſed, nearly the ſame with the perpendicular preſſure of a cylinder of water, reaching from the orifice G, to the top of the water; appears from the ſurface of the oil in the longer leg, always continuing a little above that of the water; as would happen, were a pipe of an equal bore, continued from the orifice G, to the top of the water at H. But, farther, if the ſyphon be plunged deeper into the water; the oil, by the lateral preſſure thereof, will be gradually driven quite out of the ſhorter leg into the longer; and if you immerſe it ſtill deeper, a cylinder of water will riſe in the ſame longer leg, and ſuſtain that of the oil, which is now no longer able to balance the lateral preſſure of the water at ſo great a depth; whence we find, that water has alſo a lateral preſſure againſt water, which increaſes proportionably to the depth; for the external fluid, could not, otherwiſe, impel that of the ſhorter leg into the longer, which it does by greatly exceeding the reſiſtance of the whole cylinder of oil therein. But if the tube be now gently raiſed again, and the lateral preſſure of the water againſt the immerſed end thereby diminiſhed, the oil will force the water firſt out of the longer leg, and then out of the ſhorter, till, at length, it leaves the orifice G, and emerges in drops to the ſurface. Again, when the oil in the tube is a balance for the external water; if its ſhorter leg be kept parallel to the ſurface thereof, and moving backwards and forwards any way at the ſame depth therein, the oil in the longer leg will appear at the ſame height; from whence 'tis plain, a pillar of water, with a baſis equal to the ſide of the body immerſed, may be any where ſupported in the containing veſſel; and that tho' this imaginary column, as GH, be not included in any firm ſurface, its lower parts have, by means of the incumbent fluid, a lateral preſſure, tending outwards againſt its imaginary ſides; and laſtly, that this preſſure increaſes in proportion to the height of the column of water above them. To conclude, if inſtead of holding the longer leg of our tube in a perpendicular poſture, it be variouſly inclined to different angles with the ſurface of the water A B, ſo that the orifice of the ſhorter leg F G, may ſometimes tend towards the top, and, at others, towards the bottom of the glaſs; the oblique preſſure of the water depends ſo entirely upon its height above the orifice G, that the oil in the longer leg will ſtill continue with its ſurface I K, but little higher than that of A B.

SCHOLIA.

Fig. 19. 1. We once attempted to diſcover the quantity of this lateral preſſure by the following experiment. Having obtain'd a glaſs bubble X, with a ſlender neck, whoſe gravity fell very little ſhort of that of water; and provided a large wide-mouth'd glaſs, exactly fitted with a cork, perforated in the middle, to receive, perpendicularly, a long, ſlender, glaſs pipe, that deſcended conſiderably below the cork, wherein, alſo, was another hole, near its edge, for a ſecond ſmall pipe to paſs thro', whoſe upper end reaching only two or three inches into the air, was firmly cloſed; then the veſſel being filled with water, and the bubble ſuffer'd to float thereon, the cork was applied, and well cemented to the mouth of the glaſs, ſo that nothing could paſs in or out of the veſſel, except thro' the long ſlender pipe; all the other vents being carefully ſtopped with cement. When things were thus prepared, and the bubble was made to float at a conſiderable diſtance from the deeply immerſed end of the long tube, we poured water thro' its upper orifice E, which, for want of other vent, aſcended back again into the ſame cavity; and if the water therein was only permitted to riſe as high as I, for inſtance, the bubble X, would ſtill continue a-float: but if the water roſe to K, the bubble preſently fell to the bottom of the veſſel, and remain'd thereat while the water ſtood to the ſame height in the pipe E F; but if the height were diminiſh'd, by pouring ſome of the water off, the bubble would re-aſcend. Hence we ſee, that the upper parts of water gravitate on the lower; and that, in a full veſſel, the ſame thing happens, tho' the former be not directly incumbent on the latter, but at a conſiderable diſtance from the line of their direction; for the additional cylinder of water K I, acting like any other heavy body upon the water, cauſes the bubble X, to ſubſide. And, ſince the air included in this bubble, was remarkably condenſed, it follows, that the cylinder of water K I, preſſed upon the ſubjacent fluid in the veſſel; for otherwiſe the air in the bubble could not have been compreſs'd: and as this bubble floated on one ſide, at a tolerable diſtance from the pipe E F, and above its lower orifice F; 'tis evident, that the cylinder of water I K, preſſes as well laterally, as directly, upon all the bodies that lie beneath its ſurface. And thus every aſſignable part of the ſides of the veſſel, ſuſtains a preſſure proportionable to its depth under water, and its own magnitude: if, therefore, any part were ſo weak, as to yield to the weight of the cylinder I K (due allowance being made for the obliquity of the preſſure) 'twere unfit to enter the compoſition of our glaſs. Even the cork itſelf, reſiſts a preſſure proportionable to the breadth of its under ſurface, and the height of the cylinder K I; and might, therefore, if not too ſtrongly faſten'd, be raiſed thereby; ſo that if the cement ſhould fail, the water would forcibly iſſue out of its vents. Thus, alſo, the ſtopple G, of the ſhort pipe G H, may, by this means, be violently thruſt out of its place, as will more ſatisfactorily appear, by ſtopping the orifice at G, with your finger, which will there be ſtrongly preſs'd upwards.

2. To ſhew that the very oblique preſſure ſuſtain'd by the bubble, from the water in the long tube, differ'd not conſiderably from that of water plac'd directly over it, I ſet two ſuch bubbles ſeparately a-float in long cylindrical glaſſes of water, when, if they were plunged but to a ſmall depth, they would riſe again to the top; ſo that I was obliged to immerſe them to a conſiderable depth, before the preſſure of the incumbent water would ſink them.

3. We may here alſo obſerve, how the preſſures of ſuch different fluids, as air and water, will communicate. For, having ſometimes left a ſmall part of our veſſel A B C D, unfill'd with water, ſo that air remain'd between the ſurface L M, and the cork; the preſſure of the water in the pipe from I to K, would, if the cork were tight, cauſe the bubble to ſink, as if the veſſel had been wholly fill'd with water; the compreſs'd and impriſon'd air above L M, communicating the preſſure it receiv'd to the contiguous water.

PARADOX VIII.

Water will as well depreſs, as ſupport a body ſpecifically lighter than itſelf.

We have proved that the upper parts of fluids gravitate upon the lower, or bodies immerſed therein. Now, when a body is unequally preſſed, it muſt neceſſarily move from that place where the preſſure is greateſt, to where it is leaſt; whence, if a quantity of oil, hath its upper ſurface alone, expoſed to the preſſure of water, and nothing be in the way to hinder its deſcent, the oil muſt neceſſarily be depreſs'd, and not buoy'd up thereby. To illuſtrate this by an experiment; take a ſmall glaſs ſyphon E F G H, *Fig.* 20. with its ſhorter leg G H, 3 or 4 inches long, and parallel to the other E F; plunge the ſhorter into oil of turpentine, till it be quite fill'd, and the fluid reaches to an equal height in the longer, as from F to I; then ſtopping, with your finger, the orifice E, of the longer leg, immerſe them both a little under water: remove your finger, and as you gradually plunge the ſyphon deeper, the oil, in its ſhorter leg, will ſink, and, at length, wholly riſe into the longer; which ſufficiently proves and illuſtrates our propoſition.

SCHOLIUM.

At once to confirm the preſent paradox, the ſeventh, and the ſecond, I took a ſlender glaſs pipe O P N M L, and ſuck'd oil of turpentine into *Fig.* 21.

its lower orifice L, till it reach'd to P, in the longer leg, in a level with L; and, ſtopping the upper end O, with my finger, I plunged the tube to a convenient depth under water; when, removing my finger, the external fluid firſt drove away the oil in L M, then depreſſed it from M to N; and laſtly, wholly impelled it into the longer leg N P O; ſo that the oil was, here, evidently preſſed not only laterally, but alſo downwards, and upwards.

PARADOX IX.

Notwithſtanding the doctrine of poſitive levity, an oil, lighter than water, may be kept immerſed in that fluid.

Fig. 22.

Since the ſurface of ſtagnant water is, phyſically ſpeaking, horizontal; that part which preſſes againſt the lower ſuperficies of an immerſed body, muſt be deeper than that which preſſes upon the upper: if, therefore, the water incumbent on the upper part of a body, can be made to ſtand at a greater height than the reſt of the fluid wherein 'tis immerſed, ſo as to balance what preſſes againſt the lower; oil may be ſuſpended betwixt two parcels of water. To reduce this theory to practice, I ſuck'd water into a ſtrait, ſlender glaſs pipe T P, to the height of an inch, and there held it ſuſpended, by ſtopping the upper orifice with my finger; then plunging the lower end of the tube, two inches below the ſurface of ſome oil of turpentine, deeply tinged with copper, whereby the oil, driving up the water, aſcended to the height of an inch in the lower end of the pipe; when, I again cloſed its upper end, to keep both liquors ſuſpended. In the next place, I plunged the tube into a glaſs of water, three or four inches below its ſurface; ſo that the water, upon the removal of my finger, was preſſed into the lower end of the pipe P, to Q, impelling up the oil Q R, to R, and the incumbent water to T S; at which ſtations, the three ſeveral parts of oil and water together, preſs upon P, as much as the external water upon the other parts of the ſame plain G H; yet the oil Q R, aſcends not, becauſe the water R S, is kept from deſcending below it, by the ſides of the tube; whilſt its upper ſurface T S, ſtands higher than A D, that of the reſt of the water: by which means, the incumbent fluid may be made to preſs upon the upper ſurface R, of the cylinder of oil, as forcibly as the water preſſes againſt Q, the lower ſurface of the ſame; whence the oil will remain ſuſpended.

PARADOX X.

The aſcent and flux of water in ſyphons, are explicable, without ſuppoſing a Fuga vacui.

Fig. 23.

Provide a cylindrical glaſs A B C D, both wide and deep, as alſo, a ſyphon FKG, with unequal legs, communicating with a long cylindrical pipe K E; to each leg of this ſyphon, fix a cylindrical glaſs I, and H, filled with water, as in the ſcheme, that the water may run from the higher glaſs I, into the lower H; then, while the pipe EK, is held ſtill, pour oil of turpentine, gently into the cylinder ABCD, till it riſe above the top of the ſyphon FKG; when the oil preſſing on the water, will raiſe it into the legs of the ſyphon, and force it out of the veſſel I, into H: in which caſe, 'tis manifeſt, the water runs thro' the ſyphon, whilſt the air has free admiſſion thereinto, at the orifice E; ſo that nature's abhorrence of a vacuum, cannot be the cauſe of this flux. A juſt ſolution of the phenomenon may be had from conſidering the preſſure of the oil, and the ſituation of the veſſels; for, that fluid aſcending higher than K, preſſes upon the ſurface of the external water, in the glaſſes I, and H, wherein the legs of the ſyphon are plunged, and forces it up into them; but as the exceſs of water in the glaſs I, proves an over-balance for the greater depth of oil in the glaſs H, the water is obliged to run from the ſhorter leg into the longer. Thus it happens in ſyphons; when once the water is brought to run thro' them, the air gravitates upon the ſurface of the external fluid, wherein their legs are plunged, and not upon that which they contain; and, therefore, tho' the incumbent air, ſtands a little higher over the fluid in the lower veſſel, than in the upper, yet the water in the longer leg, greatly over-balancing that in the ſhorter, it muſt needs flow out at the latter. And the preſſure of the external air being able to raiſe water to a much greater height than that of the ſhorter leg, the efflux will continue till ſome other cauſe put a ſtop thereto. But if the legs of a ſyphon exceed 35 feet in perpendicular altitude, the water will not flow thro' them; becauſe the preſſure of the external air is unable to raiſe water to that height. So, likewiſe, were an hole made in the top of a ſyphon, whilſt water was flowing thro' it, the flux would preſently ceaſe; for the air, then preſſing, as well within as without the ſyphon, would return the water in each leg to its proper veſſel. To confirm this particular, I procured two ſyphons, the one of tin, the other of glaſs, each of which had in the upper part of its bend, a ſmall perforation, which I could ſtop with my finger, or open at pleaſure; wherein I frequently perform'd the experiment. And having once carefully ſtopp'd one of the above-mention'd holes, with oiled paper and cement, and found the water to run freely thro' the ſyphon; yet a puncture made in the paper, by the point of a needle, admitted air ſufficient to make the water in the legs fall back, before I could poſſibly ſtop the needle quick enough into the hole to prevent it.

PARADOX XI.

The moſt ponderous ſolid body we know, immerſed in water to a depth exceeding that of twenty times its own thickneſs, will float, if it be there fenced from the direct preſſure of the incumbent fluid.

Let a deep glaſs veſſel ABCD, be almoſt fill'd with water; wherein if a ſmall cylinder of braſs EF, be immerſed, till its upper ſurface is juſt cover'd, and then let go; it muſt, by reaſon of its greater ſpecific gravity, neceſſa- *Fig. 24* neceſſarily fall to the bottom. But were it placed on the plain I K, which lies at above the depth of nine times the body's thickneſs below the upper ſurface of the fluid; I aſſert that it would not deſcend, unleſs it were preſſed downwards by the weight of the incumbent fluid. For braſs being nearly nine times ſpecifically heavier than water, the body E F alone would preſs upon the part F as much as a cylinder of water of an equal baſis, but about nine times the height of the braſs; and conſequently the part F will be no more depreſſed than any other part of the plain I K. But ſince water gravitates on water, and alſo upon bodies heavier *in ſpecie* than itſelf, if immerſed therein; the braſs cylinder E F, wherever it reſts in the fluid, muſt be forced downwards, both by its own weight, and that of the incumbent water. But were the incumbent water prevented from gravitating thereon, it follows, that the body would, at that depth, be kept ſuſpended. And in caſe the body reſted, in like manner, upon the plain L M; the much greater preſſure of the water on the other parts of the ſame ſurface would impel it upwards, with a force proportionable to the difference of their reſpective weights.

And having found, by exact trials, that the pureſt gold, the moſt ponderous body we know, is not quite twenty times ſo heavy as an equal bulk of water; if this metal, alſo, were immerſed into a greater depth than that of twenty times its own thickneſs, and there fenced from the preſſure of the incumbent water, it muſt neceſſarily float. To confirm this by an experiment. Suppoſe the braſs body E F to be the cover of a braſs valve, and the valve itſelf ſtrongly cemented to an open glaſs pipe O P; when the body E F, being the lower part of the inſtrument, and every way detached therefrom, would fall by its own weight, if unſupported. Then to the button Q faſten a thread, which, by being pulled upwards, may cauſe E F to cloſe the orifice of the valve: the valve being now plunged under water, to the depth of a foot, the cement, and the ſides of the pipe O P, will prevent the fluid from preſſing upon the upper part of the body E F, and conſequently that part of the plain V W contiguous to its ſurface, will be preſs'd upon only by the weight of the body E F, but in its other parts by that of the incumbent water; ſo that, letting go the ſtring which held the body E F cloſe to the inſtrument, that body will remain ſuſpended by the bare preſſure of the water beneath it. And that this alone is the cauſe of the phenomenon, appears by gently raiſing the inſtrument towards the top of the water; for when it has aſcended near the ſurface, ſuppoſe between the plains I K, and X Y, the body E F will fall down, and the water riſe in the pipe O P, to a level with that in the containing veſſel. But if the valve be at firſt plunged much deeper under water before the ſtring be relaxed, for inſtance, to the plain S R, the valve E F will require a conſiderable weight, as L, beſides its own, to disjoin it: but if the weight prove inſufficient for the purpoſe, it may ſoon be render'd effectual, by raiſing the inſtrument, far leſs than before, towards the top of the water.

Fig. 25.

Plate II.

Fig. 12. 304.

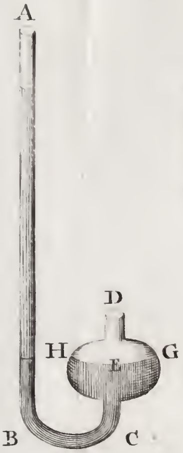


Fig. 13. p. 304.

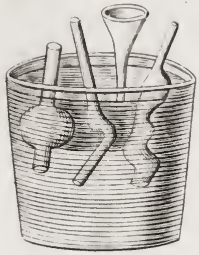


Fig. 14. p. 305.

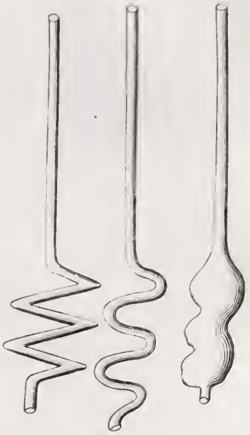


Fig. 15. p. 306.

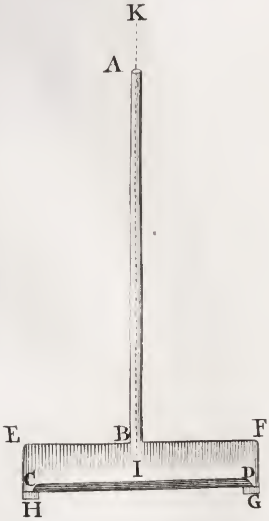


Fig. 16. p. 306.

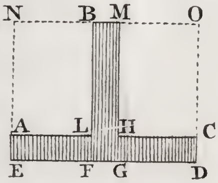


Fig. 17. p. 306.

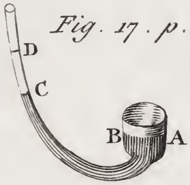


Fig. 21. p. 310.

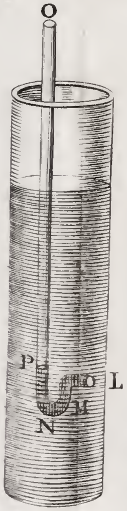


Fig. 18. p. 307.

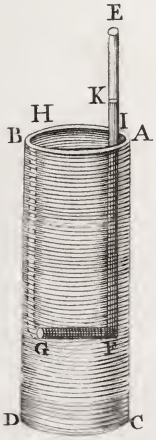


Fig. 19. p. 308.

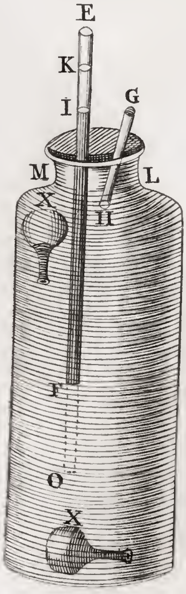
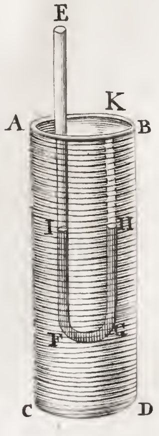


Fig. 20. p. 309.



J. Mynde sc.

COROLLARY.

This experiment renders it unneceſſary to fly to a *Fuga vacui* for a reaſon why two flat poliſh'd marbles, when they adhere, require a conſiderable force to disjoin them; they being preſſed together by air of an immenſe height, after a like manner with our valve, and the inſtrument, here, by water; which requires the great weight L to over-balance it, ſo as to force them aſunder.

HYDROSTATICS

Applied to

ORES,

And to the

MATERIA MEDICA.

SECT. I.

Foſſils, their medicinal virtue, whence?

AS the virtues of gems proceed, chiefly, from the metalline and mineral ſubſtances, that, whilſt their original matter remain'd either fluid or ſoft, are incorporated therewith, and afterwards hardned into ſtones; ſo it ſeems very probable, that ſeveral boles, clays, earths, and eſpecially minerals, *&c.* may be endow'd with conſiderable medicinal virtues, and, perhaps, greater than thoſe of the finer gems; becauſe herein are often found, more of the metallic and mineral parts, which, whilſt *in ſolutis principiis*, might very plentifully inſinuate themſelves into the more open bodies, and there ſit looſer than in diamonds, rubies, ſapphires, *&c.* which *aqua fortis*, itſelf, is unable to penetrate and diſſolve.

A new way of examining them, hydroſtatically.

I, therefore, thought it might be of uſe to phyſicians, mineraliſts, and miners, to advance a new way of examining foſſils; for tho' the method itſelf pretends not, directly, to diſcover more than one quality of the body it examines; yet that quality, being its ſpecific gravity, is ſo conſiderable, that it may lead one farther, than might at firſt be imagined.

Its foundation.

Reflecting that rock-cryſtal is of the moſt pure, and homogeneous kind of ſtones; I pitched upon this for the ſtandard, whereby to adjuſt and aſcertain the purity and ſimplicity of other mineral ſubſtances.

By carefully weighing ſeveral pieces of native cryſtal, clear and colourleſs, firſt in air, and then in water, we found its proportion to clear water, of the ſame bulk, as 2½ to 1, nearly. Thus, if a hollow, metalline cube be exactly fill'd by one ounce of water; and afterwards, again, as exactly fill'd by a cubical piece of rock-cryſtal, that bulk of the ſtone will weigh about two ounces and a half. Some of my trials, indeed, made with tender balances, repreſented the proportion of theſe two bodies, with a very inconſiderable variation; but 'tis probable, that different pieces of cryſtal, tho' of equal bulk, may not have preciſely the ſame weight: however, the difference is ſo ſmall, that it may here be ſafely neglected.

And to prevent any ſcruple about the origin and nature of cryſtal, I ſhall add, that I procured ſome ſtrong iſicles, that had been faſten'd to vaults, *&c.* as bodies that would be acknowledg'd true ſtones, and yet to have once been in a liquid form: and having hydroſtatically examin'd theſe concretions, their ſpecific gravity appear'd to be little differing from that of cryſtal.

To apply this fundamental obſervation to the preſent occaſion; when I would, with probability, diſcover whether the mere ſtony matter of a mineral body were mix'd with ſome adventitious ſubſtance of a metalline nature, or of ſome other mineral more ponderous than cryſtal; I carefully weigh'd it, as we ſaid, in air, and then in water: when, if its proportion to water of the ſame bulk, exceeded that of 2 ½ to 1, or 5 to 2, I conclude it probable, that the concrete had in it a portion of foreign matter ſpecifically heavier than cryſtal, proportionably to the exceſs of the weight of the ſolid body above that of an equal bulk of water. To illuſtrate this procedure by example.

Examples to illuſtrate the doctrine.

The magnet, upon account of its great hardneſs, is uſually reckon'd among ſtones; but having obſerv'd a particular kind thereof, to be apparently more ponderous than common ſtone, of equal bulk, we weigh'd them in air and water; and found the ſpecific gravity of ſome of them ſo far to exceed that of cryſtal, or marble, that we concluded they contain'd a conſiderable quantity of metalline matter, which, by collateral experiments appear'd to be of a ferruginous nature.

Emeri is commonly reputed a mere ſtone; but finding its weight in water conſiderably to exceed that of an equal bulk of cryſtal; for it was to that liquor nearly as 4 to 1; I conjectured that it contain'd a metalline ſubſtance; as afterwards, by proper trials, I found it did.

By the weight, alſo, of *Lapis Hæmatites* on my hand, I conjectured, it largely partook of a metalline ingredient; and preſently I diſcover'd that iron or ſteel was contain'd therein. And, in general, I have frequently ſuſpected different bodies to be of a metalline or mineral nature, and was ſeldom miſtaken in my conjecture; tho' they have commonly been thought to contain no ſuch ſubſtance. The ſame thing I likewiſe found true, even in granats and *American* talc, from which I have extracted metalline parts.

Preliminary obſervations, with regard to foſſils.

But we muſt here ſet down ſome preliminary obſervations. And firſt, if a propoſed foſſil be much lighter than an equal bulk of cryſtal, it is almoſt certain it cannot be a metalline ore; and this negative proof is here uſually ſtronger than the affirmative kind. Thus, for inſtance, when I find that jet, tho' a foſſil, has a far leſs ſpecific gravity than cryſtal, I conclude it to be no metalline body. The like determination I make as to foſſil-amber, *Sulphur vivum*, common ſulphur, *Engliſh* and *Venetian* talc, and ſome other firm concretions, which are dug out of the earth: among which, I alſo rank black-lead. For having found its weight to that of water as 1.86 to 8, I afterwards diſcovered it to be not lead-ore, but a particular kind of mineral, and near allied to a ſort of talc that I have met with.

2. We muſt diſtinguiſh between the ſeveral uſes to be made of foſſils by men of different profeſſions. So that if a foſſil be found very little heavier *in ſpecie* than cryſtal or marble, it may, poſſibly, have a metalline or mineral portion, which, however ſmall in quantity, will perhaps entitle it to the eſteem of a jeweller, phyſician, or chymiſt. But if this exceſs of its ſpecific gravity be inconſiderable, the foſſil itſelf will prove of little price to a mineraliſt; for if it be poor in metalline ſubſtance, it may chance to be hardly worth the working.

3. But tho' the great ponderoſity of a foſſil generally proceeds from a proper metalline ſubſtance embody'd with the other parts thereof, which ſhews it not to be a mere ſtone; yet this alone is not a ſure ſign that the mineral part is properly metalline; and, therefore, where any doubt ariſes, we ſhould have recourſe to collateral ſigns. For, beſides metalline ores, properly ſo call'd, there are other foſſils, which, tho' of affinity to metals, may be diſtinguiſh'd from true metalline ores; ſuch, for inſtance, are antimony, biſmuth, *Lapis Calaminaris*, and marcaſites. But there will not, perhaps, occur many caſes, wherein we need to have recourſe to collateral ſigns, to ſhew whether the mineral part of a foſſil be, in a ſtrict ſenſe, of a metalline nature, or not. For ſuch ſemi-metals are moſt commonly found either in veins, maſſes, or great lumps of their reſpective kinds; and eaſily diſcover to what ſpecies they belong. I have, indeed, receiv'd a lump of matter from *Devonſhire*, wherein I found ſome antimony mix'd with lead, which was the predominant body. But ſuch mixtures occur ſo ſeldom in *England*, that our way of eſtimating ponderous foſſils, will, nevertheleſs, be uſeful on moſt occaſions.

4. Two different eſtimates may be made of the ſpecific gravity of ores; one, when the body propoſed is weigh'd in its natural ſtate, that is, as it comes out of the earth, accompanied with the ſpar, or other heterogeneous matter, that firmly adheres to it; and the other, after it has been beaten ſmall, and ſeparated from heterogeneous ſubſtances by the help of water, after being ſkilfully agitated, wherein there is a remarkable difference in weight between theſe and the genuine parts of the ore; which being thus ſever'd from the reſt, are call'd waſhed. 'Tis ſometimes, alſo, very proper to prepare the ore by roaſting it once or more, or by keeping it for ſeveral hours in a competently ſtrong fire, as is uſually practiſed in preparing copper-ore; eſpecially if it be ſtubborn. I purpoſely mention theſe

two ſtates, wherein the weight of an ore may be taken, becauſe I have obſerv'd, that in ſeveral caſes, 'tis of great importance to diſtinguiſh them carefully. For ſeveral ores, which, in their natural ſtate, have too little ſpecific gravity, to make them judg'd worth the charge of being wrought, may yet, after they are prepar'd by water and fire, afford a metalline portion, ſo ponderous, as to allow one to ſuſpect they contain either ſilver or gold. I remember, that a piece of lead-ore, brought from *Ireland*, ſeem'd to me ſo light in the lump, that I thought it deſerv'd not to be wrought for lead; yet, afterward, upon trial, it appear'd ſo well ſtor'd with particles of ſilver, that I encourag'd the owner of the mine to work it.

To diſtinguiſh marcaſites from metalline ores.

5. There is one kind of mineral that I have obſerv'd to impoſe upon men ſo frequently, that I ſhall take a particular notice of it. For, not to mention examples from travels and voyages, I have myſelf met with numbers who had great expectations from marcaſites. And I have receiv'd not only from places near home, but even from the *Indies*, foſſils, whereof my opinion was deſir'd; which I found to be only marcaſites. Many of theſe foſſils have two qualities, that make them fit to delude the vulgar; for, they ſhew a multitude of ſhining ſtreaks, reſembling gold; or have, at the ſame time, a weight equal to that of true metalline ores. Marcaſites, then, being thus adapted to impoſe upon the unwary, I have had much ado to undeceive ſome perſons, as to the pleaſing confidence they had entertain'd, that theſe promiſing foſſils were lumps of rich gold, or ſilver-ore. Wherefore, ſince their weight, the criterion of minerals, is one of the two things that delude ſo many, I ſhall give a few inſtances of the ſpecific gravity of marcaſites, to make it appear, that ſome of them are, bulk for bulk, far more ponderous than true metalline ores. And, indeed, the great ponderoſity of foſſils, has ſeveral times occaſioned me to determine, before trial, that they were marcaſites. But, to prevent being impos'd on, by this, or the like ſpecious ſubſtance, it is but placing it in a ſtrong fire, and blowing now and then with a pair of bellows; for by this means the ſulphur wherewith marcaſites abound, will take fire, and burn with a flame for the moſt part blue, like that of common ſulphur: ſo that once, by diſtillation, in a cloſe veſſel, I obtain'd four ounces of good brimſtone from three pounds of theſe ſtones. And, if when this foſſil ceaſes to flame and ſmoke, it be taken out of the fire, and ſuffer'd to cool, it will be depriv'd of all its gaudy appearance, and turn'd to a brittle, blackiſh ſubſtance, very different from that of a proper metalline ore. Marcaſites, however, may be look'd upon as a kind of metalline bodies; for I have found ſeveral of them to contain, not only particles of copper, but alſo of iron, or ſteel; for, after calcination, applying the pulveriz'd remains, to a vigorous load-ſtone, great multitudes of chalybeate corpuſcles quickly adhered thereto. And I remember in a catalogue of the foſſils of *Miſnia*, publiſhed by *Kentmannus* under the head of *Pyrites*, he places ſeveral marcaſites; ſome whereof contain'd copper, others ſilver, others gold, and others, again, both ſilver and gold. Having preſented a very fine marca-

ſite to the overſeer of one of the emperor's beſt mines, he preſently examin'd it, by a peculiar way, with hopes to find in it ſome gold or ſilver; but, inſtead thereof, obtain'd a parcel of running mercury, which he preſented me. Notwithſtanding the caution here given, I do not deny but that 'tis poſſible for a skilful artiſt to make a profitable uſe of marcaſites, either by fixing the volatile gold, or ſilver, to be found in ſome of them, by graduating ſilver by their means, *&c.* But that for which I much more value them, is, what I deſire ſhould be well remark'd, that I am perſuaded, if they were ſimply, and, of themſelves, but dextrouſly handled, they may afford very noble and uncommon medicines, particularly excellent in continual fevers, tho' their uſual operation be almoſt inſenſible, unleſs by their good effects.

Directions to find the beſt flux-powders, for foſſils.

6. I muſt not here omit, that tho' many, who make trials of ores, value their own flux-powders, or ſuch as are cry'd up by others; yet they commonly ſeem to expect nothing from thoſe they prefer, more than that they ſhould beſt facilitate the fuſion of the ore, as that which being once done, the metalline part will ſeparate by its own weight, or, as it were, ſpontaneouſly. But yet, having purpoſely examin'd the matter more nicely, and compared the quantities of metal obtain'd from two portions, of equal weight, of the ſame ore, we found thoſe proportions conſiderably differ'd; tho' that which yielded leaſt metal, was flux'd down with a coſtly and well-adapted powder. And I doubt not but from other metalline ores a greater quantity of pure metal is obtainable, by ſome flux-powders, that are but little employ'd, or known, than by others that are much more famous and common. Thus two equal portions of the ſame lead-ore, clear of ſpar, being the one reduced with a due weight of nitre and tartar fulminated together, afforded me a much leſs proportion of malleable lead, than the other, by means of half, or a quarter the quantity of filings of iron. And, to inſtance in a much more precious mineral than lead-ore, I try'd the like with ſome ounces of good native cinnabar, finely pulveriz'd; by adding to one half a fix'd alkali of tartar, and to the other a different flux-powder, we obtain'd from the former twice as much mercury as we did from the latter, tho' diſtill'd with a fix'd alkali even of a mineral nature.

Directions relating to the management of the hydroſtatical balance.

7. Before we proceed farther, 'tis neceſſary the reader be acquainted with the method of weighing heavy bodies in water; which being liable to ſeveral contingencies, 'tis proper in this place to take notice of the more conſiderable ones.

Fig. 26.

But, firſt, to give a general notion of the thing, I deſire the figure of the hydroſtatical balance itſelf, with all its apparatus, may be view'd: wherein A A, is the beam of the balance, BB, the diſhes, CC, the frame whereon the beam is ſuſpended; with D, its ſliding ſocket; E, the arm; with F, a pulley; over which, and G, another pulley paſſes H, a line faſten'd to I, a moveable weight; by which the beam is elevated and depreſs'd. K, is a hair wherewith to ſuſpend L, the body to be weigh'd in the water of M, the glaſs ciſtern. N, is the bucket for liquors, O, the box of grains, P, the forceps, wherewith to manage them, Q, the pile of weights, R, the handle of the balance, and SSSS, the table.

The ſolid body to be examin'd ſhould be ty'd about with an horſe-hair, of a competent length, and faſten'd at its other end to one of the ſcales of an exact and tender balance; that the body, having been carefully weigh'd in the air, may be immerſed in a proper veſſel, almoſt full of fair water, ſo as to hang freely therein, and be on every ſide encompaſs'd with the fluid. This done, put weights into the oppoſite ſcale, to balance the body hanging in the water; when the beams of the ſcale will lie horizontal, and the weights employ'd give that of the body in water. Deducting therefore this weight from the weight of the ſame body in the air, by the remainder, that is, the difference of the two, divide the whole weight of the given body in air, and the quotient will ſhew the proportion in ſpecific gravity between the ſolid and an equal bulk of water. To illuſtrate this by an example, we put a fine piece of white marble, which ſeems the moſt pure of any common opake ſtone, into an exact balance, and took its weight in the air 1169 grains; then a horſe-hair being ty'd about it, and the other end of the ſame hair, faſten'd to one of the ſcales; under which, at a convenient diſtance, was placed a deep glaſs, almoſt full of fair water: we ſuffer'd the ſtone to hang freely beneath the ſurface of the fluid, and in the oppoſite ſcale placed weights enow to bring it to an equilibrium with the other. Theſe weights amounted to 738 grains, which gave us the weight of the marble in water; which being ſubſtracted from the weight of the ſame ſtone in air, there remain'd 431 grains for the weight of the equal bulk of water: by this remainder we divided the weight of the marble in air, *viz.* 1169, and found the quotient to be 2.7 nearly, for the ſpecific gravity of white marble to water.

'Tis here manifeſt, from the nature of the thing, that the body propos'd to be weigh'd, muſt be heavy enough to ſink in water; otherwiſe its weight in the fluid cannot be ſignificantly deducted from its weight in air: but if there be occaſion to weigh in water a body lighter *in ſpecie* than water, it may be done, tho' with difficulty, by joining to it another body that is able to ſink it.

8. A horſe-hair is made choice of for hydroſtatical operations, becauſe 'tis thought to be of the ſame ſpecific gravity with water; and tho' that be not ſtrictly true, yet a horſe-hair is fitter to be employ'd in theſe trials, than any other ſtring I know of; and its weight uſually differs ſo little from that of water, that the difference may be ſafely neglected: but if the ſolid propoſed be too heavy to be ſuſtain'd by a ſingle hair, ſeveral of them may be twiſted together.

9. I ſometimes had occaſion to examine bodies hydroſtatically, which, by reaſon of their roundneſs, or other inconvenient figure, could not well be faſten'd to a hair; in which caſe I uſed hairs interwoven into a kind of a ſmall hop-net, the meaſhes whereof would not ſuffer the body to ſlip thro' them.

10. And here let it be noted, once for all, that whenever any hydroſtatical trial is made, by means of a horſe-hair, there muſt be put into the oppoſite ſcale as much of the ſame hair as ſeems equal to what ſuſtains the body, and appears above the ſurface of the water; for that fluid only takes off the weight of as much of the hair as is immers'd in it: ſo that its unimmers'd part adds to the weight of the pendulous ſolid; and therefore ought to be compenſated by an equal weight in the other ſcale.

11. When I reſerv'd a balance for hydroſtatical trials, I found it convenient, on ſeveral occaſions, to take off one of the ſcales, with the ſtrings belonging to it, and ſubſtitute in its ſtead a piece of metal of a conical, or other convenient figure, and equal in weight to the oppoſite ſcale; as alſo, at the ſame end of the ſtring, to faſten one end of the horſe-hair that tied the body to be weigh'd in water. And ſometimes, likewiſe, when I did not take off one of the ſcales, I cauſed it to be perforated in the middle, without leſſening its weight, that ſo the body to be immers'd might hang perpendicularly from the centre of the ſcale.

12. Care muſt be taken that the body to be examin'd hang freely in the water, ſo that no part of it, any where, touch the bottom, or ſides of the veſſel, or reach above the upper ſurface of the water it contains; for when any of theſe circumſtances are neglected, the true weight of the ſolid is ſomewhat alter'd: and if any part of the body, or horſe-hair 'tis tied with, appears above the water, it adds to the weight thereof. And as nothing but the water is to touch the hanging body, ſo no part of the fluid muſt touch the ſcale from whence it hangs. I have ſeveral times obſerved, that immers'd bodies have been concluded to weigh more in water than they really do, from a neglect in obſerving, that if the ſtring be too ſhort, or the veſſel too full, the vibrating motion of the balance will, at one time or other, carry down the ſcale to which the ſuſpended body is tied, ſo as to make ſome part of it touch the ſurface of the water, a few drops whereof will readily ſtick to it; which, becauſe they adhere to its under part, may lie conceal'd from a heedleſs eye, ſenſibly add to the weight of the ſcale, and make the body be thought heavier than it naturally is: an error very prejudicial, when exact experiments are required.

13. But the moſt uſual cauſe of miſtakes in hydroſtatical trials, proceeds from hence, that the given ſolid, and the ſtring whereto 'tis faſten'd, carry down with them particles of air, which, perhaps, too, may extricate others that lay conceal'd in the pores of the liquor, in the form of bubbles. Theſe aerial particles faſten themſelves to the little aſperities they meet with on the ſurface of the immers'd body, and like ſo many little bladders of air, endeavour to buoy up the body they adhere to; and therefore, in proportion to their number and bulk, leſſen the due weight of the immers'd body, in water. Great care, therefore, muſt be had, in nice experiments, to ſhake the ſtring, and warily knock the body againſt the ſides of the glaſs, that the adhering bubbles may thereby be diſplac'd, and emerge to the top of the water: and, on ſome occaſions, this caution ſhould be uſed more than once in the ſame experiment; becauſe I have often ob-

ſerv'd, that after the immerſed body has been freed from the firſt bubbles that appear'd about it, others ſucceeded in their ſtead, before the experiment was finiſh'd.

I have been the more circumſtantial in explaining the way of weighing bodies in water, becauſe experience hath taught me, that the practice of it is not ſo eaſy as might, at firſt ſight, be expected.

Having obtain'd the weight of the given body, firſt in the air, and then in water, according to this method; it will not be difficult to diſcover, practically, the proportion in weight between the ſolid and the fluid. The foundation of our practice, is this theorem of *Archimedes*. "A body heavier than water, weighs leſs in that, than it does in air, by the weight of an equal bulk of water." Whence we deduce a rule for our preſent purpoſe. For if the weight of the propoſed body, whilſt it is every way ſurrounded with water, be ſubſtracted from that of the ſame body in air, the remainder gives the aerial weight of a quantity of water equal in magnitude to the ſolid; ſo that having the weight both of the ſolid and fluid, divide the greater by the leſs, and the quotient compared to an unit, will be the antecedent of the proportion ſought between the ſolid and the water. And this rule holds equally, *mutatis mutandis*, for other liquors, as well as water.

To apply this doctrine firſt to ores.

The hydroſtatical balance applied to ores, and firſt to gold-ore.

Many learned men have been of opinion, that, properly ſpeaking, there were no ſuch things as gold mines; and I confeſs myſelf to have been long kept in ſuſpence as to this particular: for, notwithſtanding all the inquiry I could make after mines, wherein gold was the predominant metal, I could not find a man who would ſay he had ſeen any. At length, I met with ſome ore, preſented to King *Charles* II. which I judged to be genuine; and alſo received from an unknown hand in the *Eaſt-Indies*, another piece, in the clefts whereof, and a little beyond them, there appear'd ſome lumps, wherein, by their colour and other ſigns, it ſeem'd manifeſt that gold was the predominant metal. But the largeſt piece, and that which was beſt furniſh'd with metalline parts, weighing about an ounce and a quarter, contain'd ſo great a proportion of ſpar, that its ſpecific gravity to water, was but as 2.21 to 1. Its metalline portion, however, ſeem'd to be all gold; for there appear'd no ſign of any other metal therein, nor, in ſome leſſer pieces that I receiv'd along with it. Its ſpar, or ſtony matter, wherein the true ore is immediately lodg'd, did not reſemble the ſpar of lead-ore, or that of any other of our *Engliſh* metals; but ſeem'd, at firſt view, to be a kind of white marble, with a daſh of yellow. And, upon trial, I found it differ'd ſtill more from the ſpar of lead-ore, which with us, is uſually white, and almoſt ſemi-diaphanous. For our ſpar of lead-ore is often ſo ſoft, that it may eaſily be cut with a knife; but the ſparry portion of this gold-ore was a ſolid ſtone; and ſo hard, that when ſtruck againſt a piece of ſteel, it would yield ſparks of fire. And, farther, the ſpar of lead-ore will eaſily, and ſuddenly, calcine to a kind of lime; but our golden-ſpar, tho' kept for ſome hours red-hot in a crucible, did not appear to be at all calcined.

cined. The ſpar of lead-ore will alſo diſſolve in ſome acid menſtrua, and even in diſtill'd vinegar: yet I could not find that this golden ſpar, when kept for ſeveral hours in ſpirit of ſalt, *Aqua fortis*, or *Aqua regia*, was manifeſtly wrought upon by any of them.

A piece of ſpar, that ſeem'd ſcarce to contain any gold at all, being hydroſtatically examin'd, we found its ſpecific gravity to water, as 2.65 to 1.

Theſe obſervations, imperfect as they are, may, perhaps, be ſerviceable to ſuch as have never ſeen true gold-ore; and, in particular, to thoſe who ſearch after gold-mines in *Jamaica*: where, as general *Venables*, the perſon who conquer'd it for the *Engliſh*, inform'd me, at his return from thence, the *Spaniſh* governor of the iſland, whilſt his priſoner, confeſs'd there was mineral-gold; tho' the *Spaniards*, for want of workmen, could not dig deep enough in queſt of it.

By what I have ſaid of the true gold-ore, I would not have any one diſcouraged from ſeeking for gold in other metals; becauſe, I know, it may ſometimes be found blended with predominant minerals, as appears from the copper-mines of *Cremnitz*, whence a conſiderable quantity of gold is yearly obtain'd. And I have ſeen an *Engliſh* tin-ore, wherein there lay, in little cells, a number of ſmall leaves or chips of gold; and tho' the tin-men, being unable to ſeparate them to profit, uſually melted both the metals together, and ſold the maſs for mere tin; yet the owner of the mine aſſured me, that one of his work-men advantagiouſly employ'd his own children to pick the gold out of the ore, when ſkilfully broken. There is alſo, a place in *Scotland*, where, over a lead-mine, near the ſurface of the ground, they often find large grains of native gold free from ſpar; ſome of which I thought worthy to be preſented to that curious examiner of ores, Prince *Rupert*; and I ſtill have a piece of native metal by me, that came from the ſame place, in weight above forty grains, and wherein gold is the predominant metal.

I have found a grain of natural *Scotch* gold, without any adhering ſpar, to weigh three drams, twenty-one grains; another grain of the ſame, that had a little ſpar ſticking to it, weigh'd three drams, three grains; ſo that, allowing for the heterogeneous ſubſtance, it weigh'd about three drams. And, laſtly, a grain of *Scotch* gold weigh'd, in air, 43 grains; in water, $39\frac{1}{2}$ grains; whence its proportion to water is as $12\frac{2}{5}$ to 1.

It often happens, that, among the leſſer grains of gold, properly call'd ſand-gold, there are pieces, ſingly big enough to be ty'd about with a horſe-hair, and weigh'd in water; to which, therefore, our hydroſtatical way of examining ores may be uſefully apply'd. For, ſince pure gold is to water of the ſame bulk, as 18 or 19 to 1; it will readily appear, whether the fragment propoſed be perfectly pure, or not.

We frequently receive from the maritime parts of *Africa*, ſmall fragments of gold, in the form of ſand or gravel, which ſeem to have been waſh'd away from hidden veins by the violence of the waters; to eſtimate the genuineneſs and degrees of purity whereof, our hydroſtatical method of examination may be of ſingular ſervice. For the ſpecific gravity of perfectly refined gold to water, being known; 'tis hence eaſy to examine its degrees of fineneſs. And, when once the true ſpecific gravity of a parcel of ſand-gold is known, together with its degree of fineneſs, gain'd by collateral trials; this ſpecific gravity may be uſed as a ſtandard, whereby to try the fineneſs of other parcels of the like native gold: whence the fraud of the *Negroes* may, in this caſe, be prevented; who often clandeſtinely mix with the right ſand-gold, filings of copper, or braſs; whoſe ſpecific gravity is not half ſo great as that of fine gold.

Aqua fortis, alſo, would immediately diſcover this fraud, which will not work upon gold; but corrodes braſs, and thereby gains a colour betwixt blue and green; tho' if much ſilver be naturally mix'd with the gold, this proof, by *Aqua fortis*, will require skill: and, therefore, good ſpirit of urine may be ſubſtituted in its ſtead, occaſionally; for this will readily work upon filings of copper or braſs in the gold, and gain from them a fine blue colour. And, to haſten the operation of this liquor on filings of braſs or copper; 'tis but ſpreading them thin, upon a piece of white paper, and moiſtening them throughly therewith, that the air may promote its action; for, by this means, I have often produced, in a few minutes time, a pleaſant blue colour upon the paper. Spirit of hart's-horn, or other volatile alkalies, or even ſtale rank urine, will ſerve in time of need, and yield the ſame phenomenon.

But if a ſolid ſubſtance be deſired for this purpoſe, common ſal armoniac may be uſed, by making a ſtrong ſolution thereof in water, and applying it as any of the former; for this alſo, will ſoon gain a greeniſh colour, or a blue, from braſs or copper, and without affecting the gold.

All minerals ſhould be carefully examined, and, if ponderous, hydroſtatically.

I have obſerv'd ſuch a variety of appearances and diſguizes in metalline bodies, and other minerals, that I would adviſe the ſearchers after mines to have their eyes always open, and ready to take notice of any unknown, or uncommon foſſil; and not neglect to weigh it in their hands; and if it ſeem to exceed the weight of cryſtal, or marble, to examine it hydroſtatically. For there are in *England*, and ſeveral other countries, uſeful foſſils, uſually overlook'd by the unskilful. I have found, in this kingdom, eagle-ſtones even upon the high-ways; and ſome other minerals that were not ſuſpected to be of *Engliſh* growth. And, I remember, that paſſing by a potter's work-houſe, and viewing the ground attentively, I made a diſcovery of manganeſe, a mineral very proper for glazing and colouring of earthen veſſels, which the potter, afterwards, gladly uſed for that purpoſe. The ſame perſon ſhew'd me a place that contain'd great ſtore of a foſſil ſubſtance, unknown in *England*, that ran very far under ground, like a vein of metalline ore; by ſome eaſy trials I found it abounded with vitriolate ſalt, much more than any marcaſite I had examin'd in the form of ſtones: whence I concluded, it might more profitably be employ'd to make vitriol, than the marcaſites uſed for that purpoſe in the works at *Deptford*, or elſewhere in *England*.

I remember, alſo, that a mineral of an odd appearance, being ſent me, as unknown to the miners who dug it up; I found it to be the ore of biſmuth, tho' the vein that afforded it was very ſmall. But the chief reaſon for which I give this particular caution, is, that an application of our general remark upon the ſpecific gravity of foſſils, may be extended to a new and conſiderable uſe; for we need not confine ourſelves to examine only thoſe foſſils, whereof we can obtain pieces ſufficiently large to be ſingly weigh'd in water; becauſe, not to mention the minerals that may be found uſeful to the phyſician, the drugſter, or the mineraliſt; the ores of metals may be often found diſguiz'd in the form of earth, or mud, that is eaſily dry'd; which foſſils, tho' unfit to be immediately ſuſpended by a horſe-hair, may conveniently be examin'd by means of a glaſs jar-bucket, of a known ſpecific gravity: for, this veſſel being almoſt fill'd therewith, and that matter, carefully counterpois'd in air, and made thoroughly wet with water, and the whole warily let down in the ſame fluid, and there kept ſuſpended by a horſe-hair, tied to a tender balance, the difference between the weight of the mineral, and veſſel in air and water, will be obtain'd; the weight, therefore, of the veſſel, in water, being ſubſtracted from that difference, will give the weight of the foſſil in water, and, conſequently, its ſpecific gravity in water.

How to examine earths, or ſoft ſubſtances, hydroſtatically.

And to ſhew that this method wants not proper ſubjects, whereto it may be apply'd; we are told, that one of the beſt ſorts of *Swediſh* iron is often found in the form of a red mud, at the bottom of lakes, or other ſtagnant water: and I have obſerv'd *Engliſh* okers to be richer in iron, than ſome ores of that metal. And, an experienc'd writer, upon the gold and ſilver mines of *America*, takes notice, that gold, itſelf, is often found diſguized in a reddiſh earth. *Vannochio*, alſo, a famous *Italian* mineraliſt, tells us, that a reddiſh ſort of earth, ſometimes, contains the richeſt metals. I, myſelf, have obſerv'd ſome finely figur'd cryſtals, to grow in a red earth. And, laſtly, a traveller preſented me with a certain earth, which he affirm'd to come from the diamond-mines; and this alſo I found to be red.

Colour'd ſands and gravel.

But a mineraliſt may make a more advantagious uſe of our hydroſtatical bucket, by employing it in weighing colour'd ſands and gravel. And, to ſhew how apt we are to over-look ſands, for want of trying them by weight; I have, ſometimes, ſeen a ſort, ſlighted as worthleſs, which being waſh'd, and view'd thro' a microſcope, appear'd like a heap of ſmall granats, and, perhaps, were really ſo. But what is more extraordinary, having obſerv'd, that the black ſand, uſually employ'd to dry freſh writing, ſeem'd, manifeſtly heavier than the common; I examin'd it by the hydroſtatical bucket, which gave us its ſpecific gravity to water, as about 4.6 to 1; and, by melting it down, with two or three parts of antimony, and caſting it into an iron cone, I found it of an irony nature; and, by applying it to a load-ſtone, to be far richer in metal, than any of our *Engliſh* iron-ores; for, at leaſt, ſeven parts in eight, would eaſily be taken up by the magnet. But ſuch obſervations as theſe, are not my chief motives to recommend the examination of ſands and gravel to the mineraliſt.

'Tis well known, that *European* traders yearly bring large quantities of gold from the coaſt of *Guinea*, which is waſh'd, or pick'd out of the ſand. And even in *Europe* there are rivers whoſe ſand is inrich'd by grains of gold. For this, the *Tagus*, that runs by *Lisbon*, and *Pactolus*, were famous among the ancients. An induſtrious chymiſt aſſur'd me, he got gold, with profit, from the ſand found on the banks of the *Rhine:* and there is a little river in *Savoy*, on the banks whereof, after a land-flood, I ſaw poor people employ'd in ſeeking for grains of gold. Some trial, alſo, that I cauſed purpoſely to be made, confirm'd me in a perſuaſion, that the ſands of many places, if skilfully treated by chymiſtry, may afford much more gold than is got from them in form of grains. For, beſides that there may be many particles of gold, ſo very minute, and cloſely fix'd to grains of ſand, as not to be taken notice of by the eye, or prove ſeparable by waſhing or picking; there may, as I conceive, be many ſmall portions of that metal incorporated with the body of the ſand; which a skilful artiſt, by the help of proper additions, might ſeparate to good advantage; eſpecially if with litharge, or minium, the ſand be firſt reduced to a glaſs, and then care be taken to get the volatile gold, by giving it a pure body, fit to retain and fix it; ſuch, for example, is fine ſilver, out of which, I remember, we ſeparated, by quartation, from a crucible full of vitrified ſand, and two or three fluxing materials, of ſmall price, ſixteen grains of pure gold.

Volatile gold is, I know, by many learned men, look'd upon as a fictitious thing; yet I have, by the help of an addition, inconſiderable as to bulk, and more ſo as to weight, without a naked fire, and in a glaſs retort, ſublimed crude gold, ſometimes in the form of a yellow ſalt; and ſometimes, when the operation ſucceeded better, in that of thin cryſtals, prettily ſhaped, gloſſy, and as red as rubies: this by the bye. It may perhaps be more uſeful to ſearchers after rich foſſils, to obſerve, that when they meet with ſands, earths, mineral fragments, *&c.* whoſe ſpecific gravity but little exceeds that of cryſtal, and yet, by the place wherein they are found, or by other tokens, give hopes of their containing particles of gold, they ſhould not haſtily reject them. For having ſometimes diſcover'd corpuſcles of iron and ſteel in a vaſt variety of foſſils, and diſguizes, I ſuſpect that ſome golden particles may lie conceal'd in ſeveral bodies, which are thought to contain no metal; and ſtill more in metalline ores.

Ores in general; and that of lead in particular.

But thoſe who would apply hydroſtatics to ores in general, ſhould endeavour to procure ſpecimens of different mines, eſpecially if they be found in the ſame country; and either by trial, or ſtrict inquiry, inform themſelves what proportion of the reſpective metal they contain. For theſe portions of ores and minerals being carefully weigh'd in air and water, and their ſpecific gravities thereby known, may ſerve for a kind of ſtandard, wherewith to compare, hydroſtatically, the metalline portions contain'd in other parcels of ore, of the ſame ſpecies. Thus, for inſtance, all our *Engliſh* lead-ores, worth taking notice of, may be divided into three kinds; and, in each of theſe, a latitude allow'd for greater or leſs degrees of goodneſs. Of the firſt ſort are thoſe that, in the ordinary ways of melting, hold, ſome of them, from thirty to forty pounds of lead in an hundred weight, and others to forty five pounds; which are often ſlighted, as ſcarce worth the working. The ſecond ſort may reach from forty-five to ſixty pounds in a hundred; but the moſt uſual proportion, I have found, in my trials, hath been about half the weight of the ore, in clean and malleable lead. Theſe are thought indifferently good, and worth the working; but others, comprized in this ſecond ſort, hold about fifty-five, and ſome near ſixty pounds; and theſe we look'd upon as pretty rich. As for the third ſort, it may conſiſt of thoſe that yield from ſixty to eighty pounds in the hundred; and ſuch ores are juſtly reputed very rich, eſpecially if they come near up to eighty pounds. I confeſs I never met with any that reach'd ſo far; but was aſſur'd by the maſter of his majeſty's mint, that he had found ſome ſuch. Theſe look as if they were all metal; and I have obſerv'd ſome lumps thereof to be compos'd of ſeveral large cubes, very firmly adhering to one another.

As to the ores, preſented me from different countries, I have inſerted their ſpecific gravities, in the table annexed to this treatiſe; ſuppoſing it may be of uſe, in making a conjecture, with other concurrent circumſtances, whether a mine may be advantagiouſly wrought; but concurrent circumſtances are by no means to be neglected.

SECT. II.

The hydroſtatical balance applied to the Materia medica; *and firſt, to the* Lapis Hæmatites.

WE come now to the ſecond part of our deſign; that is, to apply the hydroſtatical balance to the *Materia medica*. And firſt, there is a deeply red, and opake hard mineral, call'd in the ſhops by the name of *Lapis Hæmatites*, which tho' little uſed by our *Engliſh* phyſicians, is, in ſeveral places abroad, highly eſteem'd, and, in my opinion, not without cauſe; particularly for the ſomniferous quality to be obſerv'd in ſome of its preparations. Of the *Engliſh* ſort of this foſſil, we weigh'd, in a tender balance, a piece that chanc'd to amount to three ounces, two drams, and a quarter, firſt in the air, and then in water; and found its proportion to that liquor, as 4.15 to 1; that is, almoſt twice as heavy as a mere ſtone of equal bulk. So great a weight, confirm'd me in the conjecture I had made, that this ſtone contain'd a large proportion of metalline ſubſtance. I therefore ſublimed it, when finely powder'd, and diligently mix'd it with a double weight of ſal-armoniac, and found it, as I expected, very aſtringent upon the tongue, like ſeveral preparations of iron; and, for a farther proof, having put leſs than a grain of it into a ſpoonful or two of a ſtrong infuſion of galls, it immediately produced a black and inky mixture.

To the Lapis Lazuli.

Lapis Lazuli is ſometimes uſed by *European*, but more frequently by the *Arabian*, and other eaſtern phyſicians, and, particularly, as an emetic; which faculty I thought it poſſeſs'd, upon account of ſome metalline ingredient: and, accordingly, having hydroſtatically examin'd a piece, that was judg'd moderately rich, we found the proportion thereof to an equal

bulk

bulk of water, as 3 to 1; which argues, that notwithſtanding its briskneſs in operation, it contain'd a much ſmaller proportion of metalline ſubſtance than *Lapis Hæmatites*, or ſeveral leſs violent minerals.

The magnet. The load-ſtone is applicable to medicinal uſes, and may have ſenſible operations upon the human body; for which reaſon we examin'd it hydroſtatically, and found the weight of a lump thereof, that I judg'd to be either *Engliſh*, or *Norwegian*, was, in proportion to water of the ſame magnitude, as 4.93 to 1.

Calamine. *Lapis Calaminaris* is frequently uſed in phyſic, eſpecially by chymiſts, to abſorb acidities; for which purpoſe I prefer it to ſeveral more famous drugs: but tho' 'tis uſually employ'd only as an external remedy, yet ſome uncommon chymical preparations of it make me think it deſerves to be further examin'd. A famous empiric candidly aſſured me, that the medicine he with great ſucceſs made uſe of, againſt fluxes, was nothing but pure and well-ground *Lapis Calaminaris*; hence I readily conjectur'd, that it participates of a metalline nature: which may alſo be argued from its operation upon copper, which it turns into braſs: wherefore, weighing a piece of this foſſil, we found it to water, as 4.69 to 1.

Let it be here obſerv'd, that tho' when a hard foſſil is found to be much heavier than cryſtal, of the ſame bulk, 'tis very probable, the ſolid concrete contains a conſiderable portion of ſome metalline, or ponderous mineral body; whence its good or bad qualities, with regard to the animal œconomy, may probably be deduced; yet 'tis very poſſible for a foſſil to be endow'd with medicinal virtues, or noxious qualities, on account of its extraneous matter, tho' its ſpecific gravity but little exceeds that of cryſtal, or the difference be inconſiderable; for a very ſmall proportion of adventitious metalline, or mineral ſubſtance, if it be of a violent nature, may, in ſome caſes, diffuſe itſelf thro' the reſt of the maſs, and impregnate it with active qualities; as we ſhall ſee hereafter.

Red coral. But further, this method of examination may aſſiſt us to gueſs, with probability, whether a mineral body be of a ſtony nature, or not. Thus coral, for inſtance, is by ſome thought to be a plant, by others a *Lythodendron*; but, by the greater number, a precious ſtone: in which difference of opinions, its ſpecific gravity may prove of conſiderable ſervice. We therefore weigh'd a piece of choice and well-colour'd red coral, firſt in air, and then in water, and found its proportion to an equal bulk of the latter, to be as 2.68 to 1: whence their opinion, who take it for a ſtone, ſeems moſt probable; ſince its ſpecific gravity exceeds that of cryſtal.

Pearl. Pearls, becauſe of their hardneſs, are often thought of a ſtony nature. A monſtrous one being preſented me, that was taken out of an oyſter, its ſhape irregular, its magnitude extraordinary, and its aerial weight amounting to 206 grains; I weigh'd it in water, and found it to be thereto as 2.51 to 1: ſo that its ſpecific gravity nearly equall'd that of cryſtal.

Calculi humani.

Calculi humani are frequently ſuppoſed to be true and genuine ſtones; and, indeed, the great hardneſs of ſeveral of 'em, may not improperly entitle them to that appellation: however, I ſhould rather call them animal ſtones, than ſimply ſtones; which, by our method, may be eaſily diſtinguiſh'd from thoſe, and other the like hard concretions, found in the bodies of animals. For, having a conſiderable number of theſe animal ſtones; I found, not only by chymical analyſis, that they belong'd not to the mineral kingdom; but, by an hydroſtatical examination of ſeveral of them, I found they differ'd greatly in ſpecific gravity from true foſſil ſtones; one of them, weighing above ſix drams and a half, was found to be in proportion to an equal bulk of water, as 1.76 to 1; and another, that weigh'd four drams and above a half in the air, being alſo weigh'd in water, appear'd to be thereto as 1.69 to 1.

I mention theſe ſtones as belonging to the *Materia medica*, tho' they are look'd upon rather as morbific ſubſtances; becauſe a famous phyſician, who practiſed long in the *Eaſt Indies*, and had better opportunity than any *European* had before him, to try the virtue of bezoar, either equals or prefers them to oriental bezoar.

And bezoar.

We made the like experiment upon bezoar-ſtones; the firſt of which weighing in the air three drams, and odd grains, was found to be in proportion to water of the ſame bulk, as 1.47 to 1. Another, weighing ſomewhat leſs than three drams, prov'd to an equal bulk of water, as 1.53 to 1. A third, being the nucleus of a larger ſtone, weigh'd in the air two drams and fifty-one grains; and its proportion to water of the ſame magnitude, was found as 1.55 to 1. In which ſeveral inſtances, we may obſerve, that theſe animal ſtones, not amounting to twice the weight of an equal bulk of water, have a leſs ſpecific gravity, by above a fifth part, than a true foſſil ſtone.

The hydroſtatical balance will diſtinguiſh between bodies of the ſame denomination.

Again, we may by hydroſtatics be aſſiſted to diſcover the reſemblance or difference between bodies of the ſame denomination. Thus, for inſtance, we have found a notable difference between the ſpecific weights of ſeveral load-ſtones dug up in different countries or mines: and if a greater number and variety of experiments of this kind were made, we might poſſibly find, that, *cæteris paribus*, the magnets of one country or mine, are conſiderably heavier than thoſe of another: for I uſually obſerve, that the *Engliſh* and *Norwegian* load-ſtones are heavier *in ſpecie* than thoſe which are ſaid to come from *Italy*. And this difference of weight between foſſils of the ſame kind, when conſiderable, may help us to diſtinguiſh between the ſtones of the ſame loweſt ſpecies, that are peculiar to different countries or mines. But in caſe the unequal weight proceed, as it often does, from an adventitious matter inſinuated into the more genuine part of the foſſil, whilſt that remain'd fluid or ſoft; it may enable us to conjecture at its greater or leſs purity; which, on many occaſions, may be of no ſmall uſe to a phyſician, a jeweller, or a naturaliſt.

Between genuine ſtones and counterfeit.

Another conſiderable advantage of our hydroſtatical method, is, that it helps to diſcern genuine ſtones from counterfeit, which too often paſs for true, to the great prejudice of phyſicians and their patients, and the great loſs of lapidaries and their cuſtomers: for as there are few qualities of bodies more eſſential than their ponderoſity; ſo there is ſcarce any wherein impoſtors find more difficulty to make a notable alteration, than in this, without being diſcover'd. In ſeveral caſes, indeed, 'tis not very difficult to alter the ſpecific weight of a particular body, yet it may, perhaps, be impracticable to make a conſiderable change in that quality, unleſs by ſuch additions or operations, as will cauſe a ſenſible alteration in ſome other qualities too, and thereby ſubject the fallacy to a diſcovery. And this will prove more particularly difficult to vulgar cheats, or adulterators of gems, and other valuable minerals; becauſe the ſmall knowledge they have of the number and variety of natural and artificial productions, confines them within narrow bounds, to accompliſh their fraudulent deſigns. And whilſt they are intent upon counterfeiting only the more obvious qualities of things, and, perhaps, upon eluding the known and popular trials; they will, probably neglect the ſpecific gravity. By this means a pearl, for inſtance, is diſcoverable to be counterfeit, without the leaſt prejudice to it. And I remember, ſome factitious corals, which I made, to ſhew what might be done in that kind, were, notwithſtanding their fine colour, ſhape, and gloſs, eaſily diſcoverable by their ſpecific gravity manifeſtly exceeding that of natural corals.

And ſometimes I have made paſtes, or factitious gems, with red lead; which, tho' tranſparent, and finely colour'd; yet by reaſon of their weight, were very liable to be detected by hydroſtatical trials. I have ſeen a fair adulterate bezoar-ſtone ſo reſembling the genuine, that a great price was ſet upon it; but being brought to me for my opinion, I made no doubt of its being counterfeit, from its appearing as heavy, as a mineral ſtone of the ſame bulk.

And ſhew the genuineneſs and purity of bodies.

Another uſe to be made of our hydroſtatical method of examining ſolids, is, on many occaſions, to aſſiſt us in making eſtimates of the genuineneſs, or the degree of purity of ſeveral bodies, that may be uſefully employ'd in phyſic; provided they are heavy enough to ſink in water: for when we have once found the ſpecific gravity of a concrete, which we know to be good and genuine; the degree of its ponderoſity may ſerve for a kind of ſtandard, whereby to judge of other portions of the ſame denomination.

A great difference ſhould, doubtleſs, be made between the eſtimate of ſome ſtones, vulgarly called gems, according as they ſerve the purpoſes of jewellers and goldſmiths, of phyſicians or chymiſts: for the tradeſman, who uſually aims wholly at the beauty and luſtre of a ſtone, may juſtly eſteem thoſe, *cæteris paribus*, the beſt, which are *in ſpecie* lighteſt; becauſe ſuch are generally more uniform as to ſenſe, and more tranſparent; and alſo receive their colour from pigments of finer parts. But, on the contrary, thoſe who ſeek for medicinal virtues in gems, may juſtly value ſuch the higheſt, as are moſt ponderous; becauſe they are more plentifully ſtock'd with metalline or mineral ſubſtances: whence the greateſt part of their virtue is, in all probability, derived. And this difference in the ſpecific weight of ſtones of the ſame name, I have ſometimes found far greater than one, who has not try'd them, would imagine; as will appear hereafter: yet I would not from hence infer, that even ſuch ſtones as appear fine, and are but light in their kind, have not metalline particles that may give them conſiderable medicinal virtues. For there are mineral pigments of ſo ſubtile a nature, that ſuch an inſenſibly ſmall quantity, as will ſcarce render them heavier than colourleſs gems, may be diffuſed thro' the whole, and impregnate every ſenſible part thereof. Thus I have known five grains of powder'd zaphora, mix'd with an ounce and half a dram of finely pulveriz'd *Venice* glaſs, and kept an hour in fuſion by a very violent fire, afford a tranſparent maſs, which was throughout of a fine deep blue colour; ſo that one part of the pigment tinged above a hundred parts of the glaſs: and when we added eight grains of zaphora to an ounce of glaſs, that is, one part to ſixty; the mixture having been kept for the like time in ſtrong fuſion; the maſs was too deeply colour'd for a handſome gem.

And further to manifeſt, that a little metalline matter may ſuffice to give a tincture, and thereby impart a virtue to a vitreous body, and even to gems; I ſhall add another experiment. I had long conjectured, that there was in granats, eſpecially in ſome of a deep colour, abundance of chalybeate corpuſcles, more than ſufficient for the granat itſelf: upon this ſuppoſition, I took a *German* granat, which I had kept by me for a rarity, on account of its bigneſs and deep colour. Of this, carefully reduced to very fine powder, we exactly mix'd eight grains with an ounce of finely pulveriz'd cryſtalline glaſs, and afterwards kept the mixture for two hours in a very hot furnace; by which means, we obtain'd a pretty uniform maſs, ting'd of a green colour, like what prepared iron or ſteel gives to pure glaſs.

Thus much may ſerve in ſome meaſure to ſhew the uſe of the hydroſtatical method of examining drugs; upon a ſuppoſition that they are ſolid, of a ſufficient bulk, and gravity able to ſink them in water. But many ſimples, and other ponderable ſubſtances, that may upon good grounds, be refer'd to the *Materia medica*, want one or more of theſe conditions: I ſhall therefore offer the expedients I make uſe of in ſuch caſes, to bring them to this hydroſtatical trial. Now a body may either be liquid, and conſequently not immediately applicable to a ſlender ſtring; in the form of a powder, or ſmall fragments, and impoſſible, or difficult at leaſt, to faſten each of them to a hair, and ſuſpend it as a body of a greater bulk; or laſtly, diſſoluble in water, and conſequently unfit to be weigh'd therein.

This method of examination applied to fluids heavier than water, and unapt to mix therewith; for inſtance, to mercury.

Suppoſe then the liquor we would examine, be ſpecifically heavier than water, and unapt to mix therewith; I take ſuch a bucket, or wide-mouth'd glaſs, as we formerly mention'd, capable of containing an ounce or two of common water, and whoſe weight in air is about three or four drams. This glaſs we weigh very carefully, once for all, firſt in air, and then in water; and thereby find a weight equivalent to it in water: which being put into the oppoſite ſcale of the balance, whilſt the veſſel hangs under the ſurface of the water, may be conſider'd as having no weight at all; and, conſequently, the additional weight of this bucket, may be alone taken for that of the contain'd body itſelf in water: ſo that, for inſtance, the bucket thus makes a maſs of quick-ſilver, tho' fluid, as fit for weighing as if it were coagulated into a ſolid. And in this manner we proceed to weigh it hydroſtatically, as if it were a ſolid.

But, for the more ready performance hereof, 'tis convenient to have in readineſs a couple of weights, the one equal to the weight of the glaſs-bucket in the air, and the other to the weight of the ſame in water; for theſe immediately afford a counterpoiſe to the veſſel in either medium. And thus we put a ſmall glaſs-jar, capable of containing half an ounce of water, into one ſcale of a tender balance, and furniſh'd the oppoſite with a counterpoiſe thereto; into this little veſſel we pour'd 480 grains of *Spaniſh* mercury, and ſuffer'd the bucket, with this mercury in it, to hang by an horſe-hair, from one of the ſcales, in a deep glaſs veſſel of water: there was now in the oppoſite ſcale a counterpoiſe to the weight of the glaſs in the water; ſo that the weights that were added, gave us the weight of the quick-ſilver only; the weight of the glaſs being already allow'd for. By this means we found the weight of the quick-ſilver, in water, to be 446 grains; whereby the greater number being divided, the quotient is 14, and about $\frac{1}{10}$: ſo that the mercury appear'd to be in gravity, to water of the ſame bulk as 14.11 to 1: tho', in former trials, I ſcarce found common quick-ſilver to weigh full fourteen times, and ſometimes ſcarce thirteen and a half ſo much as an equal bulk of water. Whether the weight of the preſent mercury proceeded from its participating of gold more than others; which, from another experiment, ſeem'd not improbable; I will not here determine.

However, upon abundance of tryals of this kind, I did not find that all running mercuries, tho' they appear'd unadulterated, were preciſely of the ſame weight. Nay, I obſerv'd, that even diſtill'd mercuries, if once combined with metalline bodies, and particularly if drawn from fine gold, differ'd more from the common ſort, than theſe from each other: and alſo between common mercuries diſtill'd, we found a notable difference. And, by this method, tho' not always with the ſame eaſe, we may examine the ſpecific gravity of other heavy fluids that are indiſpoſed to mix with water; ſuch as are, for inſtance, the chymical oils of cinnamon, cloves, guaiacum, *&c.* But what made me the more circumſtantial in delivering the foregoing experiment, was, that this practical direction for weighing one liquor in another, will, hereafter, appear applicable to very uſeful purpoſes.

To powders and fragments of bodies.

The way of hydroſtatically examining the powders and fragments of ſinking bodies, hath been already touch'd upon; and differs little from the former: only it deſerves to be again repeated, that great caution muſt be here uſed to moiſten the powder with the ſame water wherein 'tis to be weigh'd, before the experiment is began; that the liquor may have time to inſinuate itſelf between the dry corpuſcles, and thence expel the air that was harbour'd in their interſtices: for theſe little aerial portions, if not thus ſeaſonably expell'd, would, upon the immerſion of the veſſel, produce numberleſs bubbles in the water, that would buoy up, or faſten themſelves to the ſmall bodies, and render the experiment uncertain, or fallacious.

If this way of examining bodies be carefully practiſed by a skilful hand, and with a tender balance, it may prove of conſiderable uſe to phyſicians, druggiſts, apothecaries, lapidaries and goldſmiths. Thus, for example, the fragments of the five precious ſtones, that are made ingredients of *Confectio Hiacinthi*, may each ſort of them, a-part, be properly examin'd by their weight in water, when the true ſpecific gravity of a parcel of the fineſt is once known. And, by the way, tho' granats are reckoned among the five precious medicinal ſtones, and, in ſome diſpenſatories, are prefer'd as the beſt; I have found a great difference, in point of ponderoſity, between *European* and *American* granats, compar'd with others, which I myſelf took out of an odd *American* mineral; whence, 'tis natural to conclude, that their virtues might be very different, if not as to kind, yet at leaſt as to degree. And not only factitious pearls, that often paſs for genuine, may often, by this expedient be diſcover'd, eſpecially if mercury enter their compoſition; but we may, probably, by the ſame method, diſtinguiſh the natural pearls of ſeveral kinds, and different countries; whereof I have ſeen, and poſſeſs a far greater variety than would eaſily be credited. But, becauſe it more concerns phyſicians, and their patients, to make an eſtimate of ſeed-pearls, than of the larger ſort, that are ſeldom uſed but for ornament; I ſhall here obſerve, that, when furniſh'd with very fine oriental ſeed-pearls, which ſeem'd proper for a ſtandard wherewith to compare others, we found them to water, of the ſame bulk, as $2\frac{3}{4}$ to 1.

To bodies that will diſſolve or mix with water.

But what courſe muſt we take, when the body to be hydroſtatically examin'd, will diſſolve, or eaſily mix with water? Why, tho' the propoſed body cannot be immediately weigh'd in water; yet we may ſubſtitute another liquor, that will not diſſolve it, and thereby find its ſpecific gravity to that medium; and then, by comparing the difference of thoſe two liquors, in point of gravity, we ſhall diſcover the body's weight in water; ſuppoſing it could have been kept undiſſolv'd therein. Conſidering, then, that, except quick-ſilver, the viſible fluids we can command, are either of an aqueous, or oily nature; and that moſt bodies, whereof we can make ſolutions in liquors of the former kind, will not diſſolve in thoſe of the latter; we preſume, that the moſt ſaline bodies, ſuch as alum, vitriol, ſal-gem, borax, ſublimate, &c. may be commodiouſly weigh'd in oily fluids, as oil of turpentine, for inſtance, or rather the ſpirit of turpentine, which I, in this caſe, prefer to any other fluid.

And here, to avoid the trouble of calculation, we may often eaſily find the ſpecific gravity of bodies to others of the ſame, or a different ſpecies; and ſo make a probable gueſs as to their genuineneſs, by being provided with one piece of the body, of known purity, to ſerve as a ſtandard.

dard. Thus, for example, if the goodneſs of ſublimate be ſuſpected, take an ounce of ſome that is known to be fine, carefully balance it in oil of turpentine, and note its weight therein; then an ounce of the ſuſpected ſublimate being weigh'd as the former, if it prove of the ſame gravity with the ſtandard, it may be ſuppoſed good; but if it weighs leſs, 'tis a ſign it wants a due proportion of mercury. The ſame method is applicable to *Mercurius dulcis*, and many other bodies, wholly, or partially diſſoluble in water; as alum, which is often ſophiſticated with ſome baſer ſalt; to *Roman* vitriol, which is, ſometimes, either counterfeited or adulterated by roch-alum and a tincture of copper: and, according as the weight of the body in oil recedes more or leſs from that of the ſtandard; ſo the adulteration may be probably concluded leſs or greater.

Another way of finding the ſpecific gravity of fluids.

I might alſo obſerve another uſeful way of eſtimating the gravity of ſolids and fluids, by taking a heavy ſolid, and carefully obſerving, once for all, its weight in the air; then weighing the ſame, firſt in one of the liquors to be examin'd, and afterwards in others: whence the difference between the ſolid, and each of the fluids being obſerv'd, 'tis eaſy to find the ſpecific gravity of each, and to aſſign the proportions betwixt them.

But if only one ſolid be employ'd, 'tis neceſſary ſuch an one be choſen; as hath a much greater ſpecific gravity than is barely neceſſary to ſink it in water; becauſe ſome liquors are much heavier than that: nay, it ought to be heavy enough to ſink in all fluids, except quick-ſilver, if 'tis deſign'd for experiments in general. But 'tis not very eaſy to pitch upon ſuch a ſingle ſolid as hath all the qualities requiſite to our purpoſes: for, it ought not to loſe any of its weight by the inſenſible avolation of effluvia, yet be of a conſiderable ſpecific gravity, and not too large, or intenſely heavy, leſt it overload a tender balance, or require too much liquor to ſurround it; its texture ſhould be proof againſt ſharp and piercing menſtrua; it ſhould alſo be of a make not liable to be broken or ſpoil'd; and, laſtly, of a natural, uniform, and procurable ſubſtance.

Brimſtone, hard wax, ivory, and white marble, have been, when properly ſhaped, ſeverally made uſe of for this purpoſe; yet they all wanted one or other of the deſirable qualifications mention'd. I, therefore, frequently employ'd three other bodies, as more proper. The firſt was a piece of amber, between three and four drams in weight, of a high yellow colour, very tranſparent, and of an uniform texture, and convenient figure. This we apply'd to examine the lighter ſort of liquors, ſuch as water, wine, brandy, &c. but 'tis not proper for the more ponderous liquors, ſince 'twill float at the top thereof.

The ſecond is a globular glaſs, which I caus'd to be blown at a lamp, and to be hermetically ſeal'd at the neck, that was purpoſely made very ſhort, after there had been lodg'd therein juſt as much quick-ſilver as we gueſs'd would ſerve to ſink it in any fluid, except mercury. This, by reaſon of its great bulk, in proportion to its weight, was fit to diſcover minute differences in the gravity of the liquors 'twas weigh'd in, and could not be corroded by ſharp menſtrua; and therefore, on ſeveral occaſions, I prefer'd this inſtrument to either of the other.

But this being too liable to accidents, for experiments that are to be imparted, and recorded for poſterity; I made uſe of rock-cryſtal, which, on account of its numerous good qualities, affords a fit meaſure, whereto other bodies may be compared. Of this pure concrete, we employ'd an almoſt compleat globe, weighing in the air two ounces, thirty-three grains, wherein were two ſmall holes near one another, that might eaſily be ſtopped up with hard wax, after an horſe-hair had been run thro' them; by means whereof it was eaſily faſten'd to the ſcale, and made to hang in the water. The bulk of this ſphere made it the more fit to diſcover ſmall differences between liquors, in point of gravity: but becauſe we may have often occaſion to know the weight of fluids, which, by reaſon of their ſcarcity, we can command only in ſmall quantities; as chymical oils, tinctures, eſſences, *&c.* we provide for ſuch liquors an hexagonal priſm of natural cryſtal, with a kind of pyramid at one end, which, by reaſon of its oblong figure, might be commodiouſly weigh'd in a ſlender cylindrical glaſs, that required but a ſmall quantity of liquor to ſurround a body ſo conveniently ſhaped, and that weigh'd in the air but half an ounce, and ſixteen grains. And to render the obſervations made with theſe two bodies upon medicinal and other liquors, the more uſeful; let it be taken notice of, once for all, that the ball of cryſtal was to an equal bulk of water, as 2.57 to 1, or thereabouts; and the priſmatical oblong piece, as 2.66 to 1, to the ſame.

I have been the more particular upon the way of trying the gravity of ſeveral liquors with one ſolid, becauſe a double application may be made of it on ſeveral occaſions, ſerviceable to chymiſts, phyſicians, apothecaries, and others.

The ſeveral uſes thereof.

And firſt, a piece of clear amber, or ſome ſuch convenient body, that is not too little, nor, *in ſpecie*, too heavy, may ſerve the chymiſt, apothecary, *&c.* to make probable gueſſes at the degree of ſpirituoſity, or tenuity, to be found in many liquors belonging to the vegetable, or animal kingdom, and this with far leſs error than by thoſe ſigns whereon the common conjectures are grounded. For a ſtandard liquor being provided, 'twill be eaſy, by obſerving the different weights of amber in ſeveral fluids, to judge of the fineneſs of any of them in its kind; for, *cæteris paribus*, that is the thinneſt, or abounds moſt in ſpirituous parts, where the ſolid weighs more than in another; as for inſtance, the amber we employ'd, that in water weigh'd $6\frac{3}{4}$ grains, weigh'd in common red *French* wine $8\frac{1}{2}$ grains; in common brandy, of a pretty good ſort, $17\frac{1}{8}$ grains; and in ſpirit of wine, highly rectify'd, $34\frac{1}{8}$ grains. The ſame way may be employ'd to judge of the ſtrength of ſpirit of vinegar, *&c.* But here it muſt paſs for a general rule, that, probably, as in liquors the ſolid weighs more or leſs, according as the liquor 'tis weigh'd in is more or leſs ſpirituous; ſo, on the contrary, in acid ſpirits and liquors, the leſs the ſolid weighs, the ſtronger the fluid muſt be reputed, that greater decreaſe of weight proceeding uſually from the greater proportion it contains of fixed ſalts.

'Tis another advantage of this method, that as the common way of trying the goodneſs of ſpirit of wine, brandy, *&c.* by ſetting fire to a ſpoonful of them, to ſee how much is inflammable, and what portion of phlegm will be left behind; whereby, in tract of time, a conſiderable loſs is ſuſtain'd: the proper proof may be made in our way, without any ſuch expence.

By theſe obſervations, alſo, eſtimates may be made of undiſtill'd liquors of the ſame kind; as ſeveral ſorts of beer, ale, cyder, *&c.* and the ſame ſolid may be employ'd to compare liquors of different kinds with each other.

But if the liquors to be examin'd are very ponderous, amber will not be a proper ſolid in this caſe: for I have found by trial, not only that it ſwims or floats in ſeveral fluids made by ſolution of ſalts, whether in the moiſt air, or with water; ſuch as oil of tartar *per deliquium*, a ſolution of the ſalt of pot-aſhes, *&c.* but that ſome diſtill'd liquors would not ſuffer it to ſink to the bottom of them; as I found in oil of vitriol, ſpirit of nitre, and even good ſpirit of ſalt.

There is ſtill another uſe to be made of our hydroſtatical ſolid, which may frequently be as conſiderable in general, by aſſiſting in proportioning the ſtrength of menſtrua, as the former is to diſcover the ſtrength of particular liquors already prepared. For there are many experiments that do not ſucceed ſo well, unleſs the liquor be of a determinate degree of ſtrength. Thus I have found, that if *Aqua fortis*, whoſe ſtrength is reckoned the beſt quality it can have, were rectify'd too high, it would not diſſolve ſilver, but require to be weaken'd by an addition of water; and that it would not near ſo well diſſolve the raſpings of crude lead, when only moderately ſtrong, and fit to diſſolve ſilver, as when conſiderably diluted with water. And in making extracts from many vegetable ſubſtances, chymiſts themſelves may fall into a miſtake, by affecting to employ their moſt rectified ſpirit of wine, as the beſt menſtruum for their purpoſe; for the medicinal virtue of many ſuch bodies does not wholly reſide in what they call their ſulphur, or roſin, which indeed is beſt diſſolv'd by highly rectify'd ſpirit of wine; but in a more gummy, and, perhaps, mucilaginous ſubſtance, for whoſe extraction a moderately low ſpirit is more proper. Thus we ſee that gum-arabic, gum-tragacanth, *&c.* are indiſpoſed to diſſolve with the beſt rectify'd ſpirit of wine, but readily mix with more aqueous liquors. And ſome bodies, tho' diſſoluble in both kinds of menſtrua, yet open leſs eaſily to ſtrong ſpirit of wine, than wateriſh fluids; as may be obſerv'd particularly in myrrh.

And thus much may ſerve to ſhew how uſeful it will be, on ſeveral occaſions, to regard the degree of ſtrength of the menſtruum, or liquor, employ'd in any curious experiment; ſo that when it ſhould be repeated to the ſame purpoſe only, we may be able to bring the fluid we make uſe of to the ſame degree of ſtrength with that before employ'd, which produced the deſigned effect. But, in very accurate experiments, 'tis proper to obſerve, that if the liquor be very ponderous *in ſpecie*, as oil of vitriol, or oil of tartar *per deliquium*, ſomething ought to be placed in the ſcale, from which the ſolid hangs, to compenſate for the increas'd part of the hair, which muſt be conſider'd as a ſomewhat lighter body than the fluid, and therefore capable of buoying up the ſolid.

Still other methods for the ſame purpoſe.

Beſides the method juſt mention'd, there is another, which we have often found of ſervice, in comparing the weight of different liquors of the ſame magnitude. This is done by ſucceſſively filling a vial, furniſh'd with a long ſlender cylindrical ſtem, to a certain ſtanding mark, made near the top, with the ſeveral liquors, to be, by weight, compared together. But this not being hydroſtatical, I ſhall no longer inſiſt upon it here. To proceed, therefore, there is, likewiſe, another way to diſcover whether two or more liquors propoſed differ in ſpecific gravity; and to make ſome eſtimate of their difference: *viz.* by a hollow cylinder of braſs, or other metal, made ſomewhat heavy at the bottom, that it may ſwim upright; which will ſink more or leſs, in ſeveral liquors, as they are lighter or heavier than one another. But *Merſennus*, who propoſes this way, confeſſes it to be very difficult to make ſure obſervations thereby; to which I add, that the metal may be corroded, or otherwiſe affected by urinous menſtrua.

What *Merſennus* ſaid of this inſtrument, is applicable to a different one, made of two glaſs bubbles, and a very ſlender ſtem, which is hermetically ſealed, with a ballaſt of quickſilver in the loweſt, to keep it ſteady, when partially immers'd in liquors, where, like the metalline cylinder, it ſinks deeper in the lighter than in the heavier, according to their differences in gravity. But tho' I have often employ'd this inſtrument, and found it ſerviceable, when I made uſe of ſeveral of different ſizes, according to the various liquors I was to examine; yet it is, in many caſes, inferior to the balance.

Merſennus alſo propoſes another hydroſtatical manner of weighing liquors in water, thus. Take a glaſs vial, to which, being firſt weigh'd in air, and then in water, you are exactly to fit a ſtopple of wax, or cork; this done, fill the vial with the liquor you would examine, ſo that no air be left between that and the ſtopple: the veſſel, thus fill'd, is to be weigh'd in water, and ſubſtracting from its weight there, that of the glaſs in water, and alſo that of the ſtopple, the remainder will give the weight of the liquor in water. This may perhaps be ſerviceable, on ſome occaſions, yet I fear it is troubleſome in practice; for ordinary vials, capable of containing a competent quantity of liquor, are uſually too heavy to be applied to tender balances; and common ſtopples will be ſubject to various inconveniencies: for which reaſon, I prefer to it a method I formerly thought of, and which I have ſometimes put in practice, by chuſing a ſmall round vial, and inſtead of ordinary ſtopples, fitting it with one of glaſs, carefully ground to the neck of it: for, by this means, the inconveniencies of a ſtopple, lighter than water, were avoided; nor would that alter its ſpecific gravity, either by abſorbing, or evaporating; nor would it be penetrated by the moſt ſubtle, or corroded by the moſt fretting ſpirits: to which may be added, (becauſe, in ſome caſes, it is conſiderable) that a glaſs ſtopple will not communicate any tincture, or extraneous quality to the liquor; which cannot be ſaid of cork, or wax, in regard of ſome ſubtile, and very corroſive liquors. This vial, then, together with its ſtopple, being carefully weigh'd, firſt in air, and then in water, to ſettle the gravity of the whole inſtrument; we fill'd it exactly with the liquor to be examin'd, and proceeded as if we were to weigh quick-ſilver, in the manner already deſcribed: the weight of the given liquor in water being thus obtain'd, its proportion, to an equal bulk thereof, may be eaſily diſcover'd. This way of examining liquors has its uſe; and I the rather, ſometimes, made choice thereof, becauſe 'tis applicable to all kinds of fluids, whether heavier *in ſpecie* than water, or lighter.

Laying aſide the ſtopple, the round ball may ſerve, on ſeveral occaſions, inſtead of the hydroſtatical bucket, formerly mentioned; for the weighing of quick-ſilver, and heavy powders; eſpecially coarſe ones. But, if the inſtrument be fitly ſhaped, and not too heavy, a greater conveniency than this attends it: for when you have liquor enough to ſurround the bottle, it may be commodiouſly ſubſtituted for the hydroſtatical bubble, with quick-ſilver; its exact ſtopple ſupplying the place of an hermetical ſeal: and, beſides, 'tis far leſs ſubject to break than a bubble. That I made moſt uſe of, which weigh'd about 709 grains, being well ſtopp'd, with only air in it, would ſink by its own weight, in brandy, wine, water, *&c.* And, if it were to be employ'd in liquors much more ponderous than water, as *Aqua fortis*, oil of tartar *per deliquium*, *&c.* 'twas eaſy to fit it for them, alſo, by putting into it a quantity of quick-ſilver, of a determinate weight, before it be ſtopp'd; which balaſt, when the operation is over, may be eaſily taken out, and kept a-part for the like uſes.

But, notwithſtanding all this, ſince glaſſes of a proper ſize, ſhape, and weight, fit for tender balances, and furniſh'd with exact glaſs ſtopples, are very difficult to procure; and ſince the way itſelf is ſubject to ſome inconveniencies, it ſeems, in general, that this way of finding the weight of liquors in water, is inferior to the more ſimple ones, before recommended.

The uſe and advantages of weighing one fluid in another.

Having now laid down the method of weighing one liquor in another, 'tis fit we ſubjoin ſome application of it.

Among other advantages of hydroſtatics, to a ſagacious phyſician, 'tis none of the leaſt, that it gives him the ſpecific gravities of various liquors; which may not only help to diſtinguiſh the genuine and good, from the adulterate and decay'd, but ſerve to other purpoſes alſo. Inſtances hereof are afforded by the juices of herbs, and fruits; a determinate quantity of which, being firſt weigh'd in the hydroſtatical bucket; and ſome oil of turpentine poured on it, we ſink it warily in the ſame liquor, whoſe ſpecific gravity to refined ſilver, or clear rock cryſtal, is before-hand carefully regiſtred: for thus ſubſtituting this oil for common water, we may diſcover the ſpecific gravity of liquors that would mix with water. And, by this means, we may find, not only the difference in weight, between the juices of plants of differing kinds, but, on ſome occaſions, obſerve how far the keeping of a juice, for ſome time, or the fermentation, or putrefaction thereof, will alter its ſpecific gravity. Other liquids, alſo, that are uſed in phyſic, but not ponderable in water, may, by this way, be examin'd; as honey, vinegar, verjuice, *&c.* And, by the ſame method, likewiſe, the ſpecific gravity of the juices of fruits, may be diſcover'd, and compar'd together. And hence a perſon of curioſity will, probably, be enabled to take notice of the differences produced in their ſpecific gravities, thro' their ſeveral ſucceſſive ſtates, at different times. Thus the juice of ripe grapes alters when newly preſs'd; again, when it begins to ferment; when it becomes new wine; when it has attained its full maturity, and perfection; and, laſtly, when it degenerates into prick'd wine, and abſolutely changes into vinegar, or *Vappa.*

All waters nearly of the ſame weight.

It may be here proper to obviate an objection, that will, probably, be made againſt the method hitherto deliver'd, of finding a proportion in weight, betwixt a ſinking body and water of the ſame bulk. Naturaliſts might alledge, that, by this method, we cannot diſcover the proportion between a ſolid body, and water in general; but only betwixt the propoſed body, and the particular water 'tis weigh'd in; becauſe there may be a great diſparity between liquors that are call'd common water. Thus ſome travellers tell us, that the water of the river *Ganges* is, by a fifth part, lighter than ours.

To this plauſible objection, I anſwer, firſt, that having had opportunity to examine the weight of various waters, ſome of them taken up in places very diſtant from one another, I found the difference between their ſpecific gravities to be exceeding ſmall; perhaps, not above the thouſandth part of the weight of either: nor did I find any conſiderable difference, between the weight of ſeveral waters of differing kinds; as between ſpring-water, river-water, rain-water, and ſnow-water: tho' this laſt was ſomewhat lighter than any of the reſt. And having received water from the river *Ganges* itſelf, I found it very little, if at all, lighter than ſome of our common waters.

And ſecondly, 'tis not neceſſary that the proportion obtainable by our method, ſhould be mathematically exact: for in experiments where we are to deal with groſs matter, and employ material inſtruments, a phyſical accuracy will ſuffice.

To diſcover the magnitudes of bodies, hydroſtatically.

There is an uſe of hydroſtatics, which tho' it do not directly tend to the examination of the *Materia medica*, may yet be ſerviceable, both to the phyſician and the naturaliſt, in delivering deſcriptions thereof; and thereby indirectly conduce to the knowledge of drugs; and help to diſtinguiſh between genuine and adulterate ſimples. 'Tis known, that the writers upon the *Materia medica*, uſually ſet down the magnitude of the bodies they deſcribe, by very uncertain gueſſes; and thoſe who aſſign them determinate meaſures; either do it by ſaying, ſuch a fruit, for example, is an inch, or two inches long, and half an inch, or a whole one, broad: but, according to this way of deſcribing bodies, there may, by reaſon of the great variety of figures they are capable of, be a very great difference in the bulk of bodies, to each of which, the ſame length and breadth are applicable.

I cauſed ſeveral cubes to be carefully made, by skilful artificers, of different ſizes, and materials, whoſe ſides were each of them an exact inch, or a preciſe number of inches. Theſe cubes being carefully weigh'd in exact balances, firſt in air, and then in common water, we concluded, from the reſult of our ſeveral trials, that, without conſiderable error, a cubical inch of water might be ſuppoſed to weigh 256 grains.

Suppoſe then for example, that a ſolid heavier *in ſpecie* than water, having been weigh'd firſt in the air, be found in water to loſe 256 grains of its weight; the dimenſions of this ſolid, if it were of a cubical ſhape, will equal a cubic inch; ſo that if the given body be ſuppos'd a metal eaſily fuſible, as tin, or lead, melted and pour'd into a hollow, metalline, cubic inch, and ſuffer'd to cool, it would exactly fill it. For a ſinking ſolid weighs leſs in water than in air, by the weight of an equal bulk of water. And, as a cubical inch of water weighs 256 grains, it follows, that when the decreaſe of a body's weight in water is 256 grains, the ſolid content of that body is a cubical inch; ſince a body of water, weighing 256 grains, is equal in magnitude, as well to the ſolid, as to a cubic inch of water.

And here it may prevent a ſcruple, to obſerve, that to make bodies equal in magnitude, it is not neceſſary that they ſhould be of the ſame weight or matter; as is evident in bullets of copper, tin, and gold, caſt in the ſame mould. For tho' they be all equal in bulk, yet the bullet of copper will be much heavier than that of tin; and the bullet of pure gold, more than twice as heavy as that of copper. Whenſoever, therefore, a ſolid, ponderous enough to ſink in water, loſes therein 256 grains of the weight it had in air; the magnitude, or bulk of that body, is equal to a cubical inch; of whatever matter it conſiſts, or of what ſhape ſoever it be. And, in caſe the ſolid propoſed, loſes of its weight in the water, leſs than 256 grains; its bulk will be proportionably leſs than a cubical inch. Thus every 32 grains that the ſolid loſes of its weight in the water, anſwers to an eighth of an inch in the bulk of the body; ſo that if the decreaſe be 128 grains, the ſolid will be half a cubic inch; and if but 64 grains, a quarter of a cubic inch: on the other ſide, if the decreaſe of the given body, exceed the ſtandard, 256 grains, twice, thrice, *&c.* the body will be equal to two, three, *&c.* cubical inches.

To gain the ſolidity of a body, hydroſtatically, tho' lighter than water.

'Tis eaſy, from the doctrine deliver'd, to diſcover, hydroſtatically, the ſolid contents of a body, heavier *in ſpecie* than water: but to meaſure, by the help of water, the ſolidity of a body lighter *in ſpecie* than that fluid, is a work of more difficulty. In order to it, we may conſider there are two ſorts of bodies, which will not, naturally, ſink in water; ſome, being of a cloſer texture, reſiſting it; and others abounding with pores, which diſpoſe them to imbibe the water.

Now the weight of a body of the firſt kind, may be gather'd from that of the water equal in magnitude to the immerſed part of the body, when it floats freely thereon. Thus, if a parallelopipid, or a cylinder of wood twelve inches long, placed upon water, ſhould reſt there when a twelfth part of it lies beneath the ſurface of the fluid; the weight of the water, equal in bulk to that immerſed twelfth part, would be equal in weight to the whole body of the wood. But becauſe the bodies, whoſe bulk phyſicians and chymiſts may have occaſion to examine, will very ſeldom happen to have regular figures; 'tis proper to add another method, more ſuitable to the preſent deſign. To meaſure then the ſolid contents of bodies ſpecifically lighter than water, but irregularly ſhaped*; the body muſt, firſt, be weigh'd in air. 2. A plate of lead, capable of ſinking this body in water, and of ſome round ſum in weight, ſhould be provided. 3. This plate being weigh'd in water, and its weight therein ſubſtracted from its weight in air, will give the weight of as much water, as is equal in bulk to the immerſed lead; or the ſpecific weight of the lead in water. 4. The plate of lead, and the lighter body, muſt be tied together with horſe-hair, and the weight of the aggregate noted. 5. This aggregate muſt be weigh'd in water, and its weight therein ſubſtracted from that it had in air; and the difference will give the ſpecific weight of the ſaid aggregate in water. 6. From this difference ſubſtract the ſpecific weight of the plate in water, and the remainder will give the weight of the lighter body in the ſame fluid. Then, laſtly, that weight of the light body in water being divided by 256 grains, will give its ſolid content.

To clear and confirm this method by an example; we took a conveniently ſhaped piece of oak, that weigh'd in air $193\frac{1}{2}$ grains; to this we tied, with an horſe-hair, a plate of lead, weighing juſt 240 grains. But before we tied them together, the lead was weigh'd in water, where it loſt of its former weight 20 grains; which, being deducted out of the 240 grains, left a difference of 20 grains, for the ſpecific weight of the lead in the water. Then the aggregate of the wood and lead was weigh'd, firſt in the air, and found to be $433\frac{1}{2}$ grains, and next in water, where it amounted but to 162 grains; which being ſubſtracted from the aggregate of the ſame bodies in air, the difference was $271\frac{1}{2}$ grains; from which, the other difference of 20 grains of the leaden-plate in water being deducted, there remain'd $251\frac{1}{2}$ grains, for the weight of a bulk of water equal to that of the given piece of wood. Now, if this number of grains had amounted to 256, we might have concluded the ſolidity of the body to be a cubic inch; ſince 256 grains of water, which we formerly found equal to a cubic inch of water, was alſo now found equal to the bulk of the given piece of wood. And having cauſed the wood I employ'd to be formed into a cubic inch; the difference of its weight in water from 256 grains, may, probably, be imputed to ſome little imperfection in the figure, or other like circumſtance. Firſt, then, the cube of oak in air, weigh'd 193 ½ grains. 2. The lead in air, 240 grains. 3. The lead in water, 220 grains; which, ſubſtracted from its weight in air, leaves for its ſpecific weight in water, 20 grains. 4. The aggregate of the two in air, 433 ½ grains. 5. The weight of both together in water, 162 grains; which, ſubſtracted from the ſum of their weight in air, gives the difference of the two ſeveral aggregates, 271 ½ grains. 6. The difference between the weight of the lead in air, and in water, 20 grains; which, ſubſtracted from the difference of the weights of the aggregates in air, and in water, gives for the weight of the propoſed cube 251 ½ grains.

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* There is a very eaſy method of obtaining the content, or cubic meaſure, of any ſolid, tho' ever ſo irregularly figured. For, ſince ſuch a body, immerſed in a fluid wherin it will not diſſolve, cauſes that fluid to riſe in direct proportion to the bulk immerſed; if the fluid be contain'd in a cylindrical or priſmatic veſſel, and the additional height given it by the body be marked on the veſſel; this will enable us to meaſure a part of the whole cylinder or priſm, equal in bulk to the irregular body. And thus may the ſolid content of ſtatues, carv'd or emboſs'd work, &c. be known.

This way of meaſuring bodies is appropriated to ſuch of them as will not readily diſſolve in water. But, becauſe there are many other ſolids, as ſalt, alum, vitriol, ſugar, *&c.* whoſe magnitudes it may often be proper to know and compare; I ſhall add, that the ſame method is applicable, alſo, to ſolids diſſoluble in water, if, inſtead of that fluid, be ſubſtituted oil of turpentine, whoſe proportion of ſpecific gravity to water is otherwiſe known. To diſcover this, I employ'd the hollow cubic inch of braſs, made uſe of to find the weight of a cubic inch of water; and found, that, when carefully filled, it contained 221 ⅛ grains of this oil: by which number, the difference of the weight of a ſolid in the air, and in that oil, being divided, the quotient will give the ſolid contents of the body to be examined.

Let us now conſider how ſuch bodies as, by their poroſity, are ſubject to imbibe too much of the liquor, while the experiment is in hand.

Merſennus's expedient, in this caſe, is to cover the body to be weigh'd in water, with wax, pitch, or ſome other glue, of a known ſpecific weight in water. But I prefer bees-wax for this purpoſe; and proceed with it in the following manner. The ſolid which is lighter than water, having been firſt weigh'd in air, over-lay it carefully with thin bees-wax; then take, alſo, in the air, the weight of the wax employ'd; and faſten to the body thus coated, a plate of lead or tin, heavy enough to ſink it; and obſerve the weight of the aggregate in water. This done, ſubſtract the weight of as much water as is equal in bulk to the wax, and proceed as is before taught.

But this method helps us only to the weight of the propoſed body in water; to diſcover its ſolid content, we muſt divide the weight of the ſolid in water, by 256 grains.

And, by the way, I have, ſometimes, made uſe of another expedient to hinder ſmall ſolids, whether lighter or heavier *in ſpecie* than water, from imbibing the liquor wherein they were weigh'd: for having firſt found the weight of a cubic inch of quick-ſilver; we placed the body to be meaſured, in a veſſel, whoſe ſolid contents were known before-hand; when, the ſpace that remain'd unpoſſeſſed by the firm body being fill'd with quick-ſilver, 'twas eaſy to know, by the difference in weight of that quick-ſilver, from the weight of the quick-ſilver requiſite to fill the whole veſſel, how much thereof was equal to the ſurrounded body. And, by this means, and the knowledge before gain'd of the weight of a cubical inch of mercury, the ſolid content of the body propoſed, was eaſily obtain'd.

What accuracy is to be expected in hydroſtatical experiments.

Before I put an end to this diſcourſe, 'tis proper to ſhew what credit may be given to the eſtimates of the weights and proportions of bodies obtain'd by hydroſtatical trials; becauſe mathematicians, either not knowing, or not applying our obſervation about the ſpecific gravity of rock-cryſtal, and the nature of oil of turpentine, have given us very different accounts of the proportions of metals, and a very few other familiar bodies, which are all they ſeem'd to have examin'd by this method.

And, indeed, I ſhould not be ſurprized to find, that the experiments of the ſame perſon, made at diſtant times, and under different circumſtances, diſagreed; for ſome difference there may be betwixt the waters employ'd in this caſe; eſpecially if the air be at one time intenſely hot, and at another exceeding cold. The difference, alſo, of degrees in goodneſs of the balances uſed in nice experiments, is not altogether inconſiderable. But a greater hindrance to the accuracy of hydroſtatical experiments, is the difficulty of finding an exact uniformity in weights of the ſame denomination, which are vulgarly ſuppoſed to be exactly equal. I have myſelf found it ſo difficult in practice to procure, and keep weights as exact as I deſired, that I have left off the hopes of it; for the very air may, in time, cauſe an alteration in them. And tho' the accurate *Ghetaldus*'s tables of the weight of metals, and ſome other bodies, in reſpect to one another, are look'd upon as the moſt authentic that have been publiſh'd; and are, accordingly, the moſt made uſe of; yet 'tis certain, the weights he employ'd are not divided as ours are. For tho', according to him, as well as with us, the ounce conſiſts of four and twenty ſcruples; yet the ſcruple, which with us is divided but into twenty grains, he divides into twenty-four. But tho' hydroſtatical experiments are not, always, either ſingly accurate, or exactly agreeable to one another; they prove accurate enough to be very uſeful in practice, and more exact than any other method, hitherto employ'd, of determining the proportions of bodies, in point of weight and bulk, and of meaſuring their ſolid contents; but, eſpecially, ſuch little ones as are neceſſary to be examined in the *Materia medica*. And this is a corollary from the whole of what we have deliver'd.

And, indeed, as little as my ſkill is in hydroſtatics, I would not be debarr'd from the uſe of it, for a very valuable conſideration; for it has already done me acceptable ſervice, and on a vaſt variety of occaſions; eſpecially in the examination of metal and mineral bodies, and of ſeveral chymical productions. I have often been able, by its means, to undeceive artiſts in their perſuaſion of poſſeſſing *Luna fixa*, and other valuable commodities; and to make a judgment, as to the genuineneſs or falſity, and as to the degrees of worth or ſtrength of many rich and poor metalline mixtures, and other bodies, both ſolid and fluid; whoſe fair appearances might, otherwiſe, have greatly deceived me.

And to ſhew ſome curious perſons how far hydroſtatics might be ſerviceable to as accurate menſurations as need be expected in phyſical experiments; I deſired a virtuoſo to mix tin and lead in a certain proportion, unknown to me; and melt them into one maſs; which I carefully weigh'd in water, and alſo examined it algebraically; and from hence aſſign'd the reſpective quantities of each; which agreed, within little more than a grain, with thoſe he had committed to paper, before he mixed them. And this ſmall difference, probably, proceeded from ſome ſcarce avoidable inaccuracies in melting and managing the given bodies.

To conclude; 'tis not to be expected, that the ſpecific gravities of the bodies mention'd in the following table, ſhall, all of them, be found, in future trials, preciſely the ſame as we there exhibit them. For, beſides that experiments are made by perſons of different qualifications, with different degrees of care, and with different inſtruments; * the varieties may proceed from a difference in the texture and compactneſs that may be found in ſeveral bodies of the ſame kind. For, neither nature, nor art, give to all the productions of the ſame name, a mathematical preciſeneſs, either in gravity, or other qualities.

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* Dr. *Jurin* recommends it, as a neceſſary caution, to all thoſe who ſhall attempt to weigh dry, porous ſolids in water, for philoſophical purpoſes, that, by ſome proper means, they firſt extricate the air out of all the ſmall pores and cavities of them; whereby the water may have free liberty to enter thereat: otherwiſe the air contain'd therein, by keeping the water out, will render the ſolid lighter in water than it really is. The beſt way of avoiding this inconvenience, the Doctor tells us, is to ſet the veſſel of water, wherein the ſolid is immerſed, under the receiver of an air-pump, and extract the air out of the body; which will ſucceed the better, if the water be firſt heated over the fire: or, in defect of an air-pump, he directs us to let the ſolid, if its texture will ſafely permit, continue for ſome time in boiling-water, over the fire. To a neglect of this obſervation, the Doctor thinks, among other things, is owing that difference found in the accounts of the ſpecific gravity of the *Calculi humani*. And when the air is thus extracted out of wood, as oak, fir, *&c.* and the roots, ſtalks, leaves, and ſeeds of vegetables, they prove ſpecifically heavier than water. *Philoſ. Tranſact.* Nº. 369. p. 223.

A TABLE of the ſpecific Gravities of Bodies compared with Water. *

	Weigh'd in Air.	In Water.		Proportion.	
AGate	251	156		2.64	
A piece of alum-ſtone	280¾	152¼		2.18	
Amber	306	12		1.4	
Antimony, good, and ſuppoſed *Hungarian*	391	295		4.7	
Bezoar ſtone	187	61		1.48	
A piece of the ſame	56½	22		1.64	
A fine oriental one	172	60		1.53	
Another	237	61		1.34	
Calculus humanus	2570	1080		1.72	
Another	302	97		1.47	
Calx of lead	138½	123		8.94	
Cinnabar, native	197	171		7.57	
Cinnabar, native, and very ſparkling	226	194		7.6	
Cinnabar common	802	702		8.1	
Cinnabar of antimony	197	169		7.3	
Coco-ſhell	331	85		1.34	
Copper-ore	1436	1090		4.15	
Copper-ore, rich	413	314		4.17	
Copper-ſtone	65½	49½	*Grains.*	4.09	to 1
Coral, white	336	204		2.54	
Another piece, fine	139	85		2.57	
Coral, red	129¼	80¼		2.63	
Cornelian	148	103		3.29	
Crabs eyes, native	77½	36½		1.89	
Crabs eyes, artificial	90½	54		2.48	
Cryſtal	256	140		2.21	
Gold-ore, not rich, but brought from the *Eaſt-Indies*	1100	682		2.63	
Another lump of the ſame	1151	717		2.61	
Granat, *Bohemian*				4.16	
Granati minera	217	147		3.1	
Ivory	173½	83		1.11	
Lapis Calaminaris	477	380		4.72	
—— *Hæmatites*, Engliſh	1574	1156		3.16	
—— *Judaicus*	261½	164		2.69	
—— *Lazuli*, one piece	385	256		2.78	
—— *Manati*	452	293		2.16	
A fragment of the ſame	218½	123		2.22	

Another

	Weigh'd in Air.	In Water.		Proportion.	
Another	345	197		2.33	
Another from *Jamaica*	2011	1127		2.27	
Lead-ore	686	590		7.14	
Another piece from *Cumberland*, rich	1872	1586½		·54	
Manganeſe, a piece	321	230		3.53	
Marcaſite	814	631		4.65	
Another from *Stalbridge*	243	189		4.5	
Another more ſhining than ordinary	287	227		4.78	
Mineral, *Corniſh*, like a ſhining marcaſite	145	129		9.6	
Ore of ſilver, choice from *Saxony*	458	366		4.22	
Another piece	1120	960		7	
Oſteocolla	195	108		2.24	
Rhinoceros's horn	8563	4260		1.99	
Rock-cryſtal	256	140	*Grains.*	2.20	to 1
Slat, *Iriſh*	779	467		2.49	
Sulphur vivum	371	185		2	
German, very fine	306	152		1.98	
Talc, a piece like *Lapis Amianthus*	596	334		2.28	
Talc *Venetian*	082	508		2.73	
Talc of *Jamaica*	1857	1238		3	
Tin-glaſs	468	419		9.55	
Tin-ore, *New-Engliſh*	812	613		4.8	
Tin-ore, black, rich	1293	984		4.18	
Another piece, choice	2893	2314		5	
Tutty, a ſingle piece	104	83		5	
Vitriol, *Engliſh*, a very fine piece	1093	512		1.88	
Vitrum Antimonii	357½	282½		4.76	
Unicorn's horn, a piece	407	195		1.91	

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* Later trials have furniſh'd us with the following table of the ſpecific gravities of ſolids and fluids.

A TABLE of the ſpecific Gravities of ſeveral ſolid and fluid Bodies.

Fine gold	19,640	Biſmuth		9,700
Standard-gold	18,888	Copper		9,000
Quick-ſilver	14,000	Caſt braſs		8,000
Lead	11,325	Steel, the ſame piece	ſoft	7,738
Fine Silver	11,091		hard	7,704
Standard-ſilver	10,535		ſpring temper	7,809

Iron	7,645
Tin	7,320
Glaſs of antimony	5,280
A pſeudo-topaz	4,270
A diamond	3,400
Clear cryſtal glaſs	3,150
Iſland cryſtal	2,720
Fine marble	2,700
Rock-cryſtal	2,650
Common green glaſs	2,620
Stone of a mean gravity	2,500
Sal gemmæ	2,143
Brick	2,000
Nitre,	1,900
Alabaſter	1,875
Dry ivory	1,825
Brimſtone	1,800
Dantzick vitriol	1,715
Alum	1,714
Borax	1,714
Calculus humanus	1,700
Oil of vitriol	1,700
Oil of tartar	1,550
Bezoar	1,500
Honey	1,450
Gum arabic	1,375
Spirit of nitre	1,315
Aqua fortis	1,300
Serum of human blood	1,190
Pitch	1,150
Spirit of ſalt	1,130
Spirit of urine	1,120
Human blood	1,040
Amber	1,040
Milk	1,030
Urine	1,030
Dry box-wood	1,030
Sea-water	1,030
Common-water	1,000
Camphire	0,996
Bees-wax	0,955
Linſeed oil	0,932
Dry oak	0,925
Oil olive	0,913
Spirit of turpentine	0,874
Rectified ſpirit of wine	0,866
Dry aſh	0,800
Dry maple	0,755
Dry elm	0,600
Dry fir	0,550
Cork	0,240
Air	0,001$\frac{1}{4}$

AN
Hydroſtatical Diſcourſe,

By way of

Anſwer to the Objections of Dr. MORE, *and others, againſt ſome Explanations of particular Experiments:*

With farther Conſiderations thereon.

SECT. I.

Mechanical ſolutions of phenomena, what?

FInding no cauſe to alter my judgment, as to the ſolutions I have given, of ſome of my experiments, attack'd by Dr. *More*; I here deſign to vindicate them from his objections.

And, firſt, the Doctor having made a deſcription of my pneumatic engine, pretends, that the aſcent of the ſucker, after it is depreſs'd, and clogg'd with a weight, is not mechanically accounted for, by the gravity and preſſure of the atmoſphere; becauſe, ſays he, "if this ſolution "were truly mechanical, he muſt have aſſign'd the true mechanical cauſe "of the gravity of all the parts, and of the whole atmoſphere."

To this I anſwer, that in delivering my experiments about the effects of the air, I did not intend to write a whole ſyſtem, or ſo much as the elements of natural philoſophy; but having ſufficiently prov'd, that the air we live in, is not deſtitute of weight, and has an elaſtic power; I endeavour'd, by thoſe two principles, to explain the phenomena exhibited in our engine; without recourſe to a *Fuga vacui*, an *Anima mundi*, or any ſuch unphiloſophical principle. And ſince ſuch kinds of explanations have, of late, generally been call'd, mechanical, as being grounded upon the laws of mechanics, I thought, as I permit, ſo I might be allow'd, the uſe of that term; and to entitle my explanations, mechanical, in the uſual ſenſe of that expreſſion. I am not obliged to treat of the cauſe of gravity in general, ſince many propoſitions of *Archimedes*, *Stevinus*, and others, who have

STATICS.

written of ſtatics, are confeſs'd to be mathematically, or mechanically demonſtrated; tho' thoſe authors do not aſſign the true cauſe of gravity, but take it for granted, as a thing univerſally acknowledg'd. And, if in each ſcale, of an ordinary balance, a pound weight, for inſtance, be put; he who ſhall ſay, that the ſcales hang in equilibrium, becauſe the equal weights balance one another; and, in caſe an ounce be added to one of the ſcales, and not to the oppoſite, he who ſhall ſay, that the former is depreſs'd, becauſe urg'd by a greater weight than the other, will be thought to have given a mechanical explanation of the equilibrium of the ſcales, and their loſing it; tho' he cannot give a true cauſe why either of thoſe ſcales tends towards the center of the earth. Since, then, to aſſign the true cauſe of gravity, is not required, even in ſtatics, tho' one of the principal, and moſt known parts of mechanics; why may not other propoſitions, and accounts, that ſuppoſe gravity in the air, and prove it too, be look'd on as mechanical?

The Doctor, however, is pleas'd to grant me almoſt as much as I need deſire, as to the truth of the hypotheſis whereon my explanations are founded. The principal thing which I ſuppoſe, in my hydroſtatical writings is, that in water, tho' ſtagnant, the upper parts actually gravitate upon the lower; or preſs upon them, even when they do not ſenſibly depreſs them. This hypotheſis, the Doctor allows, agreeable to the principles of the mechanical philoſophy: and, accordingly, having ſhew'd, that in a ſuſpended tube of water, the whole liquor gravitates upon the bottom of it; and, conſequently, that all the parts thereof do ſo, the upper upon the lower, " provided" ſays he" " there be no immaterial principle in nature."

Now, I as freely, as the Doctor himſelf, aſſert an incorporeal Being, that made and governs the world. All that I have endeavour'd at, in explaining what happens among inanimate bodies, is to ſhew, that ſuppoſing the world to have been at firſt made, and to be continually preſerv'd by God's divine power and wiſdom; and ſuppoſing his general concourſe to the maintenance of the laws he has eſtabliſh'd in it; the phenomena I conſider, may be ſolv'd mechanically; that is, by the mechanical properties of matter; without recourſe to nature's abhorrence of a vacuum, to ſubſtantial forms, or to other incorporeal creatures. And, therefore, if I have ſhewn, that the phenomena, I attempted to ſolve, are explicable by the motion, magnitude, gravity, ſhape, and other mechanical affections of the ſmall parts of liquors; I have done what I pretended: which was not to prove, that no angel, or other immaterial creature, could interpoſe in theſe caſes; for, concerning ſuch agents, all that I need ſay, is, that, in the caſes propoſed, we want not their aſſiſtance; and, therefore, have no occaſion to fly to it, in ſolving our phenomena.

That the upper parts of fluids gravitate upon the lower.

But the Doctor, it ſeems, would have the gravitation of the elements, in their proper places, ſuſpended by an incorporeal principle; and hereto he is led by this experiment, which, he ſays, is moſt manifeſtly repugnant to our hypotheſis. He conceives, then, that in a bucket of water, with a perfectly cylindrical cavity, whoſe diameter is of 62 parts, there is forcibly kept at the bottom, by means of a ſtick, a round piece of wood, whoſe diameter amounts but to 61 of thoſe parts; and that as ſoon as ever the ſtick is remov'd, the piece of wood will emerge to the top, and float: "which," ſays he "is impoſſible, if all the parts of the water, did not "only jointly preſs the bottom of the veſſel, but each preſs'd the other di- "rectly downwards."

But, firſt, ſince according to his computation, the area of the interval between the ſides of the veſſel, and the edges of the round board, is 123 of ſuch parts, whereof the area of the board amounts to 3721; 'tis evident, that there muſt be room enough for the water to paſs between the ſides of the veſſel, and the edges of the board; which is ſuppoſed, on all hands, to be of ſome wood lighter *in ſpecie* than water, ſince elſe it would not riſe, upon with-drawing the ſtick.

Secondly, this round board is not ſuppos'd to be made exactly fit to the bottom of the veſſel, and, conſequently, the water may get in between them; for which reaſon, 'tis neceſſary to keep the piece of wood forcibly down with a ſtick; which were otherwiſe needleſs; and, conſequently, this interpos'd water, will communicate with the upper, along the ſides of the veſſel; which latter may, according to the laws of hydroſtatics, by means of that interpoſed, exerciſe its preſſure upwards, againſt the lower ſurface of the wooden plate.

Thirdly, the Doctor ſuppoſes an imaginary plane of water to be parallel to the bottom of the veſſel, and to paſs along the bottom of the board; ſo that of the water that lies between this plane, and the bottom of the veſſel, one part is cover'd by the piece of wood; and the other, between the edges of that, and the ſides of the bucket, is cover'd with the incumbent water only.

Now, 'tis manifeſt, that in water, thoſe parts which are moſt preſs'd, will thruſt out of their place, thoſe that are leſs preſs'd. 'Tis alſo evident, that the part of the imaginary plane, cover'd by the round piece of wood, muſt be preſs'd by a leſs weight than the other part of the ſame plane; becauſe the wood being, bulk for bulk, lighter than water, the aggregate of the wood and water, incumbent on the cover'd part of the ſame plane; muſt be lighter *in ſpecie*, than the water alone, that is incumbent on the uncover'd part of the ſame plane; and, conſequently, this uncover'd part being more preſs'd than the other part of the plane, the heavier muſt diſplace the lighter; which it cannot do, but by thruſting up the board; as it does when the external force that kept it down, is removed. And this greater preſſure againſt the bottom, than againſt the top of bodies immers'd in water, ſpecifically heavier than themſelves, is a true reaſon of their emerſion.

'Tis true, that according to the Doctor's computation, if the ſolid cylinder, conſiſting of the wooden plate, and all the water directly incumbent on it, were put into an ordinary balance; it would there greatly outweigh the hollow cylinder of water alone, that reſts upon the uncover'd part of the imaginary plane. And this is what ſeems to have deceiv'd the

Doctor. But there are many hydroſtatical caſes, wherein the phenomenon depends not ſo much upon the abſolute weight of the compared bodies, as upon their reſpective and ſpecific gravity; on account whereof, a ſmall pebble, for inſtance, that weighs not a quarter of an ounce, will readily ſink to the bottom of a river, on whoſe ſurface a log of a hundred pound weight will float. 'Tis a rule in hydroſtatics, that when two portions of water, or any other homogeneous liquor, preſs againſt each other; the prevalency will go, not according to the abſolute weight, but the perpendicular height of thoſe portions. And, accordingly, we find, that if a ſlender pipe of glaſs, being fill'd with water, have its lower orifice unſtopp'd at the bottom of a veſſel of water, which contains much more of that liquor than the pipe; yet if the water in the tube were, for inſtance, two feet high, and that in the veſſel but one; the water in the pipe will readily ſubſide, till it comes almoſt to a level with the external water; tho' it cannot do ſo, without raiſing the whole maſs of water ſtagnant in the veſſel.

Demonſtrated by experiments.

We took an open-mouth'd glaſs, or jar, three inches and a half in diameter, and ſomewhat leſs in depth, its cavity being cylindrical; into this, having put ſome water to cover the protuberance that is left at the bottom of ſuch glaſſes, we took a convenient quantity of bees-wax, and having juſt melted it, we pour'd it cautiouſly into the glaſs, warm'd before-hand to prevent its cracking, till it reach'd to a convenient height. This veſſel, and the contain'd liquors, we ſet aſide to cool, that the wax might ſhrink from the glaſs, and, conſequently, have a little interval every where between the concave ſuperficies of the veſſel, and convex of the hardned wax; when, carefully pouring ſome water between the glaſs and the wax, ſo that it fill'd all the interval left between thoſe two bodies, both at the bottom and the ſides, the wax was made preſently to float; being viſibly rais'd up from the bottom, and its upper part appearing a little above the level of the water, as it ought, according to the true principles of hydroſtatics. For water, being ſomewhat heavier, *in ſpecie*, than wax, and that which was poured into the bottom, and ſtagnated there, being preſs'd by the collateral water, every way interpos'd between the concave part of the glaſs, and convex of the wax; (ſo that this collateral liquor anſwer'd what I call a hollow cylinder of water, in the Doctor's experiment) that part of the ſtagnant water which was preſs'd upon by the wax, being leſs preſs'd than the other part of the ſame ſtagnant water was by the water incumbent on it; this latter muſt diſplace the former; which it could not do, but by raiſing up the wax that reſted upon it; yet this collateral water was ſo far from being heavier than the wax impell'd up by its preſſure, that both the collateral and the ſtagnant water together, being weigh'd, amounted to little above a fourth of the weight of the wax; which happen'd by reaſon of the narrowneſs of the veſſel: but if it had been wider, the experiment, I doubt not, would have ſucceeded, tho' the wax had out-weigh'd the collateral water ten times more than in our experiment it did. But, that the ſolid body exceeded almoſt four times the weight, not only of the collateral, but the ſtagnant liquor too, ſufficiently overthrows the Doctor's reaſoning; the fallacy whereof will further appear from hence, that tho' we gradually pour'd in water, as long as the veſſel would contain any; the cylinder of wax was, indeed, lifted higher and higher from the bottom of the glaſs, but did not appear rais'd more than at the firſt, above the upper ſurface of the water; which argues, that it was not at all the quantity of the lower water that continually increaſed; but the preſſure of the collateral water, which continued ſtill at the ſame height, with reſpect to the wax, that caus'd the elevation of the body.

And, to manifeſt yet more clearly the Doctor's miſtake, I deviſed the following experiment. We took a round plate of lead, about the thickneſs of a ſhilling; and having ſtuck it faſt to the bottom of the cylinder of wax, we ſucceſſively placed upon the upper part of the wax, ſeveral grain-weights, till the wax ſubſided to the bottom: by this means, the glaſs being, at firſt, almoſt fill'd with water, there ſwam about an inch of that liquor above the upper ſurface of the wax. And, laſtly, we took off, by degrees, the grain-weights that we had added, till we ſaw the wax, notwithſtanding the adhering lead, riſe, by degrees, to the top of the water; above which, ſome part of it was viſibly extant.

From this experiment I argue, that, according to the Doctor's ſuppoſition, here was incumbent on the wax, a cylinder of an inch in height, and of the ſame diameter, or breadth, with the round ſurface of the wax; but, upon the removing part of the water that lay at the bottom, when the wax began to riſe, there was incumbent no greater a weight than that of the collateral water; and as much of the upper and ſtagnant, as was directly incumbent upon that collateral water. But now, according to the Doctor's reaſoning, this cylinder of water incumbent on the wax, being an inch deep, and above three inches broad, it muſt preſs the wax with a far greater weight than that which the lateral, and hollow cylinder of this ſtagnant water, could exert upon the reſt of the collateral water; yet the height of this aggregate of collateral water, being the ſame with that of the wax, and the water ſwimming upon it, the difference of the preſſure was ſo ſmall, that, barely taking off a weight of four or five grains, the wax would, notwithſtanding the preſſure of the water incumbent on it, be impell'd up, and made to float; and, by the like weight put on again, it would be made to ſink; and, by another removal of ſuch a weight, it would, tho' ſlowly, re-aſcend. And this phenomenon depends ſo much upon a mechanical balance of preſſure, that even four grains would not have been neceſſary to make the wax riſe or ſink, were it not for ſome little accidental impediments, that are eaſily met with in ſuch narrow glaſſes; for, otherwiſe, in a larger veſſel, we have made the ſame lump of wax readily ſink, or float, by putting on, or taking off, a ſingle grain, or leſs.

Hence it appears, that, for the regulation of hydroſtatical phenomena, nature has her balance too, as well as art; and that, in the balance of nature, the ſtatical laws are nicely obſerv'd.

And ſtill further to demonſtrate, that in ſtagnant water, the upper parts gravitate upon the lower; we took a very ſlender pipe of glaſs, whoſe cavity was narrower than that of an ordinary gooſe-quill, that heterogeneous liquors might not be able to get by one another in it. This pipe, near one end, was bent upwards, like a ſyphon, that it might have a ſhort leg parallel to the longer; into this crooked pipe we put a little oil, and then held it perpendicularly in a deep, wide-mouth'd glaſs, fill'd with water, and a lump of wax, of the bigneſs and ſhape of that before-mention'd; ſo that the preſſure of the incumbent water upon the open orifice of the ſhorter leg, might impel the oil into the longer, above the ſurface of the water in the veſſel. The pipe being thus held upright, 'twas eaſy to take notice, by a mark fixed on the outſide, to what height the oil reach'd in it.

Now, if we conceive a horizontal plain, parallel to the bottom of the veſſel, to paſs by the baſis of the floating wax; 'tis evident, that, of this imaginary plane, the part on which the wax reſts, is as ſtrongly preſs'd by the weight of the wax, as the lateral part of the ſame plane is by the weight of the water incumbent on it; and, conſequently, that part of this plane, which is placed directly over the orifice of the ſhorter leg of the pipe, is no more preſs'd, than any equal portion of that part of the ſame plane cover'd by the wax. This body, being taken out of the water, the liquor ſubſided a great way in the veſſel; and ſo did, proportionably, the oil in the longer leg of the pipe. And laſtly, having weigh'd out as much water as we found the wax to amount to: this liquor was, inſtead of the wax, pour'd into that which remain'd in the glaſs: whereupon, the oil in the longer leg of the pipe, was again impell'd up to the former mark, to which the wax had rais'd it. Whence we may gather, that the water newly put in, tho', in the air, it weigh'd no more than the wax did, yet preſs'd the water that lay beneath the fore-ſaid imaginary plane; and, conſequently, that which was directly over the ſhorter leg of the pipe, as much as the wax before had done. And, ſince we have already prov'd, that the wax conſiderably preſs'd that plane; it cannot be deny'd, that the water did, in like manner, preſs that plane: and, conſequently, that water may gravitate in water, as well as a ſolid body, ſuch as is wax.

But, to rectify that plauſible miſtake, which has long deluded both philoſophers and mathematicians, who think a body does not actually gravitate, when it does not deſcend; we have ſeen, that the immerſed wax, and the braſs-grains which lie on it, actually preſs, or gravitate, upon the ſubjacent water, and bottom of the veſſel, on which 'tis incumbent; and, conſequently, its preſſure, not being ſurmounted by that of the collateral water, which is unable to raiſe it, muſt be as great as that of this collateral water. Therefore, when, upon the removal of a ſingle grain, the wax, with its incumbent weight, is made to aſcend, and that but very ſlowly; 'tis evident, that 'twas ſo far from not gravitating before, becauſe it did not actually deſcend, that it retain'd its gravity even whilſt it aſcended; as may appear, not only by the ſlowneſs of its motion upwards, proceeding from its being in nature's ſcale very little leſs heavy than it need be, to balance the preſſure of the collateral water; but by this alſo, that if but a ſingle grain be laid on, when it begins to riſe, its aſcent will be check'd, and hinder'd; which could not be done by the addition of ſo inconſiderable a weight, if the wax, and the adhering metal, did not, even during their aſcent, retain their former gravity; tho' that were fruſtrated as to the act of deſcending, or ſo much as keeping their ſtation, by the prevailing preſſure of the collateral water. So that, ſince the wax, and adhering metal amounted to conſiderably above four thouſand grains; it did, in the balance of nature, weigh, whilſt aſcending, not ſo much as a four thouſandth part leſs than it did, whilſt it was actually deſcending.

Upon the whole, then, Dr. *More*, had he throughly conſider'd the matter, needed not have concluded the account of his experiment, as he does, by ſaying, " this is ſo evident a demonſtration againſt the gravity of the " parts of water downwards; that unleſs it prove true, I ſhall never aſ-" ſent to any reaſoning of my own, for the future, nor to that of any other " man whomſoever." But, I hope, he will conſider, as well as I, that a man may be very happy in other parts of learning, who has had the miſfortune to miſtake in hydroſtatics; a ſcience which very few ſcholars have been at all vers'd in.

Water made to ſupport a body of a much greater ſpecific gravity than itſelf.

As for the laſt experiment in my hydroſtatical paradoxes, the Doctor ſeems to ſuſpect the matter of fact; or, ſuppoſing it true, accounts for it from his hylarchic principle, and the interpoſition of the valve of the inſtrument; but what conſiderable intereſt the imaginary retraction of the valve, or the air itſelf, can have in this phenomenon, I confeſs I do not diſcern: for, I think, the experiment would ſucceed, when try'd *in vacuo*, tho' all the atmoſpherical air were annihilated.

However, to avoid all miſtakes and diſputes, that may ariſe upon account of the valve employ'd in our experiment; a quantity of quick-ſilver, being by ſuction rais'd into a very ſlender glaſs-pipe, whoſe upper orifice was ſtopp'd with the finger, to keep the mercury from falling out; we thruſt the open end of the pipe, with the mercury in it, into a deep glaſs of water, till the little cylinder of mercury had, beneath the ſurface of the water, attain'd to a depth, at leaſt fourteen times as great as the height of the mercurial cylinder; when, the finger being removed from the upper orifice, the glaſs-pipe will be open at both ends, and nothing can hinder the quick-ſilver from falling to the bottom, but the reſiſtance of the cylinder of water that is under it; which cylinder can reſiſt but by virtue of the weight, or preſſure of the ſtagnant water above it, tho' but collaterally placed: yet this water being, by the pipe, whoſe upper part is higher than the ſurface of that, and acceſſible only to the air, kept from preſſing againſt the mercury any where but at the bottom of the pipe; and being about a fourteenth part of the weight of an equal bulk of mercury; it is able, at that depth, to make the ſubjacent water preſs upward againſt the mercury, which is but a fourteenth part as high as the water is deep, with a force equal to that of the gravity, wherewith the mercury tends downwards. And, to manifeſt that this phenomenon depends merely upon the equilibrium of the two liquors; if you gently raiſe the lower end of the pipe towards the ſurface of the water, this liquor, being not then able to exerciſe ſuch a preſſure as it could at a greater depth, the mercury preponderating, will fall out to the bottom of the tube. But if, when the quick-ſilver is at the firſt depth, inſtead of raiſing the pipe, you thruſt it down farther under the water; the preſſure of that liquor againſt the mercury, increaſing with its depth, will not only ſuſtain the mercury, but impel it up in the pipe to a conſiderable diſtance from the lower orifice, and keep it about the ſame diſtance from the ſurface of the water, which is laterally above it. And this experiment may not only ſerve for the purpoſe for which I here alledge it; but, alſo, if duly conſider'd, and apply'd, very much illuſtrate and confirm the explanation, formerly given, of the ſeemingly ſpontaneous aſcent of the clogg'd ſucker in our exhauſted air-pump.

The caſe of divers, with regard to the preſſure of the water they ſuſtain, at great depths.

The laſt argument which the Doctor urges againſt the gravitation of water, in its proper place, as they ſpeak, is deduced from what happens to divers, who, in the midſt of the ſea, tho' ſalt-water be much heavier than freſh, do not find themſelves compreſſed by the vaſt load of the incumbent water.

But if obſervations about diving were made by philoſophers, and mathematicians, or, at leaſt, by intelligent men; we ſhould, I do not doubt, have accounts of it, very different from the current reports. A learned phyſician, of my acquaintance, upon diving leiſurely, perceived a conſtriction of his thorax, by the action of the ſurrounding ſea-water.

A *Spaniſh* prelate, who liv'd long in *America*, ſpeaking of the *Indians*, employ'd by their inhuman maſters in fiſhing for pearls, tells us, "it is "impoſſible that men ſhould live for any long ſeaſon under the water, "without taking breath; ſo that they die commonly thro' vomiting blood, "and of the bloody flux." And a general of the *Engliſh* in the *Eaſt-Indies*, ſent on an embaſſy to the emperor of *Japan*, ſpeaks thus of ſome female-divers he met with in his voyage. "All along the coaſt, and ſo up to *Ozaca*," ſays he, "we found women-divers, who lived with their houſhold and fa-"mily, in boats upon the water. Theſe women," he ſays, "would "catch fiſh by diving, which they miſſed by net and line, and that in eight "fathoms depth. Their eyes, by continual diving, grow as red as blood; "whereby a diving woman is diſtinguiſhed from all others." However, the reaſon why ſo little damage is ſuſtain'd by diving, ſeems, in my opinion, owing to the uniform preſſure of the ſurrounding fluid, and the robuſt texture of a human body*.

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* 'Tis remarked by Sir *Iſaac Newton*, that ſince fluids, in preſſing bodies immerſed in them, do not change the external figures thereof, they will not change the ſituation of the internal parts among one another; and, therefore, that if ſenſation proceeds from the motion of the parts of animals; fluids will not damage the animals immerſed therein, nor excite any ſenſation in them, farther than as their bodies

Vaſt preſſure of a fluid ſuſtain'd by weak and tender bodies.

The ſwims of ſmaller fiſh appear very unable to reſiſt compreſſure, being much more thin and delicate to the eye, than a piece of fine *Venice* paper; yet having cauſed one of theſe bladders, above an inch in length, and proportionably wide, to be taken out of a roach, and anointed with oil, to keep it ſupple, and preſerve it from being pierced, or ſoften'd, by the water; and having, by a weight of lead, faſten'd to the neck of it, let it down to the bottom of a cylindrical tube of water, ſeal'd at one end, and made large, and about fifty-ſix inches long; we could not perceive that, by the weight of all the incumbent water, it was manifeſtly compreſs'd, or that it diſcover'd the leaſt wrinkle, or other depreſſion of that very thin membrane, tho' fill'd but with air. And this trial was made more than once with the ſame ſucceſs. Yet that this proceeded rather from the ſtrength of the bladder, that was able to reſiſt the weight of a taller pillar of water, than from the levity of water in the upper part of the tube on that in the lower, we ſhew'd, by preſently letting down a mercurial gage, by a ſtring, to the bottom of a tube: for the weight of the incumbent water forced up ſome of the mercury out of the open leg of the ſyphon into the ſeal'd one; and, conſequently, compreſs'd the air included there: and the uncompreſſed air, being three inches and $\frac{5}{8}$ in length, we judged it, at the bottom of the tube, to be about $\frac{5}{8}$ impell'd up by the intruſion of the mercury. And, to ſatisfy myſelf, and others, that if the incumbent water had been heavy enough, it would, viſibly, have affected the bladder, in ſpite of any *Principium hylarchicum*; we ſunk it in a cryſtal glaſs, that had a long cylindrical neck, and was exactly fitted with a ſtopple; then a competent quantity of air being left above the water, the ſtopple was warily, and by degrees, thruſt down; and ſo, leſſening the capacity of the glaſs, compreſſed the air that was next it, and, by the intervention of that, the water under it. And tho' there did not, upon a ſlight compreſſion of the outward air, appear any ſenſible effect upon the bladder that was at the bottom of the water; yet, upon a farther intruſion of the ſtopple, the preſſure being increaſed, the immerſed bladder diſcover'd two conſiderably deep wrinkles, that preſently diſappear'd upon drawing up the ſtopple; which, being thruſt in again, the depreſſions were again to be ſeen on the ſwim. And, having convey'd a mercurial gage into the ſame glaſs, we eſtimated, by the condenſation of the air in the ſeal'd leg of it, that the bladder had been expoſed to a preſſure equal to that of a column of about forty feet of water.

This may leſſen our wonder, that bodies, of ſo firm a texture as thoſe of luſty men, ſupport the preſſure of the water at ſuch depths as divers uſually remain; ſince we ſee what reſiſtance can be made by ſo exceeding

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bodies may be condenſed by compreſſure. *Newton. Princip.* p. 264, 265. And *Borelli* having ſhewn, that a quantity of ſand, contain'd in a very hard or rigid veſſel, cannot poſſibly be divided, or entred, by a wedge; and that water, in like manner, contained in a bladder, every way equally compreſſed, can neither be contracted, bent, or otherwiſe diſtorted; hence deduces the reaſon why divers are not ſenſible of the preſſure of the water. *De motib. nat. à gravitats factie.* Prop. 29. 34.

thin and delicate a membrane, diſtended only with the air, in compariſon of the ſtrong membranes and fibres of a man, fill'd, beſides air, with more firm parts. 'Tis alſo conſiderable, what great weights may be ſuſtain'd in the air, by ſuch fibres or tendons, and by other fibres interwoven into membranes, in compariſon of what an ordinary man would expect. And, not only upon account of the ſtable parts of a human body, but of the ſpirits too, it may reſiſt very violent preſſures of a fluid, without any manifeſt contuſion, or diſlocation of parts, or even ſenſe of pain; as appears from the great effects which guſts of wind have upon trees, houſes, &c. tho' a man will withſtand the impetuoſity of ſuch a ſtrong wind, and walk directly againſt it, by virtue of the vigour of his muſcles and ſpirits, without being thrown down, or bruiſed, by ſo violent a current of air, and without ſo much as complaining of pain; and this, tho' the wind that beats againſt him, acts as a ſtream, and does not uniformly compreſs him, but invade only the fore-part of his body. Thus, alſo, in the lifting up of heavy weights by luſty men, we may ſee the ſlender tendons of the hand loaded with an hundred and fifty pounds, or more, without having their fibres ſo far compreſſed, or ſtretched, as to make the perſon complain of pain. A human body, therefore, is an engine of a much firmer ſtructure, than mere ſcholars uſually take notice of. And, I doubt, whether, if the ſtructure of a man were not conſiderably firm, he could, eſpecially in a deep ſea, ſupport the preſſure of the water, tho' not immediately apply'd, without pain. For, having, ſeveral times, convers'd with a man who got his living by diving for ſhip-wreck'd goods; he aſſured me, that, when he ſtay'd at a conſiderable depth, as ten or twelve fathoms, under the ſurface of the ſea, he felt a great pain in both his ears, which often put him to ſhifts to leſſen it; and this, by his manner of deſcribing it, I concluded, aroſe from the incompetent reſiſtance of the air, which he found, by manifeſt tokens, to be greatly compreſs'd by the upper water. This relation, from ſuch a perſon, not only confirms our explanation, but likewiſe warrants us to doubt whether the common reports that are made concerning divers, ought to be rely'd on, without further examination.

We took a common fleſh-fly, of a middle ſize, and having put it into the ſhorter leg of a bent glaſs, which we cauſed to be hermetically ſealed, we added as much mercury as fill'd that leg, and a part of the other; leaving little more than an inch of air between the quick-ſilver, and the ſeal'd end, that there might be room both for the fly, and the condenſation of the air; and then, with a little rammer, fitted for the purpoſe, we cauſed the mercury, in the open leg, to be thruſt againſt that in the ſeal'd one; which neceſſarily crowded the air, near the fly, into leſs room; ſo that it ſeem'd condens'd into about a third part of the ſpace which it poſſeſs'd before, and which it regain'd, when the rammer was withdrawn: and tho' this were done more than once, yet the fly appear'd not ſenſibly hurt; and I perceiv'd her, whilſt ſhe was pent up, to move her legs, and to rub them one againſt the other, as 'tis uſual with that ſort of inſects in the free air.

Another experiment, to the ſame purpoſe, we made with water; tho' this wet the wings of the fly, and ſoon after, by miſchance, drown'd it: but we had, firſt, an opportunity to compreſs the air into a third, if not a fourth part, of its former dimenſions; yet the fly continu'd to move ſeveral of her parts, and eſpecially her legs, very vigorouſly; as if nothing troubled her, but being, as it were, glued to the inſide of the glaſs by her wetted wings. And this, I hope, will keep the reſiſtance of divers, to the ſurrounding water, from ſeeming incredible; ſince ſuch flies were able to reſiſt, and, for ought appear'd without harm or pain, the preſſure of the crowded particles of the air: tho' we gueſs'd it to have been as much compreſs'd by the force of the rammer, as it would have been by a cylinder of water, of between 50 and 60 feet high.

Hence, too, we may be aſſiſted to conceive, how great a difference there is, whether the ſame preſſure be exerciſed by a ſolid, or by a fluid body. For, according to our eſtimate, the preſſure againſt the body of the fly was as great, as if a ſlender pillar of marble, having the fly for its baſe, and 18 or 20 feet in height, had reſted upon the little animal.

We alſo took ſome ordinary black flies, of a middle ſize; and having placed one of them, with the head upwards, and left ſome diſtance betwixt her and the ſeal'd end of the glaſs tube, we pour'd in quick-ſilver very ſlowly, and cautiouſly, leſt the force of ſo heavy a body, acquired by its velocity in the fall, ſhou'd, more than the mere weight of the fluid, oppreſs her: at length we got in as much mercury as the tube would receive; and then holding it upright, we watch'd whether the fly would make any motions; and finding that ſhe did manifeſtly ſtir, notwithſtanding the incumbent mercury, we meaſur'd the height of the mercurial pillar, reaching from the middle of her body, to the top of the fluid, and found it to be about eight inches; and the quick-ſilver being pour'd out, the fly appear'd very lively, and vigorous.

We repeated the experiment with one of the beſt flies we could take, when their ſeaſon was almoſt over, and of the ſame ſize with the former; when, ordering the matter ſo, that the mercury incumbent on her, appear'd to be of a greater height than that of the tube before employ'd, we ſaw her move one, or other of her legs, ſeveral times, tho' the tube was held upright; and therefore meaſuring the height of the mercury above her, we found it to amount to above ſixteen inches; then freeing her from this preſſure, we obſerv'd, that ſhe immediately found her legs again, and moved up and down, briſkly: tho' when ſhe was afterwards oppreſs'd, with 23 or 24 inches of the ſame quick-ſilver, ſhe gave no ſigns of life. But having got another fly, of about the ſame bigneſs; tho', when ſhe was at the bottom of the quick-ſilver, ſhe ſeem'd ſo compreſs'd as not to have any motion, yet, upon being taken out of the glaſs, ſhe preſently diſplay'd her wings; tho' the pillar of mercury, that preſs'd upon her, amounted to above 27 inches.

STATICS.

Another fly, that ſeem'd but about half ſo big, as one of thoſe hitherto mention'd, being well placed, with ſome mercury under her, in a glaſs pipe, held upright, ſuſtain'd a mercurial pillar of 25 inches; and tho' ſhe was not obſerv'd to move under ſo great a weight, yet, when once that was taken off, ſhe appear'd unhurt; and, probably, would have eſcap'd under a much greater weight, if the tube, which was too large, had not already employ'd all the ſtock of mercury we then had at hand. Since, then, ſo ſmall an animal, as a fly, may ſurvive ſo great a preſſure, and remain able to move ſuch long and ſlender bodies, as her legs, when preſs'd againſt by above 16 inches of mercury; and, conſequently, by a weight equivalent to a pillar of water, of more than 18 feet and a half; which being 590 times her own length, and many times more her height, it appears, that a diver, ſix foot tall, to have as many times his height of water above him, as our fly might have had, and yet continue to have moved under it, muſt dive to near an hundred fathoms; which is a depth vaſtly greater than, from what I could learn by enquiry, the divers, either for coral or pearl, deſcend to.

Thus, then, we have accounted for a phenomenon, which Dr. *More* thinks inſoluble, without his hylarchic principle. There would, indeed, be much more weight in what he objects, if our aſſertion, of the gravitation of water in water, were, like the *Principium hylarchicum*, a mere hypotheſis, advanced without any poſitive proof: but our doctrine is directly proved, by particular experiments; to elude the force whereof, ſo ingenious a perſon, is obliged to call in a principle that is not phyſical.

An actual preſſure at the bottom of the ſea.

But, whatever power he is pleas'd to ſuppoſe at the bottom of the ſea, to ſuſpend the preſſure of the incumbent water, I think, that ſuppoſition muſt give place to experience; which ſhews, there really is a great preſſure exercis'd by the water at the bottom of the ſea. A gentleman, who has been often preſident of the royal ſociety, aſſured me, that a friend of his, having let down a pewter bottle into a deep ſea, with weight enough to ſink it, that he might try whether any ſweet water would ſtrain into it; found, when he pull'd it up again, the ſides of it very much compreſs'd, and, as 'twere, ſqueez'd inwards by the water. An acquaintance of mine, trying to cool his liquor, when he ſail'd thro' the torrid zone, by letting the containing veſſels to a great depth into the ſea, was, at firſt, amaz'd to find the corks, with which the ſtrong ſtone bottles had been well ſtopp'd before, ſo forcibly, and ſo far thruſt in, that they could ſcarce have been ſo violently beaten in with a hammer. And an ingenious perſon who practiſes phyſic in the *Indies*, told me, he try'd, in a very deep part of the ſea, whether any freſh water would ſtrain into ſtone bottles, thro' a thick cork, ſtrongly ſtopp'd in; and having let it down with a convenient weight to 100 fathoms, was much diſappointed, when he drew it up, by finding that the preſſure of the water, at ſo vaſt a depth, had quite thruſt down the cork into the cavity of the bottle: an effect which he ſcarce could have expected from the ſtroke of a mallet.

STATICS.

And, to ſhew, *ad oculum*, that water may gradually, as it grows deeper, preſs againſt the ſtopple of a bottle, tho' the veſſel be inverted; we took a glaſs vial, furniſhed with a cylindrical neck, and its cavity large, in proportion; into this we put as much quick-ſilver as would, in the neck, make a ſhort mercurial pillar, of between half an inch, and an inch; then a piece of very fine bladder, dipped in oil, was ſo ty'd over the orifice of the glaſs, that no mercury could fall down, or get out, nor water get in at the orifice; and yet the bladder, by reaſon of its great limberneſs, might be eaſily thruſt up towards the cavity of the vial, or depreſs'd by the weight of the mercury. This little inſtrument, firſt furniſh'd with a weight of lead, to ſink it, being inverted, the mercury deſcended into the neck, and cloſed the orifice as exactly as a ſtopple; but, with its lower part, depreſs'd the bladder beneath the horizontal plane, that might be conceiv'd to paſs by the orifice: then the glaſs, being, for a while, kept in the water, and, by a ſtring, let further down into the ſame glaſs veſſel, fill'd to about two feet in height; the preſſure of the liquor againſt the orifice of the vial, by degrees drove up the bladder, and the mercurial ſtopple into the cavity of the neck; as was manifeſt by the aſcent of the quick-ſilver: and when the inſtrument was leiſurely drawn up again, the weight of this mercury made it ſubſide, and plump up the bladder, as before.

Meeting, caſually, with an ingenious mechanic, who deviſed proper accommodations, and a boat, wherewith he could continue, for a great while, at a conſiderable depth under water; he aſſured me, that when he was about four or five yards deep in the river *Thames*, his breaſt and abdomen were ſo compreſſed, that, there being hardly room enough left for the free motion of his lungs, he could ſcarce fetch his breath; and was obliged to be drawn quickly up: and that, to remedy this, he cauſed a kind of armour for the cheſt and back, to be made of copper; and tho' the metal defended him from any miſchief in thoſe parts, yet, in others, where only leather was interpoſed, when he came to the depth of about ſix fathoms, he found a great preſſure upon his legs and arms, and all the other parts, againſt which the water was able to thruſt the leathern ſuit inwards: and this preſſure he found pretty equal; ſo as to receive no great inconvenience from it; being able to continue under water, tho' not at any great depth, for about two hours. He farther declar'd, from his own experience, that the ambient water endeavour'd to preſs him, and his diving ſuit, every way inwards. I have alſo been aſſured, that a profeſs'd diver, when he deſcended in his bell, to very great depths, has often had the blood ſqueez'd out at his noſe and eyes.

Upon the whole, it appears, that water actually preſſes againſt bodies under it, whether ſpecifically lighter, or heavier than itſelf.

SECT.

SECT. II.

Water actually weigh'd in water, by common ſcales.

TO my particular method of actually weighing water in water, it has been objected, by a late writer of hydroſtatics, that there is a miſtake in it. I ſhall here, therefore, endeavour to clear this matter, and ſet my thoughts about it in a fuller light.

My opinion is, that water, as it is a heavy fluid, always retains its gravitation, and power of preſſing; (by which I mean, a tendency downwards, whatever be the cauſe of that gravity) whether it have a body under it, either ſpecifically heavier or lighter than itſelf, or one equi-ponderant to it. For I ſee not what ſhould deſtroy, or aboliſh this gravity, tho' many things may hinder ſome effects of it. And, therefore, I ſuppoſe that water retains its gravity, not only in air, but in water too, and in heavier liquors; and, conſequently, by virtue of this, that liquor preſſes upon them. But, if a ſurrounding fluid have, upon account of its ſpecific gravity, an equal, or a ſtronger tendency downwards than water, it will, by virtue of that, be able to impel up this liquor, or to keep it from actually deſcending; ſo that a portion of water, ſuppoſed to be included in a veſſel of the ſame ſpecific weight with water, will, placed in a greater quantity of the ſame water, neither riſe nor fall, tho' it retains its gravity there; only this gravity is kept from making it actually deſcend by the contrary action of the other water, whoſe ſpecific gravity is ſuppoſed equal: as when a juſt balance is loaded with a pound weight, in each of the ſcales, tho' neither of the weights actually deſcend, being hinder'd by its counterpoiſe; yet each retains its whole weight, and, with it, preſſes the ſcale it reſts on: ſo that our included portion of water, really preſſes the ſubjacent water, tho' it does not actually depreſs it. Nor do I think that the only way of judging whether a body gravitates, is to obſerve, whether it actually deſcends: ſince, in many caſes, its gravity may be proved, by the reſiſtance it makes to heavy bodies, which would otherwiſe raiſe it; as appears by equal weights in a balance. And, for want of this diſtinction, I have known even learned men, treating of hydroſtatics, miſtake the queſtion.

Now, the adverſaries I had to deal with, both in print and in diſcourſe, deny'd, that in ſtanding water, the upper preſs'd, or gravitated upon the lower parts. And tho' they could not but grant, that the whole weight of the water gravitated upon the bottom of the veſſel; yet they would have the parts of it to do ſo *actione communi*, as they ſpeak; and fancied I know not what power of nature, to keep the homogeneous portions of water, as well as other elements, from preſſing one another, whilſt it is in its proper place. Againſt this opinion it was alledg'd, (beſides other things, which I found many, otherwiſe good ſcholars, were not fitted to underſtand) that if a glaſs vial, or bottle, well ſtopp'd, were deeply immerſed under water, it would ſtrongly tend upwards; but if it were dextrouſly unſtopp'd, when 'twas thus immers'd, ſo that the water could get in; allowing for the weight of the glaſs itſelf; 'twould, by the water that crowds in and thruſts out the air, be made ſtrongly to tend downwards, and continue ſunk. But this not ſatisfying, becauſe 'twas pretended that the reaſon of the empty bottle's emerging when ſtopt, was the poſitive levity of the air that fill'd it, and that the ſinking of it when unſtopt, was from the receſs of the ſame air, which by the intruding water was driven with large bubbles out of the bottle; I thought this evaſion might be obviated by contriving an experiment, wherein the water ſhould be plentifully and ſuddenly admitted into the glaſs, and yet no air expell'd out of it, ſo that if then the glaſs, which was ſuſtain'd before, ſhould fall to the bottom, with a gravitation amounting to a conſiderable weight in reſpect of its capacity, the ſinking of it could not be aſcrib'd, as before, to the receſs of the air, endow'd, as they ſuppoſe, with poſitive levity, but to the weight of the water admitted, which when thus weigh'd would be environ'd with water of the ſame kind; and to ſhew that this water might have a conſiderable weight, notwithſtanding the place it was in, I employ'd a pair of ſcales, after the manner recited in the experiment, *p.* 288. *Vol.* II.

However therefore my expreſſions diſagree with thoſe of my adverſary, the diſtance of our opinions is not ſo wide as it ſeems at firſt ſight; for he allows as well as I that the ſuperior parts of water do by their gravity preſs the inferior; but this he would not have amount to ſo much as to mean, that water weighs or gravitates in water. But if he thinks that in my experiment I meant to propoſe a method of making water deſcend in water, and weigh it in that liquor with a pair of ſcales, juſt as I would a piece of lead, or a portion of mercury, which are bodies much heavier *in ſpecie* than water, either he miſtakes my intention, or I did not ſufficiently declare it; what I deſign'd to ſhew, and I think have ſhewn, was, that by the help of an ordinary balance it may be made appear, that water admitted into the glaſs bubble I employ'd, caus'd it to weigh much heavier than it did before that liquor enter'd into it; and that this new weight manifeſted by the balance was not due, as my adverſary ſuppoſes, to a receſs of the air.

To this experiment our author objects the following. "Take a piece "of wood lighter *in ſpecie* than water, and add weight to it by degrees, "till it become of the ſame weight with water; faſten it with a ſtring to "a balance, and weigh it in water, and you will find the whole weight "ſupported by the water." But this is ſo far from overthrowing my opinion, that it agrees very well with it; for ſuppoſe the weight added to the light wood be lead, it cannot be ſaid that the metal loſes its native ponderoſity whilſt it reſts in the water; and the reaſon why it deſcends not, is, that it, and the wood whereto it is join'd, are hinder'd by the counterpoiſe of the collateral water, which by its preſſure would raiſe the ſurface of the water whereon the floating body reſts, if it were not hinder'd by the weight of theſe incumbent ſolids; and this reſiſtance of theirs to the endeavour of the water upwards being exercis'd only upon account of their gravity, ſhews that they do, in my ſenſe, gravitate, tho' not pregravitate.

The relative levity of bodies under water.

'Tis obvious that if wood, wax, or other bodies lighter *in ſpecie* than water, be detain'd under it, they will, upon removal of the force, emerge to the top; and this they do ſo readily, and as it ſeems ſpontaneouſly, that the generality of philoſophers, both ancient and modern, aſcribe it to an internal principle, which they call poſitive levity.

But this principle was not always ſo univerſally receiv'd among philoſophers as in later ages; *Democritus*, and ſeveral of the ancients, admitting no abſolute, but only a relative levity; which opinion ſome of the moderns have attempted to revive.

The experiment uſually urg'd to prove the poſitive levity of wood in water, ſeems to me too ſlightly made to be acquieſced in; 'tis true indeed, if a flat board, for inſtance, a trencher, have its broad ſurface kept by a man's hand upon the horizontal bottom of a bucket full of water, when the hand that detain'd it is remov'd, the trencher will commonly ſoon aſcend to the ſurface of the water. But I do not perceive that a deciſive experiment of this kind is eaſy to be made with ſuch materials; for the wood whereof both the trencher and the bottom of the bucket conſiſt, are ſuppoſed to be lighter *in ſpecie* than water, and conſequently they muſt be of a porous and lax texture; whilſt the more ſolid woods, as *Lignum vitæ*, brazil, *&c.* whoſe texture is cloſer, ſink in water; if therefore there be not much care uſed to bring the ſurfaces of the trencher and the bottom of the bucket to a true flatneſs and ſmoothneſs, the experiment will not be accurately made; and perhaps tho' it be mentally, yet it is ſcarce practically poſſible to bring ſuch porous bodies as the lighter woods to ſuch a contact as is neceſſary in this caſe; and were that actually done, I ſhould not expect that the trencher would aſcend; for in my opinion the cauſe why in ordinary inſtances wood, wax, and other bodies ſpecifically lighter than water, being let go at the bottom of a veſſel full of that liquor, emerge to the top, is chiefly, that there is no ſuch exquiſite congruity and contact between the lower ſuperficies of the wood and the upper of the bottom of the veſſel, but that the lateral parts of the water, being impell'd by the parts of the ſame liquor incumbent on them, are made to inſinuate and get between the lower parts of the wood and the bottom of the veſſel, and ſo lift or thruſt upwards the wood; which, bulk for bulk, is leſs heavy than the water that extrudes it.

But as the whole of the argument of thoſe I diſpute with conſiſts in a ſuppoſition, that becauſe the trencher is plac'd on the bottom of the veſſel, no water can come between to buoy it up, whence they conclude it muſt aſcend by an internal and poſitive principle of levity, I made the experiment after another, and, if I miſtake not, a better manner.

Shewn by experiments, which overthrow the doctrine of poſitive levity.

We took then two round, cylindrical plates of black marble, which had thoſe ſuperficies that were clapp'd together, ground very flat, and carefully poliſh'd, that they might touch in as many parts as poſſible, and that the upper being taken up, the other might ſtick to it, and aſcend with it; and the better to keep out the water, the internal ſurfaces were, before they were put together, lightly oil'd, which did not hinder them from moſt eaſily ſliding along one another, as long as the contiguous ſurfaces were kept horizontal; then a blown bladder of a moderate ſize was faſten'd to the upper marble, and both of them were let down to the bottom of ſome ſtagnant water, where, by the help of an eaſy contrivance, the lower marble was kept level to the horizon; and now the patrons of poſitive levity would have concluded, that the bladder, being a body much lighter than wood, and being in an unnatural place, beneath the ſurface of the water, ſhould of its own accord and with impetuoſity emerge; but I expected a contrary event, becauſe the bladder being ty'd to the upper marble, ſo that both of them might in our caſe be conſider'd as one body, the water could not impel them up, ſince the cloſe contact of the ſurfaces of the two marbles kept the water from inſinuating between them, and conſequently from getting beneath the upper marble, and preſſing againſt the lower ſuperficies of it; and to ſhew that this was the reaſon why the bladder did not emerge, I cauſed part of the oil'd ſurface of the upper marble to be ſlid off from that of the lower, which by reaſon of the ſmoothneſs and ſlipperineſs of the ſurfaces was very eaſy to do; but the contact ſtill continuing in a greater part of the ſurface than was requiſite, more and more of the upper marble was gradually ſlipt from the lower, till at length, when the marbles touch'd but in one half of their ſurfaces, the endeavour of the water to force up the bladder full of air, being ſtronger than the reſiſtance which the contact but of part of the ſurfaces of the ſtones was able to make, they were ſuddenly disjoin'd, and the bladder, by the extruding water, impetuouſly ſhot up above the top of the water.

With the ſame marbles we made ſeveral other experiments of this kind, moſt commonly letting them down both together, but once or twice at leaſt by placing the upper marble under water upon the lower, already fix'd to the bottom of the veſſel.

That 'twas not the weight of the upper marble, nor want of lightneſs, whether poſitive or relative, of the air included in the bladder, that kept it from aſcending, is manifeſt by the impetuous aſcent of it upon disjoining the marbles; and by this, that the bladder would lift up from the lower parts of the water, not only the upper ſtone, when it touch'd not the other, but a weight of 7 or 8 pounds hanging at it.

We alſo took a bladder, in great part freed from its air, and tying the neck of it very cloſe, that none of the remaining air might get out, we faſten'd a conſiderable weight to it, and by the help of this ſunk the bladder to the bottom of a wide-mouth'd glaſs full of water, that the ſurface of the liquor might be conſiderably higher than the upper part of the bladder; this wide-mouth'd glaſs we included in a great receiver, which was carefully cemented on to the engine. The principal deſign of the experiment was to ſhew, that tho' the air included in the bladder was very far from being able, by its abſolute levity, to lift up ſo great a weight as the bladder was clog'd with; yet the ſame air, included in the bladder, would, by its mere expanſion, without any new external heat, acquire a power of aſcending, notwithſtanding that weight; which aſcent, therefore, muſt be attributed to the water, that, according to hydroſtatical laws, ought, *cæteris paribus*, to reſiſt thoſe immerſed bodies the more powerfully, that being lighter, *in ſpecie*, than it, poſſeſs the greateſt place in it, and hinder the more water from acquiring its due ſituation. For the proſecution of this trial, we began to pump the air out of the great receiver; and its preſſure upon the ſurface of the water being thereby gradually leſſen'd, the ſpring of the included air began by degrees, to diſtend the ſides of the bladder, till, at length, the included air, ſwelling every way, took up ſo much more room in the water than it did before, that the water was able to lift the bladder, and the annexed weight, to the top, and detain it there, till we thought fit to let in ſome of the excluded air; which forcing that in the bladder into leſs dimenſions, the weight was preſently able to ſink it to the bottom.

But if inſtead of hanging ſo great a weight at the neck of the bladder, we faſten'd only a moderate piece of lead, ſuch as would only ſerve to ſink the bladder, and keep it at the bottom of the water, ſo that the aggregate of the bladder, air, and metal, was but a little heavier than an equal bulk of water; then, upon the firſt ſtroke of the pump, which could withdraw but a ſmall part of the air in the receiver, that in the bladder ſuddenly expanding itſelf, the whole would immediately be impetuouſly extruded by the water; tho' after ſome reciprocations, it floated in its due poſition; but, upon the return of a little outward air, it would immediately ſubſide.

It may be objected, that the aſcent of the weight was not effected by the preſſure of the water; becauſe rarity and levity, being ſimilar qualities, the great rarifaction of the air might, proportionably, increaſe the levity of it; and, conſequently, enable it to perform much greater things than it could do before. I ſhall anſwer this objection by the following experiment.

About the neck of a conveniently ſhaped vial, able to hold a few ounces of water, I cauſed to be carefully ty'd the neck of a ſmall bladder, whence the air had been well expreſs'd; ſo that the bladder, being very limber of itſelf, and, probably, made more ſo, as well as more impervious to the air and water, by the fine oil we had caus'd it to be rubb'd with, lay upon the orifice of the vial, like a skin clapp'd together, with many folds and wrinkles. This done, we let down the vial into a veſſel full of water; and the vial being poiſed for that purpoſe, ſunk perpendicularly in the liquor, till its neck was partly above, and partly beneath the ſurface of the water: then covering the external glaſs with a large receiver, we cauſed the air to be pumped out; and as the preſſure of that was gradually withdrawn, the air in the floating vial expanded itſelf into the bladder, and unfolded the wrinkles of it, till, at length, it became full blown, without altering the erect poſture of the glaſs it reſted on. But

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this great expanſion, being made above the water, and, conſequently, in a medium, not heavier than the included air, gave that highly rarify'd air no ſuch increaſe of levity, as ſhew'd that it made ſo much as the neck of the glaſs riſe higher in the water than it did before. Nor did we obſerve, that the return of the air into the receiver, by reducing the air in the bladder to its former unrarify'd ſtate, made the glaſs ſink deeper than before. But when the experiment was try'd with the ſame glaſs and bladder, at the bottom of the water; upon pumping out the air, the bladder, being dilated under water, was, after a while, carried up to the top, and took up with it about eight or ten ounces, that, to clog it, had been faſten'd to the bottom of the vial.

But, to examine whether a portion of air, by being rarify'd, or expanded, acquires a new and proportionable degree of poſitive levity; we took a bladder, of a moderate ſize, which was very fine and limber, that it might be the lighter, and more eaſily diſtended. The greateſt part of the air, being ſqueez'd out of the bladder, the neck of it was ty'd up very cloſe, that no air might get in or out of it. This limber bladder was hung at one of the ſcales of a balance, whoſe beam had been purpoſely made more than ordinarily ſhort, that the inſtrument might be ſuſpended, and continue capable of playing in the cavity of a great receiver, into which we convey'd it; having firſt carefully counterpois'd the bladder with a metalline weight, in the oppoſite ſcale. This done, the air was pumped out, and as that was withdrawn, the bladder was gradually expanded by the ſpring of the internal air; till, at length, when the receiver was well exhauſted, it appear'd to be quite full. But, notwithſtanding this great dilatation of the included air, it did not appear, by the depreſſion of the oppoſite ſcale, to be grown manifeſtly lighter than it was at firſt. And the bladder ſeem'd alſo to retain the ſame weight, after it had, by the air that was let into the receiver, been compreſſed into its former flaccid ſtate.

To determine whether a fiſh riſes or ſinks in water, by contracting or expanding itſelf; take a bolt-head, with a wide neck, almoſt fill it with water, and put therein a large live fiſh; then draw out the neck of the glaſs exceeding ſlender, and fill that alſo with water. Obſerve the motion of the fiſh; and if, upon his ſinking, the water at the ſlender top ſubſides; we may infer, he contracts; and if, upon his riſing, the water be raiſed, we may conclude, he dilates himſelf.

The preſſure of the air's ſpring on bodies under water.

I employ two ſorts of experiments, to ſhew, that a ſmall quantity of incloſed air, may, by its preſſure, have a conſiderable effect upon bodies under water, notwithſtanding the interpoſition of that liquor. For this preſſure may be manifeſted by what it directly and poſitively performs upon bodies cover'd with water; and alſo by the phenomena that regularly enſue upon the removal of the incloſed air, or the weakning of its ſpring.

Manifeſted by experiments.

We took a ſquare glaſs vial, capable of holding twelve ounces of water; the neck of this we luted on carefully, and ſtrongly, over the orifice

STATICS. of the ſmall pipe, at which the air paſſes in our engine, out of the receiver into the pump: then covering this glaſs with a great receiver, we luted it ſtrongly to the engine, and, at the top, pour'd in as much water as ſufficed to ſurround the internal receiver, if I may ſo call it, and cover it to a pretty height. This done, we exactly cloſed, with a turn-key, the hole of the great receiver, at which the water had been pour'd in, that no air might paſs that way. And laſtly, we began to pump out the air contain'd in the internal receiver; that the air, which, by the above-mention'd pipe, had communication with the external air, might no longer, by its preſſure, aſſiſt the glaſs to reſiſt the preſſure which the incumbent and incloſed air, by virtue of its ſpring, conſtantly exerciſes upon the ſubjacent water, and, by its intervention upon the ſides and bottom of the internal receiver. At the firſt exſuction, which could not be ſuppoſed to have well emptied the internal glaſs, this veſſel was, by the preſſure of the ſuperior air upon the ſurrounding water, broken into numerous pieces. The ſame experiment, with a like ſucceſs, was repeated with a ſtronger internal glaſs.

Having ſqueez'd out of a moderately ſized bladder the greateſt part of its air, we tied the neck of it very cloſe; and then faſtening to it a competent weight, we placed it at the bottom of the talleſt and largeſt glaſs we could cover with our great receiver; that ſo, tho' the incumbent air were pump'd out, none of the water might eſcape with it, but ſtill retain the ſame height above the bladder. Having then pour'd upon the bladder as much water as would ſwim high above the upper part of it; we cover'd this glaſs of water with a great receiver, which being carefully cemented on to the engine, the pump was ſet on work; and as the air which by its ſpring preſs'd upon the ſurface of the included water, was by degrees pumped out; ſo the air impriſon'd in the bladder, gradually expanded at the bottom of the water, as if no ſuch liquor had interpoſed between them, otherwiſe than by its weight; upon which account it muſt be allow'd a little to hinder the expanſion of the bladder, in proportion to the height it has above it. Thus the immerſed bladder was at length full blown, by the dilatation of the air incloſed in it; and, by its intumeſcence, made a conſiderable part of the water run over the ſides of the glaſs that before contain'd it all. And when acceſs was given again to the external air, the internal being compreſs'd, the bladder was preſently reduced to its flaccid ſtate.

We took a ſmall fine bladder, whoſe neck was ſtrongly ty'd up, when it was about half full of air; this we put into a ſhort braſs-cylinder, the lower of whoſe baſes was clos'd with a braſs-plate, and the other left open; this open orifice we afterwards ſtopp'd, but not exactly, with a cylindrical plug, that was ſomething leſs than it, and was, by a rim at the top, hinder'd from reaching too deep into the cavity of the cylinder, that it might not damage the bladder which lay beneath it: upon the plug we placed a conical weight of lead; and this pile of ſeveral things, being ſo placed upon our engine, that we could cover it with a great receiver, we carefully

fully cemented on, ſuch a veſſel, and at the top of it pour'd in ſo much water as would ſerve to fill the vacant part of the braſs-cylinder, and the cavity of the engine to ſuch a height, that it covered all the leaden weight, which was ſeveral inches high, except a rim faſtened to the top of it, for the conveniency of removing it. This being done, the pump was work'd; and, long before we had exhauſted the air of the receiver, that which was incloſed in the lank bladder had, by degrees, diſplay'd ſo vigorous a ſpring that it raiſed up the weight, which lay upon it, to a conſiderable height, and kept it there, till the air was let in from without, to aſſiſt its depreſſure by the lead, which amounted to no leſs than about twenty-eight pounds.

We took a cylindrical copper-veſſel, of a conſiderable height; into this, being firſt almoſt fill'd with water, we put a ſquare glaſs-vial, capable of containing nine or ten ounces of water, and exactly ſtopp'd with a cork, and a cloſe cement, this vial, by a competent weight, was detained at the bottom of the water, from whoſe upper ſurface it was conſiderably diſtant; then the copper-veſſel, being placed upon the engine, and included in a great receiver, well cemented on; the air was, by degrees, pumped out: but, before it was quite exhauſted, the glaſs, at the bottom of the water, was, by the ſpring of the air included in it, burſt into many pieces, not without great noiſe, and a kind of ſmoke, or miſt, that appeared above the ſurface of the water: and another glaſs, of the ſame ſort, was broke, after the ſame manner, in another veſſel

Now, the conſequence that naturally reſults from the three laſt experiments, is, that ſince barely upon withdrawing the preſſure of the included air, the air reſiding in the immerſed bodies, did, by virtue of its ſpring, expand itſelf ſo forcibly, and perform ſuch conſiderable things; the air above the water muſt have exerciſed a very ſtrong preſſure upon the ſurface of it; for, it muſt have been, at leaſt, equivalent to that force of the immerſed air, the exerciſe whereof it was able totally to hinder.

And, from hence, it may be eaſily deduced, that the weight of the atmoſphere acts upon bodies under water; tho' the interpoſed liquor is vaſtly heavier, *in ſpecie*, than in air: for, we have juſt now proved the preſſure of incloſed air, which conſiſts in its ſpring upon bodies under water. And, 'tis manifeſt, that the ſtrength of the ſpring of this lower air, we make our trials with, is cauſed by the weight of the upper, which bends and compreſſes thoſe little ſpringy particles whereof our air conſiſts; ſo that the weight of the atmoſphere, being equivalent to the ſpring of the lower air, muſt preſs upon the ſurface of the ſubjacent water, with a force equal to the ſpring of that part of it which is contiguous to the water.

Fluids preſs in a different manner from ſolids.

It has proved a great impediment to the reception of the doctrine of the weight and preſſure of the atmoſphere, that, if it could, really, exerciſe ſo great a force as we aſcribe thereto, it would, unavoidably, oppreſs all the bodies expoſed to it: ſo that neither men, nor other animals, would be able to move under ſo great a load, or withſtand its force.

This I grant to be a plauſible objection, but the force of it will be taken away by the following conſiderations put together.

Whence, the weight of the atmoſphere is not prejudicial to the bodies of animals.

And firſt, the power of preſſing, that we aſcribe to the air, is not a thing deduced from doubtful ſuppoſitions, or bare hypotheſes, but from real and ſenſible experiments. And therefore, ſince we have clear and poſitive proof of the preſſure of the air; tho' we could not explain how men and other animals are not deſtroyed by it; yet we ought rather to acknowledge our ignorance in a doubtful problem, than deny what experience manifeſts to be true. Thus the attractive virtue, and other powers of the loadſtone, are freely acknowledg'd, even by thoſe who confeſs themſelves unable to explain them; tho', if experience did not ſatisfy us of them, they were liable to many more conſiderable objections, than any urg'd againſt the preſſure of the air.

But thoſe who make this objection againſt the great preſſure of air, ſeem not aware, that we were conceiv'd, and born in places expoſed to the preſſure of the atmoſphere: and, therefore, how great ſoever that preſſure appears to be, it ought not to cruſh us now: ſince, when we were but embryos, or newly born, we were able to reſiſt it; and not only live, but grow to all dimenſions, in ſpite of it.

If there were any place about the moon, or other celeſtial globes, which ſome ſuppoſe to be inhabited, that had no atmoſphere, or equivalent fluid, about it, and where men, yet, could be generated a-new; and one of theſe men ſhould be tranſported thence, and ſet down upon our earth, there might be made an experiment proper to decide our controverſy. In the mean time, I doubt, ſince nature is not obſerved to make things ſuperfluouſly ſtrong, ſuch a human body, not being deſign'd to reſiſt any weight or preſſure of air, would be of ſo tender and compreſſible a make, that it would eaſily be cruſh'd inwards, by our atmoſpherical preſſure. And tho' we cannot give an inſtance of this kind, yet we make trials ſomewhat like it, in our pneumatical engine. For when we place water in our receiver, and pump out the air that was above it, there ~~will~~ be generated a multitude of bubbles; ſome of which, when the air is carefully withdrawn, ſwell to a ſtrange and ſcarce credible bulk. Theſe bubbles being generated where the air cannot preſs upon them, ſuch dimenſions are ſo natural to them, that if the receiver be ſtanch, and no other unfriendly accidents intervene, they would probably be durable; ſince I have known the ſpring of highly-dilated air continue for many months: and a bladder would, for no leſs time, continue blown and filled, in our vacuum, by a little air left in it, when the external was withdrawn from it. Yet the large bubbles, when once the ontward air is ſuffer'd to come in upon them, are thereby ſo violently compreſs'd, that in a trice they ſhrink into dimenſions too ſmall to keep them viſible. And if I could have ſucceeded in my attempt of producing living bodies in our vacuum, I ſuppoſe the ſucceſs would have confirm'd our doctrine.

You will tell me, that ſo great a weight and preſſure, as I aſſign to the atmoſphere, muſt needs make a man feel pain; and if not diſlocate ſome of the parts, at leaſt preſs the whole body inward. But, 1. being accuſtom'd to this preſſure from our very birth, and even before it, we are hinder'd from taking notice of it; thoſe preſſures only being ſenſible to us, that are made ſo by ſome additional cauſe, which, by making a new impreſſion, excites us to obſerve it. Thus we are not ſenſible of the weight of the clothes we wear; and ſo a healthy man is not ſenſible of the heat in his heart, becauſe 'tis conſtant there, and the ſentient parts of the heart have been uſed to it, tho' that heat has been often very conſiderable; but when, in live diſſections, a man puts his finger into the heart of an animal, which probably has a fainter, or at leaſt no ſtronger degree of heat, than a human heart, he will feel in his fingers, accuſtom'd to the air, a manifeſt degree of heat. 2. It has been proved, that a cubic inch of air, for inſtance, has a ſpring ſufficient to reſiſt the weight of the whole atmoſphere, as far as it is expoſed thereto; otherwiſe it would be more compreſs'd than in fact it is. 3. A very little portion of air, tho' it much ſooner loſes its ſpring by expanſion than a greater, yet will reſiſt further compreſſion, as much as a greater. 4. The pores of the parts of animals, whether fluid or conſiſtent, as in their blood, gall, urine, heart, liver, *&c.* there are included a multitude of aerial corpuſcles; as appears from the numerous bubbles afforded by ſuch liquors, and the ſwelling or expanſion of the conſiſtent parts in our exhauſted receiver. 5. To this we may add, that beſides the bones, whoſe ſolidity is not queſtion'd, a much greater part of the human body than is uſually imagined, really conſiſts of membranes and fibres, and the coalitions and contextures of them; and that theſe ſubſtances are, by the moſt wiſe author of things, made of a much cloſer and ſtronger texture, than thoſe, who have not try'd, will be apt to think. Laſtly, there is a far greater difference than men commonly ſuſpect, between the effects of the preſſures made upon bodies by ſolid weights, and thoſe they ſuffer from ſurrounding fluids.

Hence 'tis not neceſſary that the preſſure of the atmoſphere, tho' as great as we ſuppoſe it, ſhould oppreſs and cruſh the bodies of men, who live under it; for the ſolidity of bones, and the ſtrong texture of the membranes and fibres, with the ſpring of the aerial particles, (which abound in the ſofter as well as in the fluid parts of bodies) that is equivalent to the preſſure of as much of the atmoſphere, as can exerciſe its preſſure againſt them; make the frame of a human body ſo firm, that it may well reſiſt the preſſure of the outward air, without having any part violently diſlocated; whilſt this external preſſure is exerciſed only by the air, which being but a ſurrounding fluid, preſſes equally on every ſide. And becauſe our bodies have been produced in the atmoſphere, and from our very birth expoſed, without intermiſſion, to the preſſure of it; our having been continually accuſtom'd to this preſſure, and the firmneſs of our ſtructure, keep us from being ſenſible of the weight or preſſure.

Multitudes of men have had occaſion to paſs over high mountains, and I myſelf have been upon the *Alps* and *Apennines*; yet I never met with, nor heard of any one, who took notice of the difference, as to the weight of air ſuſtain'd; or who complain'd, that, when come down to the foot of the mountain, they felt any greater compreſſure from the air, than at the top. Yet numerous experiments witneſs, that on more elevated parts of the earth, which have a leſs height of the atmoſphere incumbent on them; the weight and preſſure of the air is not ſo great as below. And, on very high mountains, 'tis not unlikely that this difference may be very conſiderable; ſince, when the *Torricellian* experiment was made near *Clermont* in *France*, upon the *Puy de Domme*, which is none of the higheſt mountains in the world, they found the difference of the mercury at the top and bottom, to amount to about three inches; and, conſequently, if the trial had been made with water inſtead of quick-ſilver, the difference would have been about three feet and a half, perpendicular height. And on much higher mountains, the difference of the height of the mercurial cylinder at the top and bottom, would be much greater. But, at the bottom of ſome very deep well, or mineral groove, which may, without improbability, be ſuppoſed placed at, or near the foot of one of theſe mountains; if we conceive the barometer to be let down, the variation would be much more conſiderable; yet we find not that the diggers in the deepeſt mines, of mountainous countries, are ſenſible of being depreſſed, or compreſſed, by any unuſual weight. But, to build only on matter of fact; it appears by the preceding obſervations, that when a man was at the bottom of the hill, he had an additional weight of air preſſing upon his head, above what he ſuſtain'd at the top, equal to the height of an imaginary veſſel of water, having his head for its baſis, and three feet and a half high; which is ſo conſiderable a weight, as could not but have been, of itſelf, very uneaſy to ſupport. And the ſame muſt be, proportionably, applied to the other parts of his body. Whence, we may infer, that ſuch a weight was not felt by the man it compreſſed, becauſe the preſſure was exerciſed after the uniform manner of fluid bodies.

And this may ſuffice to ſhew, that there is no neceſſity the compreſſure of the atmoſphere ſhould make it impoſſible to live in it. But, to clear this matter ſtill farther, we made the following experiments.

Experiments to illuſtrate and confirm this doctrine.

Into a ſhort hollow cylinder of braſs, we put a fine bladder, ty'd ſo cloſe at the neck, that none of the air, whereof it was about half full, would probably get out. This we did, to the end that the hen's egg we were to bed in it, might lie ſoft, and have its ſides almoſt cover'd with the limber flaccid bladder, and contained air; then we cover'd the remaining part of the egg with another bladder, that nothing hard might come to bear immediately upon the ſhell; when putting a wooden plug into the cylinder, and a weight upon the plug, tho' ſlowly and warily, leſt the quick deſcent of the weight ſhould make the plug break the egg; we, in the laſt place, cover'd the cylinder with a large receiver, and the air being drawn out, that air which was ty'd up in the bladder, by degrees, expanded itſelf ſo ſtrongly, as to lift up the plug and the incumbent weight, to a conſiderable height, and keep it there, till the external air was re-admitted.

Now ſince the air in ſuch caſes expands itſelf vigorouſly every way, it muſt preſs againſt the egg with the ſame force that it did, proportionably, againſt the bottom of the plug; and that force was more than ſufficient to lift up the weight, which, together with the plug, amounted to about thirty pounds: yet the egg being taken out, appear'd perfectly whole, and no way harmed. But the ſame egg was afterwards cruſh'd to pieces, by laying warily ſmall weights one after another upon it, till they amounted to about four pound weight.

We took a glaſs bubble, about an inch and a half in diameter, blown at the flame of a lamp, that it might be far more eaſy to break than the thinneſt vial; this bubble we included between bladders, as we did the egg in the former experiment; and then having warily put the plug into the cylinder, ſo as to preſs upon the bladder that ſurrounded the glaſs, we leiſurely put the weights upon the plug, till they, together with it, amounted to thirty pounds, or more; which being removed, the plug was removed; and the glaſs-bubble, tho' it were perhaps as thin as fine white paper was taken out whole.

But, leſt the great reſiſtance of ſo thin a glaſs, which yet was not hermetically ſeal'd, ſhould be aſcrib'd to its ſpherical figure; we employ'd, inſtead of it, the ſhell of an egg, whence, by a hole made at one end, the yolk and white had been taken out. This empty and imperfectly cloſed ſhell, we treated as the glaſs-bubble, in the laſt experiment; and, notwithſtanding the great leaden-weight that reſted upon the plug, it eſcaped without the leaſt crack.

And, to ſhew that what we obſerv'd about the nature of the compreſſure of fluid bodies, will hold as well in water as air; we put an hen's egg into a limber bladder, almoſt full of water; and tying the neck very ſtrait, that nothing might get in or out, we ſo placed the bladder in the braſs-cylinder, that the egg might not be immediately touch'd by any thing that was hard; then putting the plug into the cylinder, we warily and leiſurely heaped upon it convenient weights of lead, till they amounted to about ſeventy-five pounds: notwithſtanding all which, the egg was taken out ſound and uncrack'd; and probably might have ſupported a much greater preſſure.

Nor were eggs the only bodies we endeavour'd to cruſh after this manner: the trial having been alſo made upon a piece of butter, of a very irregular ſhape, it came out unalter'd.

A N

Eſſay-Inſtrument,

Wherewith to examine if COIN *be adulterate or counterfeit.*

An eaſy method of determining the ſpecific gravities of fluids and ſolids.

HAving formerly uſed a little inſtrument, conſiſting of a glaſs-bubble, with a long ſlender ſtem, to compare the ſpecific gravities of ſeveral fluids, by its degrees of ſinking in them; and coming to conſider its uſes more attentively, I found it alſo proper to diſcover the ſpecific gravities of ſeveral ſolids; for every ſolid heavier than water, loſes in water as much of the weight it had in air, as a bulk of water equal to the immerſed ſolid would weigh in the air; and conſequently, as gold is by far the moſt ponderous of metals, a piece of it, and one of equal weight of any other metal being produc'd, the gold muſt be the leaſt in bulk; if both of them therefore be weigh'd in water, the gold muſt loſe leſs of its former weight than the other metal, becauſe both grow lighter herein by the weight of a bulk of water equal to each reſpectively; hence I concluded that my floating inſtrument would be made to ſink deeper by an ounce, for inſtance, of gold hanging at it under water, than by an ounce of braſs.

Fig. 27.

This inſtrument may be made of glaſs, copper, ſilver, or almoſt any other ſolid body that is fit to float on water with a piece of coin hanging at it, and of a texture cloſe enough to keep out the water; it conſiſts of three parts, the ball, the ſtem, and the place for the coin; the ball BCDE, if of metal, conſiſts of two thin concave plates ſolder'd together in the middle; and at the moſt diſtant parts from the juncture there ought to be left two oppoſite holes, one in each plate, for the two other parts of the inſtrument; the ball ſhould not be exactly round, but of any fit ſhape, to keep the inſtrument erect and ſteddy in the water; and it muſt contain air enough to prevent the whole inſtrument, when loaded,

from

from ſinking below the top of the ſtem; the ſtem A B muſt be ſolder'd on to the ball, at the upper hole thereof; it may be either hollow or ſolid, but it ought to be made very ſlender, and conſiderably long; in the lower hole of the ball is ſolder'd the undermoſt part of the inſtrument, which I call the ſtirrup G; the ſcrew F is a very ſhort piece of braſs, with a broad ſlit in it, capable of receiving the edge of the coin, which with a turn or two of a ſlight ſmall ſide-ſcrew may be kept faſt in; the ſtirrup G is made of wyre, which a little below the bottom of the ball is bent round, ſo as to ſtand horizontally, that the coin may be laid upon it; it would be convenient to have the under ſtem and ſcrew made ſeparate, that the ball of the inſtrument being ſufficiently large, there may be room for two or three flat round pieces of metal K, with each of them a hole in the middle, fitted to the ſize of the ſtem, to ſerve as balaſt, being plac'd near the lower part of the ball, when the ſcrew may be thruſt on to ſupport them.

Fig. 28.

Fig. 29.

Applied to the examination of coin.

To adjuſt this inſtrument for the examination of a guinea, you muſt, by the help of the ſtirrup or ſcrew, hang a guinea, known to be genuine, at the bottom of it, and having ſtopt the orifice of the ſtem, if hollow, that no water may get in, immerſe the inſtrument gently and perpendicularly in a veſſel of fair water almoſt to the top of the ſtem; then letting it alone, if, being ſettled, it continues in the ſame ſtation and poſture, the work is done; if it emerge, add a little weight to it, either at the bottom or the top, that the guinea may depreſs it as low as is required; but if it ſink quite under the water, lighten it, by filing off from the balaſt-plates above-mention'd, or otherwiſe; then ſet a mark H where the ſurface of the water touches the ſtem, and taking out the inſtrument, ſubſtitute in the place of the guinea a little round plate of braſs, of the ſame weight, or a grain or two heavier, in the air; and putting the inſtrument into water, as before, let it ſettle, and make another mark I at the interſection of the ſtem, and the horizontal ſurface of the water.

Fig. 27.

But there may happen a caſe wherein the practical application of our inſtrument will be unſecure; for if a falſifier of money have the ſkill, by waſhing or otherwiſe, to take off much of the ſubſtance of the coin, without altering or impairing either the figure or the ſtamp, the coin will not depreſs the inſtrument to the uſual mark, whence the guinea may be judg'd counterfeit, tho' it be only too light, however it preſently ſhews that the propoſed guinea is abus'd; and if the want of weight appear by the inſtrument to be very great, it is a ſtrong preſumption that it is rather waſh'd than counterfeit; but the balance will preſently reſolve the doubt, for if the ſuſpected coin has its due weight in the air, the great lightneſs of it in water ſeems to proceed from its not being of the requiſite fineneſs; and if it want much of its due weight in the air, it is probably waſh'd, *&c.* rather than adulterated.

It may be very convenient for thoſe who have frequent occaſions to examine various ſorts of coin, to have a different inſtrument adjuſted to each; but if the ball be made large, and with a ſtem long and ſlender enough, we may ſoon, by changing the balaſt-plates occaſionally, fit the ſame inſtrument to examine coins of different metals and weights; thus one of theſe made of copper ſerves me for guineas, crown-pieces, and half-crowns, and may eaſily be made to ſerve for ſeveral foreign coins.

And other mix'd metals.

After the ſame manner may pewter be compar'd and examin'd; for having once obſerv'd how far the inſtrument is ſunk, by a determinate weight of it, which is known to be good in its kind, and to contain its due proportion of lead and tin, if you load the inſtrument with an equal weight of any other maſs of pewter, and it ſink deeper, the proportion of lead very probably exceeds in the mixture; tho' 'tis poſſible to debaſe pewter with mineral ſubſtances, whoſe ſpecific gravities are not well known; but to do it with lead ſeems to be the moſt gainful way.

This inſtrument may alſo aſſiſt us to make a near eſtimate of the fineneſs of gold, and its different alloys with ſilver, or other determinate metals; for being fitted to ſink to the top of the pipe with a certain weight of the pureſt gold, as ſuppoſe an ounce, make a mixture containing a known proportion of the metal wherewith you alloy the gold, ſuppoſe of 19 or 15 parts of gold, and one of ſilver, and letting the inſtrument ſettle in the water, mark the place where the ſurface of the fluid cuts the ſtem; then putting in another mixture, wherein the ſilver bears a greater proportion to the gold, as, ſuppoſe the former be an 18th or a 14th part of the latter, you may obſerve how much leſs than before this depreſſes the inſtrument; and ſo proceed to as many degrees of alloy as can be diſtinguiſh'd on the ſtem; always obſerving that the weight of each maſs in the air be exactly the ſame with that of the pure gold, which we ſuppoſe an ounce.

By the ſame method may be examin'd the different alloys of pure ſilver with copper, or other metal ſpecifically lighter than ſilver; and with a ſlight variation it will not be difficult to eſtimate in what degree ſeveral coins, whether ſilver or gold, are debaſed by the known ignobler metal mix'd in the piece propoſed, and this with greater exactneſs than by the touchſtone, or much more troubleſome and chargeable methods; and the ſame is applicable to other mixtures of metals.

A
Statical Hygroſcope.

SECT. I.

Various bodies propoſed, to diſcover the dryneſs and moiſture of the air.

HAVING had occaſion, amongſt other things relating to the air, to conſider its moiſture and dryneſs, I eaſily diſcern'd, they had no ſmall influence upon ſeveral bodies; and, among the reſt, upon thoſe of men; as the air we breathe, either paſſes from one of theſe fluid qualities to the other; or from one degree to another, in the ſame quality.

I therefore immediately conſider'd of a proper way to diſcover the changes of the air, as to moiſture and dryneſs; with their ſeveral degrees. For this purpoſe it ſeem'd, to me, that if a ſtatical hygroſcope could be contrived, it would be very convenient; and fit both to determine the degrees of the moiſture or dryneſs of the air; and to tranſmit the obſervations made of them to others. And conſidering further, that among bodies, otherwiſe well qualified for ſuch a purpoſe, that was likelieſt to give the moſt ſenſible informations of the changes of the air, which in reſpect of its bulk had the largeſt ſurface expoſed thereto; I pitch'd upon a fine ſpunge, as a thing which is portable, not eaſily diſſipated, but by its readineſs to imbibe water, ſeem'd likely to attract the aqueous particles it might meet with in the air, and which, by its great poroſity, has much more of ſuperficies, with regard to its bulk, than any body not otherwiſe leſs fit for the purpoſe.

But I here propoſe not the beſt hygroſcope that might be contriv'd, only one that is ſimple, cheap, contain'd and ſet up in a little room, eaſy to be made and tranſported, ſtatical, commodious, and applicable to various uſes.

In conſidering what bodies were the fitteſt to be employ'd for ſtatical hygroſcopes, I made trial of more than one, that ſeem'd not the leaſt promiſing. I know that common or ſea-ſalt will greatly relent in moiſt air; and ſalt of tartar much more: but then theſe ſalts will not eaſily part with the aqueous corpuſcles they have once imbibed; and are, in other reſpects, leſs convenient than a ſpunge. I made tryal alſo with lute-ſtrings, choſen very ſlender, that they might have the greater ſurface, in reſpect of their bulk; and thoſe I found very well to imbibe the moiſture of the air; but afterwards they did not anſwer my expectation. I alſo cauſed a cup to be turned out of light wood, which, that it might leſs burden a tender balance, had, inſtead of a foot, a little button, whereto a hair might be tied to ſuſpend it; and this cup being exceeding thin that it might have much ſurface expoſed to the air, proved ſo good a hygroſcope that I made ſeveral obſervations with it for a conſiderable time. It agreed alſo with many trials made on other occaſions, that white ſheeps leather, ſuch as is uſed for plaiſters, would be very convenient for my purpoſe. And indeed I found, by ſeveral obſervations, that if this leather were a ſubſtance as little liable to corruption as a ſpunge, it would be a fitter matter, than any other I had employ'd, for hygroſcopes.

A ſpunge made choice of.

But upon the whole, I found no body ſo convenient for my purpoſe, as a ſpunge; to try the poroſity whereof, we weigh'd out a dram of the fine ſort; and having ſuffer'd it to ſoak up what water it could, we held it in the air, till it dropp'd ſo very ſlowly, that we counted a hundred between the falling of two ſucceeding drops; then putting it into the balance wherein it had been weigh'd before, we found, that as its dimenſions were increaſed to the eye, ſo its weight increaſed upon the ſcale; amounting now to above two ounces and two drams; ſo that one dram of ſpunge, tho' it ſeem'd not altogether ſo fine as the portion we had choſen for our hygroſcope, imbibed and retained ſeventeen times its weight of water.

And turn'd into a hygroſcope.

Now a ſpunge is eaſily turn'd into a hygroſcope; for having weigh'd it when the air is of a moderate temperature, it only requires to be put into the ſcale of a good balance, ſuſpended on a fix'd and ſtable ſupporter, For the ſpunge being carefully counterpoiſed at firſt with a metalline weight, which alters not, ſenſibly, with the changes of the air, it will, by its riſing or falling, ſhew how much the neighbouring air is grown dryer or moiſter in the place where the inſtrument is kept. The weight of the ſpunge may be greater or leſs, according to the ſize and goodneſs of the balance, and the accuracy required in the diſcovery. For my part, tho' I have with very tender ſcales long employ'd but half a dram of ſpunge, I found it anſwer my expectation: and tho', when I uſed a bulk ſeveral times as great, in a ſtronger, but proportionably leſs accurate balance, I had ill ſucceſs; yet after trials with different quantities of it, I preferr'd that of a dram; as not being ſo heavy as to overload the finer ſort of goldſmith's ſcales, and yet ſufficient to diſcover changes conſiderably minute; turning, diſcernably, with a ſixteenth or twentieth part, and, manifeſtly, with half a quarter of a grain.

Experiments and obſervations made with it.

With ſuch hygroſcopes as theſe (wherein the balance ought to be ſtill kept ſuſpended and charged) I made ſeveral trials, ſometimes in the ſpring, ſometimes in the autumn, and ſometimes alſo in the ſummer and winter. Having once kept a dram of ſpunge ſuſpended, during a whole

Fig. 22. p. 310.

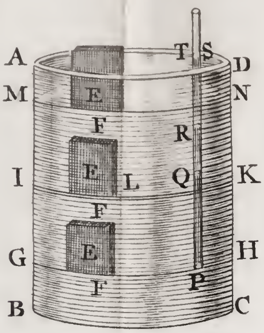


Fig. 23. p. 311.

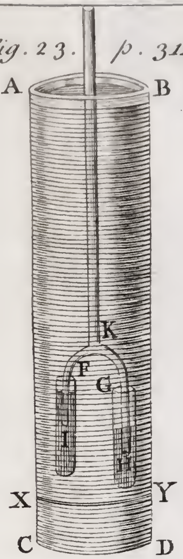


Fig. 24. p. 312.

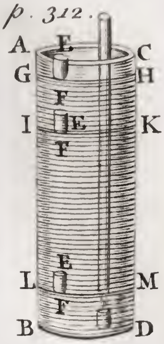


Fig. 25. p. 312.

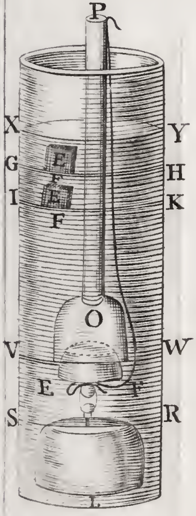


Fig. 26. p. 318.

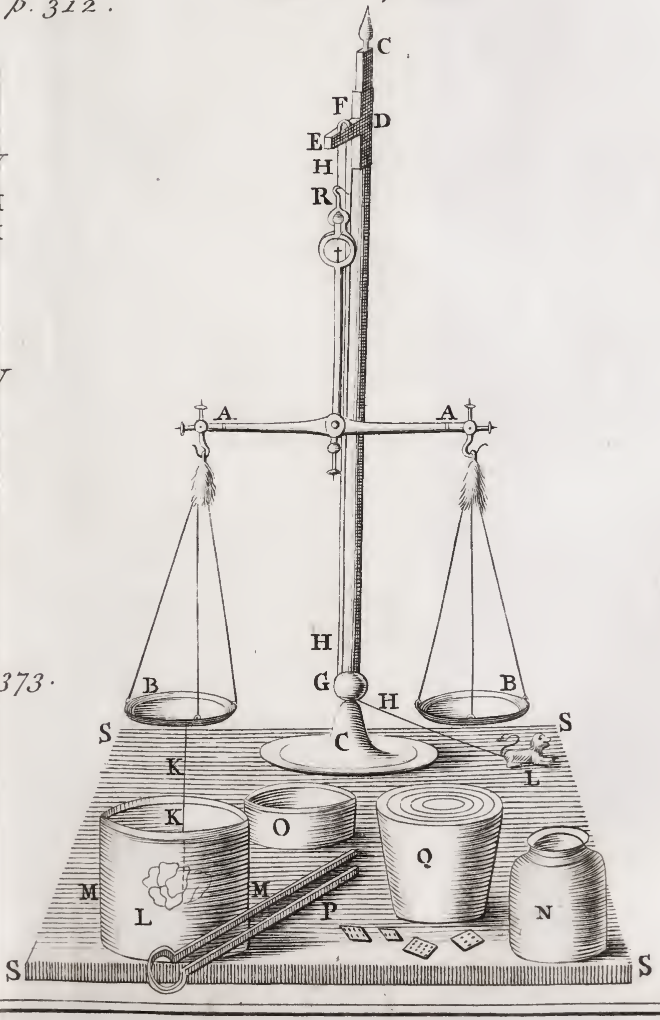


Fig. 27. p. 372.

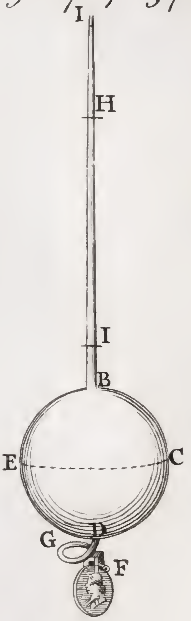


Fig. 28. p. 373.

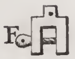
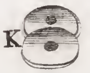


Fig. 29. p. 373.



J Mynde sc.

whole ſpring, and a great part of the preceding winter, and ſubequent ſummer, I was well rewarded by the obſervations it afforded me.

Now the uſe of a hygroſcope is either general, or particular.

The uſe of hygroſcopes.

The general uſe is, to eſtimate the changes of the air, as to moiſture and dryneſs, by ſuch ways of meaſuring them, as are eaſy to be known, provided and communicated. 'Tis obvious that our ſtatical hygroſcope has the advantage of the common ſorts, in theſe ſeveral reſpects.

There is however, one great imperfection, which all that I have been acquainted with, are liable to; the want of a ſtandard of the dryneſs and moiſture of air; by relation to which, hygrometers ſhould at firſt be adjuſted, and afterwards compared with one another: tho' this inconvenience may poſſibly be remedied. In the mean time, even our ſlight inſtrument will, in many reſpects, prove very ſerviceable.

The firſt uſe of the ſtatical hygroſcope, to ſhew the different variations of weather in the ſame month, day, and hour.

And firſt, it will, on many occaſions, be uſeful to ſhew, both that the air is uſually leſs moiſt at one part of the artificial day, than at any other; and at what particular time of the day, or night, it moſt commonly is ſo. I remember, when the weather was at a ſtand, I uſually obſerved, that the ſpunge had manifeſtly gain'd in the night, tho' it were kept in a bed-chamber, and grew lighter again between the morning and noon. But this obſervation, which was made towards the end of winter, would not hold, in caſe froſty nights, or ſome other powerful cauſe intervened. 'Twere proper, alſo, to obſerve, whether there be not a correſpondence betwixt the hygrometer, and barometer; and if there be, in what kind of weather, or conſtitution of air, it is moſt, or leaſt to be diſcern'd. And this enquiry ſeems the more dubious, becauſe the ſame changes of the atmoſphere may, upon differing accounts, have either the like, or quite contrary operations upon theſe two inſtruments. For, in ſummer, when the atmoſphere is uſually heavier, the hygroſcope is uſually lighter: ſome ſtrong winds, as with us the north-weſt, may make both the atmoſphere, and barometer lighter; whereas, ſoutherly winds, eſpecially if accompanied with rain, often make the atmoſphere lighter, and the ſpunge heavier. On the other ſide, I obſerve, that eaſterly winds, eſpecially when they begin to blow in winter; tho', by reaſon of their dryneſs, they make the hygroſcope lighter; yet, at leaſt here, at the weſt-end of *London*, they commonly ſhew the air, by the barometer, to be heavier. It were, likewiſe, fit to be obſerved, particularly by thoſe who live on the ſea-coaſt, whether the daily ebbing, or flowing of the ſea, do not ſenſibly alter the weight of the hygroſcope. It would alſo, be very convenient, to take notice, at what times of the day or night, *cæteris paribus*, the air is moſt damp and moſt dry; and not only in ſeveral parts of the ſame day, but in ſeveral days of the ſame month; eſpecially on thoſe, wherein the full or new moon happens: And this ſeems a more hopeful way of diſcovering, whether the full moon diffuſes a moiſture in the air, than thoſe vulgar traditions of the plumpneſs of oyſters, ſhell-fiſh, the brains of ſome animals, the marrow in their bones, *&c.* It may alſo be noted, whether monthly ſpring-tides, eſpecially when they happen near the middle of *March* or *September*, have any ſenſible effect, upon our inſtrument.

A ſecond uſe, to ſhew how much one year, and ſeaſon, is dryer and moiſter than another.

The hygroſcopes made of an oaten beard, after ſome months, begin to dry up and ſhrink; ſo that their ſenſe of the varying degrees of the moiſture of the air, is not ſo ſudden as before; and the informations they give of the degrees of it, eſpecially towards the outmoſt bounds of their power, to ſhew the air's alterations, become more and more irregular. But durableneſs, and the other convenient qualifiations of our ſpunge, making its capacity of doing ſervice more laſting, may the better help us to compare the greateſt moiſture and dryneſs, both of the ſame ſeaſon, and of the ſeaſons of one year, with the correſpondent ones of another. And, if the weight of the ſpunge, at a proper time, when the temperature of the air is neither conſiderably moiſt, nor conſiderably dry, be taken for a ſtandard; a perſon may, by computing for how many days, at ſuch an hour, and how much at that hour, it was heavier or lighter than the ſtandard; and alſo, by comparing the reſult of ſuch an account, in one year, with the reſult of the like account in another, be aſſiſted to make a more particular, and near eſtimate of the differing temperature of the air, as to moiſture and dryneſs, in one year than in another; and in any correſpondent ſeaſon, or month, aſſigned in each of the two years propoſed. And, how far the compariſon, or continuance of ſuch obſervations, both in the ſame place, and alſo in different countries, and climates, may be uſeful to phyſicians, in reference to thoſe diſeaſes, where the moiſture and dryneſs of the air have much intereſt; and the husbandman, to foreſee what ſeaſons will prove friendly, or unkind, to particular ſoils and vegetables; it muſt be the work of time to teach us: in the interim we have no reaſon to deſpair, that the uſes to be made of ſuch obſervations, may prove conſiderable. And if, by the reſult of many obſervations, men may be enabled to foreſee the temperature of a year, or even of a ſeaſon, it may advantage other profeſſions of men, who receive much profit, or prejudice, by the great dryneſs, or exceſſive moiſture of the ſeaſons. Not to mention thoſe who cultivate hops, ſaffron and other plants, that are tender, and bear a great price; ſuch a foreſight, as we are ſpeaking of, may be of great uſe to ſhepherds, who in ſeveral parts of *England*, often ſuſtain great loſſes, by the rot of ſheep; which uſually happens through exceſs of moiſture, in certain months of the year. And, in order to lay foundations, whereon to build predictions, it may not be amiſs to regiſter the number, magnitude, and duration, of the conſiderable ſpots, that may at this, or that time of the year, happen to appear, or be diſſipated, on, or near the ſun; or to take notice of any extraordinary abſence of them; and to obſerve whether their apparition, or diſſipation, produce any changes in the hygroſcope. This, indeed, I ſhould not venture to propoſe, but that eminent aſtronomers have caſually obſerved great dryneſs to attend the extraordinary abſence, or fewneſs of the ſolar ſpots. And ſuch perſons as are aſtrologically given, may extend their curioſity, in the uſe of this inſtrument, to obſerve, whether eclipſes of the ſun and moon, and the great conjunctions of the ſuperior planets, have any remarkable effect upon it.

I have

The third uſe, to diſcover and compare the changes of the temperature of the air, made by winds, froſty, ſnowy, and other weather.

I have found, by removing the ſpunge into the wind, that it ſoon received a very conſiderable alteration, in point of weight; as it, alſo, did, when remov'd out of a room, into a garden, where the ſun ſhone: for, tho' the ſeaſon were not warm, it being then the month of *January*, yet in three quarters of an hour, the ſpunge loſt a 24th part of its weight. We may alſo, in ſome caſes, uſefully ſubſtitute a broad piece of ſheep's-leather, by diſplaying it to the wind. For this having, by reaſon of its thinneſs, in proportion to its breadth, a very large ſuperficies immediately expoſed to the wind, we found it to be conſiderably alter'd thereby; ſo that half an ounce of it, well-prepared, being kept, for an hour, in a place where the ſun-beams might not beat upon it, did, in a ſtrong wind, vary an eighteenth part of its original weight. But tho' I think it very poſſible to make ſnch obſervations of the temperature of particular winds, as will frequently prove ſo true as to be uſeful, at leaſt, to thoſe who live in the places where they are made; yet I am of opinion, rhat to be able to ſettle general rules, to determine, with any certainty, the quality of winds, according to the points of the compaſs, from whence they blow, great warineſs will be required: and he who has not ſome competent skill in phyſics, and coſmography, muſt be very ſubject to miſtake in forming his rules.

For, winds that blow from the ſame quarter, are not, in ſome countries of the ſame quality they are in moſt others; but participate much of the nature of the region over which they blow, in their paſſage to us. At *Archangel* they obſerve, that tho' a northerly wind, almoſt every where elſe, without the tropics, produces froſt in winter, it is there attended with a thaw; probably becauſe this wind comes over the ſea, which lies north from that place: and on the contrary, a ſoutherly wind blowing over a thouſand, or twelve hundred miles, of frozen land, rather increaſes the froſt, than brings a thaw. The northern winds, which are elſewhere drying, are ſaid in *Egypt* to be moiſt. And we are told, that the northern wind in *Egypt*, is the moiſteſt. Monſieur *de Serres*, ſpeaking of the changes of the air, with regard to huſbandry, in ſeveral parts of *France*, informs us, that about *Tholouze*, the ſouth-wind dries the ground, and the north gives rains. On the contrary, from *Narbonne* to *Lyons*, all over *Provence* and *Dauphine*, the latter cauſes dryneſs, and the other brings moiſture.

But, farther, the vehemence or faintneſs of the wind, though blowing over the ſame country, may much diverſify its operation on the hygroſcope; and the ſame wind, which, when it blows but moderately, appears moiſt by the hygroſcope, may, when vehement, or impetuous, make the inſtrument grow lighter; diſcuſſing and driving away more vapours, by the agitation of parts it makes in the ſpunge, than is balanced by thoſe aqueous vapours that are brought along with it. And froſty weather, as we found often, made the hygroſcope grow lighter, even at night; but ſnowy weather, which laſted not long, added ſomething to the weight of the ſpunge: and it has been obſerved, that miſts and foggy weather add a weight to it.

Nay, 'tis probable, that a tranſient cloud, in fair weather may be manifeſtly obſervable by our inſtrument. For, the 9th of *September*, being for the moſt part, a very fair ſun-ſhiny day, tho', about ten a clock in the morning, the ſun ſhone brightly, the ſpunge began to preponderate; which unexpected phenomenon made the perſon, in whoſe chamber the inſtrument was, look out at the window, where he diſcovered a cloud that darken'd the ſun; but, after a while, this being paſt, the balance return'd to an equilibrium. And, I have more than once, or twice, obſerved, eſpecially in ſummer, that when the air grew heavier, the hygroſcope either continued at a ſtand, or perhaps, grew lighter; as if, when ſuch caſes happen, the effluvia that get into the air, either from the terreſtrial, or ſome other mundane globe, were not fit, like vapours, to enter and lodge in the pores of the ſpunge; and ſo were corpuſcles of another nature; with which, when we find by the baroſcope, that the air is plentifully ſtock'd, it may be worth while to obſerve, whether any, and what kind of meteor, will commonly be ſignified and produced.

A fourth uſe, to compare the temperature of different houſes, and different rooms in the ſame houſe.

As it is of great uſe, both in reſpect of mens health, eſpecially if they be of a tender, or ſickly conſtitution; and for the conveniency of the keeping fleſh, ſeveral ſorts of wares, *&c.* that are ſubject to be damaged by moiſt air; ſo this may be readily and manifeſtly derived from our inſtrument. For, by removing it into ſeveral houſes, or into ſeveral parts of the ſame houſe, and letting it ſtand, for a competent time, in each, we have often obſerved a notable difference; of which I ſhall here give two or three inſtances.

Having, upon the 10th of *October*, taken a piece of fine ſpunge out of a cabinet, and clip'd it till it weigh'd juſt half a dram, in a nice pair of ſcales, and a warm room; we afterwards removed them into a neighbouring room, deſtitute of a chimney. This ſtatical hygroſcope, was, *October* 12. at night, remov'd into the former room, and the ſpunge found to have gain'd three grains, and an eighth; and, conſequently, more than a tenth part of its firſt weight: but, being ſuffer'd to ſtand in this warm room; in leſs than twelve hours, it loſt a grain, and about one eighth of its former weight; tho' the time it ſtood in this room were, for the moſt part night, and the weather rainy.

At another time, we took a piece of very fine ſpunge, which had formerly weigh'd juſt a dram; but having been, for many months, kept in a very warm room, it was grown much lighter: for, removing it into an upper-chamber, in a neighbouring houſe, and weighing it in tender ſcales, in the evening 'twas found to want four grains, and three quarters, of a dram; and tho' there was a fire in the room, and the ſcales ſtood not far from it, yet, in a ſhort time (the day being foggy and rainy) the ſpunge viſibly depreſs'd its ſcale three eighths, and the next morning, was found to want but one grain and a half, of a dram; ſo that it had gain'd about three grains and a quarter: and the following evening, being the ſecond of *January*, it weigh'd one dram, a grain, and almoſt a half. So that in about one natural day, the ſpunge had acquired ſix grains from the moiſture of the air;

that

that is, a tenth part of its firſt weight, and a greater proportion to the weight it had the day before. The third of *January*, the weather being yet moiſt, the weight exceeded two grains; but about three or four of the clock in the afternoon it began to loſe of that great weight, which diminiſh'd more by the next morning, the weather having chang'd that night, and become froſty.

A dram of ſponge that had for ſeveral weeks been kept in a dry room, was (*January* 10.) carried out into a room where a fire is not uſually kept, the weather being extraordinary foggy; the next morning it was brought into the former room, and tho' now the weather was clear, it appear'd to have gain'd about 11 grains in weight; yet it preſently loſt two grains by ſtanding in this room.

The fifth uſe, to obſerve in a chamber the effects of the preſence or abſence of a fire.

In ſmall chambers I have often obſerv'd a moderate fire to alter the weight of the inſtrument, plac'd at a diſtance from it, after it had been well kindled for a very little while; but in wet weather, if the fire were not ſeaſonably recruited with freſh fewel, the decay of it would ſoon begin to be diſcernible by the inſtrument.

The ſixth uſe, to keep a chamber in any aſſign'd degree of dryneſs.

Suppoſing the alteration of weight in our ſponge to depend only upon the degree of the moiſture of the air, if a convenient part of the room be choſen for the hygroſcope, and it be kept conſtantly there, it is eaſy from time to time to perceive when it will be requiſite to increaſe or moderate the fire, ſo as to keep the ſponge at that weight it was of when the temperature of the air of the chamber, as to dryneſs and moiſture, was ſuch as is deſired. I have ſometimes obſerv'd with pleaſure, how manifeſt and great a change in the weight of our ſponge would be made when the room was waſh'd, and for a conſiderable time after, tho' a large fire was kept in it to haſten its drying.

Beſides theſe ſeveral uſes of our hygroſcope, there may poſſibly be many others; and perhaps, by a little alteration and improvement, it might be made capable of ſhewing ſome difference betwixt ſteams of differing natures, as thoſe of water, ſpirit of wine, chymical oils, or new kinds of ſubſtances in the air, wherein I ſuſpect there may ſometimes be diſperſed great plenty of unknown corpuſcles; for I have, more than once, obſerved the hygroſcope not to be affected with the alteration of weather, anſwerably to what the manifeſt conſtitutions or variations of it ſeem'd plainly to require; whether unobſerv'd particles perform'd this, by making the other ſteams, in point of figure or ſize unfit to enter the minute pores of the ſpunge, or by diſſipating or otherwiſe procuring the avolation of more of the watery particles than they could countervail, I ſhall not now examine; and perhaps too by aſſociating this inſtrument with the thermoſcope, baroſcope, &c. it might be ſo improv'd as to help us to foreſee many conſiderable things, that either are themſelves changes of the air, or conſequences thereof; for inſtance, ſickly and healthful conſtitutions of the air, barren or plentiful ſeaſons in particular places or countries, ſtrong hurricanes, earthquakes, inundations, and their ill effects; and eſpecially thoſe accidents that depend much upon the ſurcharge of the air, with other exhalations and moiſt vapours which before-hand operate ſenſibly upon our inſtrument, and therefore may be diſcernable by it long before they arrive at that height which makes them formidable meteors; and if it were but the foretelling approaching rain, this very thing may on many occaſions prove very ſerviceable, and recommend our inſtrument, which often receives much earlier impreſſions from the ſteams that float in the air than our ſenſes do; ſo that I have been able to foretel a ſhower of rain, eſpecially in dry weather, for a conſiderable time before it fell.

And here I muſt advertiſe, with regard to our inſtrument as well as the barometer, that if a theory or hypotheſis, in itſelf rational, be found agreeable to what happens the moſt uſually in obſervation, it ought not to be rejected or laid aſide, tho' ſometimes particular inſtances may call it in queſtion; for it is very poſſible that the theory or hypotheſis may be as good as a wiſe man would require, about ſo mutable a ſubject as the weather; and the cauſe aſſign'd by the hypotheſis may really act ſuitably to what that requires, tho' a contrary effect enſue, by reaſon of this cauſe being accidentally over-rul'd by ſome more powerful agent, which for that time invades the air. Thus we know that tides do in general correſpond with the motions of the moon, and generally ebb and flow at ſuch times and in ſuch meaſures as the theory grounded on that correſpondence requires; yet ſeamen find that in this or that particular harbour, or mouth of a river, fierce contrary winds, great land-floods, and other caſual cauſes, ſometimes both very much diſturb the regular courſe of the tides, and either increaſe or leſſen them.

SECT. II.

Inſtances of the power of the moiſture in the air at all ſeaſons.

STill farther to recommend the uſe of hygroſcopes, I ſhall here ſhew, that the quality they acquaint us with may be very conſiderable, not only upon mens healths, but upon ſubjects far leſs tender, and leſs curiouſly contriv'd than human bodies.

Upon animal ſubſtances.

That the moiſture of the air may have no ſmall influence, and uſually a bad one upon mens healths, tho' experience did not ſo often teach it, I ſhould directly infer from its operation upon the dry and firm parts of animals; and even in thoſe caſes, where, for want of time, or other impediments, it cannot produce any ſenſible degree of putrefaction.

The ſkins of animals may eaſily be invaded by the moiſt particles of the air, becauſe of the great number of their pores, which may be concluded from their hairineſs, their ſweat, or both; and I lately obſerv'd, that ſheeps leather imbibes the moiſture of the air, and increaſes in weight upon it, as plentifully as almoſt any body I have expos'd to it.

But to ſhew that much cloſer membranes, and which nature made to be impervious to ſuch a liquor as urine, may be affected by the vapours of the air, I ſhall add, that having purpoſely taken pieces of fine well-blown bladders, and, as far as appeared, of a very cloſe texture, and counterpois'd them in a good balance, I found they would conſiderably increaſe their weight in moiſt, and loſe it again in dry weather; ſo that I might have employ'd the moſt membranous part of a bladder to make a ſtatical hygroſcope.

And tho' bones are the firmeſt and moſt ſolid parts of animals, and as it were the pillars by which their fabric is ſuſtain'd, yet even theſe may be pierc'd and ſenſibly affected by the moiſture of the air; for having cauſed the ſkeleton of a human body to be ſo ſet together, that by the help only of ſlender wyres artificially order'd, the natural motions which the muſcles make of the bones might be well imitated, I obſerv'd, that tho' in dry and fair weather the flexures of the limbs might be readily made, yet in very moiſt weather the joints were not eaſily bent, as if the parts were grown ſtiff and rigid; which ſeem'd to proceed from the moiſt particles of the air, which having plentifully inſinuated themſelves at the pores into the bones, had every way diſtended them, and thereby made the parts bear harder againſt one another at the junctures or articulations.

Upon vegetables.

But farther, the moiſture of the air may exerciſe a conſiderable force even upon inanimate and inorganical bodies; even well-ſeaſon'd wood will ſuffer a kind of divulſion of a multitude of its parts, and be manifeſtly diſtended by aqueous corpuſcles getting into its pores. Having procured a piece of ſound and ſeaſon'd wood, of about an inch in diameter, to be turn'd exactly cylindrical, and alſo a ring of braſs or ivory to fit it, ſo that it might with eaſe be put on and taken off, we ſet the piece of wood in fair water, and left it to ſoak there for ſeveral hours, at the end of which it was viſibly ſwell'd, ſo that the ring, which was adjuſted to it before, was manifeſtly too little to go again upon it, or with its orifice to cover the whole baſis of the diſtended cylinder; which afterwards being dry'd in the air, ſhrunk into a capacity fit to enter the ring again. And in this experiment I took notice, that the great diſtenſion of the wood was not produc'd all at once, or ſoon after it was put into the water, but it ſwell'd by degrees, and lay ſoaking there for ſeveral hours before it arriv'd at its utmoſt dimenſions, the aqueous corpuſcles requiring it ſeems ſo much time to inſinuate themſelves ſufficiently into the wood; which argues, that the internal parts were likewiſe affected; tho' when theſe came to ſwell, they had a great thickneſs of wood about them, to hinder their dilatation.

And to manifeſt that even in ſummer there are aqueous vapours floating in the air, within ſome days of *Midſummer*, and in clear ſun-ſhiny weather, we took a pint-glaſs bottle, and having put into it a convenient quantity of water, plac'd it and four ounces of beaten *ſal-armoniac* in one ſcale of a good balance, and a counterpoiſe in the other, and then putting the ſalt into the water, I obſerv'd, that tho' for a while the equilibrium remain'd, yet when the frigorific mixture had ſufficiently cool'd the outſide of the bottle, the roving vapours of the air that chanc'd to paſs along the ſurface of the veſſel, were by the contact of that cold body arreſted, and turn'd into a kind of dew, which from time to time made the ſcale that held the glaſs, preponderate more and more; and at length the drops growing larger, ran in ſmall rivulets down the ſides of the glaſs; ſo that in leſs than an hour the condenſed ſteams amounted to near a dram, which weight was afterwards much increas'd, within about two hours more.

And that the multitude, figures and motions of theſe particles, in the form of vapour, may enable them to diſplay no ſmall force in their operations on ſome bodies, appears from the breaking of the ſtrings of muſical inſtruments, firſt brought to a ſtrong tenſion, in rainy weather; for the vapours which then wander thro' the air, inſinuating themſelves into theſe ſtrings, diſtend and ſwell them, and thereby endeavour to ſhorten them, ſo forcibly, that they often break with a ſmart noiſe and great violence; and if we conſider how great a weight ſome of the bigger ſtrings of baſs-viols, that have been obſerv'd to break in moiſt weather, require to ſtretch them till they break, this effect of the moiſt air will appear to require a very great force.

And even upon metals and minerals.

Beſides, an ancient muſician and a great organ-maker, inform'd me, that the wyre-ſtrings of muſical inſtruments will, for the moſt part, be out of tune in wet weather, the ſtrings then generally affording their notes ſharper than uſual; and that upon great changes of weather ſeveral organs would (after they had been long tun'd) grow out of tune, the wooden pipes ſwell, and the metalline ones be untun'd.

But if bodies be of ſuch a conſtitution as to aſſiſt the operation of the moiſt air, the penetrancy and efficacy of it may be ſtill more conſiderable; for there are ſome kinds of thoſe marcaſites that yield vitriol, which whilſt they lie under ground, or are cover'd with the ſea-water on ſhores, retain a ſtony hardneſs, and are often taken for mere ſtones; yet credible perſons have caſually obſerv'd, that theſe being expos'd to the air, would in tract of time be ſo penetrated by the moiſt particles of it, as to ſwell and burſt; that this will happen to ſuch kind of ſtones by the help of rain, experience has perſuaded me; and that it may alſo happen even to very hard and ſtone-like marcaſites, when they are merely expos'd to the air, I am apt to think, from ſome trials of my own; for from ſhining marcaſites, tho' kept but in my chamber-window, I have had vitriolate efflorеſcencies, which ſeem'd to be produc'd by the action of the piercing moiſture of the air upon the mineral; and I remember, that very hard and heavy lumps of a marcaſitical ſubſtance, which ſeem'd to be ſtony, were ſo diſpos'd to be wrought on by the air, that tho' kept in my own chamber, or other cover'd places, yet in a ſhort time were ſo penetrated by the moiſt corpuſcles of the air, that they burſt into many pieces, which fell from one another, and ſeveral of the divided portions might eaſily be crumbled betwixt ones fingers; and of ſome of theſe I have obſerv'd, that a vitriolate ſubſtance was produc'd more plentifully in their innermoſt parts, than on or near their outſide; ſo that conſidering how great an external force would have been requiſite to make ſuch a comminution of minerals thus ſolid and hard, it is obvious that the air's moiſture is capable, when it meets with fitly diſpos'd bodies, to exerciſe a far greater force than is uſually imagin'd.

The chord-hygrometer.

And thinking it probable, that ropes themſelves would conſiderably imbibe and part with the moiſture of the air, and that ſo as to ſhrink in rainy weather, tho' clogg'd with a weight faſten'd at the lower end; I firſt caus'd a rope, that was about twenty yards in length, but of no great thickneſs, to have one of its ends faſten'd to an immoveable body, at a convenient height from the ground, and a pully to be faſten'd to another ſtable body, at the diſtance of eighteen or twenty yards from the firſt, that the rope reſting upon the pully, lay almoſt horizontally. But to the end of that part of the rope, which, from the pully, reach'd within two or three feet of the ground, was faſten'd, by a ring, a leaden weight of fifty pound. To this was, alſo faſten'd a light index, placed horizontally, whoſe end moved along an erected board, which by tranſverſe lines was divided into inches and parts of inches, reaching a conſiderable way upwards and downwards, that the index might, within thoſe bounds, have room to play, according to the alterations of the weather.

It being now ſummer, we placed this apparatus in a garden, tho' partly under a pent-houſe, that the rope might be more expoſed to the air, than it would have been within doors; and in two or three days, the weight had brought the rope to the utmoſt ſtretch that was able to give it: after which, it began manifeſtly to ſhrink and lengthen, according to the weather. So that once looking, over-night, upon the ſuſpended weight and marking how low it reach'd upon the divided board; when I came again the next morning, a great part of the night having been rainy, I found the cord ſo ſhrunk, that the weight was rais'd above five inches: yet the day growing dry and windy, and ſometimes warm, the weight had at night, ſtretched the rope more than the moiſture had contracted it before.

Afterwards having procured a far greater weight, I ſubſtituted it in the place of the other; and having ſuffer'd it to ſtretch the rope as far as it could, I regiſter'd ſome obſervations about it; two whereof I ſhall here ſet down.

June 4. At half an hour after nine at night, I look'd upon the hundred weight that hung at the bottom of the rope, the weather being then fair; and a mark being put at that part of the erected board, where the bottom of the weight touch'd, I perceiv'd the sky a while after to be cloudy and overcaſt, but without rain: wherefore going to view the weight again, I found it in an hour and a quarter to have riſen a quarter of an inch or more.

June 6. Yeſterday the weight was obſerved to reſt at the eleventh inch of the erected board. This morning about eight, I found it riſen about half a quarter of an inch above the eighth; the morning being cloudy, tho' the ground very dry and duſty. The weather being overcaſt, within leſs than an hour afterwards, (ſome ſcatter'd drops of rain then beginning to fall) I found the weight riſen about half an inch above the eighth mark.*

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* Dr. *Jurin*, to gain materials for an exact meteorological hiſtory, deſires the curious of all parts to keep a diary of the weather, and to ſend in their accounts of it, annually, to the ſecretaries of the Royal Society; where being compared with thoſe made

Hence the force of the air's moiſture is not ſmall, ſince it could raiſe an hundred weight, tho' the chord meaſured, in diameter, but about the third part of an inch.

made by the order of that illuſtrious body, the reſult may be publiſh'd in their Tranſactions. As ſuch a work, for its great Uſefulneſs, is higly deſirable, we ſhall here give the particulars, which the Doctor deſires ſhould be principally inſerted in ſuch a journal.

And, 1ſt, he requeſts, that the heights of the barometer, thermometer, and the ſtate of the hygroſcope, be obſerv'd, at leaſt once every day; from what point of the compaſs the wind blows, and with what force; the appearance of the sky, and the quantity of rain or ſnow, that fell ſince the preceding obſervation was made.

2. That in caſe of a ſtorm, its beginning, increaſe, height, decline, and ceſſation, be accurately remark'd, with the preciſe times wherein each happen'd, and the heights of the barometer correſponding to thoſe times.

3. That the common barometer, artificially fill'd, be made choice of, the tube being $\frac{1}{4}$ or $\frac{1}{3}$ inch in diameter; and the veſſel containing the ſtagnant mercury, 8 or 10 times wider than the tube.

4. If a portable barometer be employ'd, the Doctor recommends thoſe of that excellent artiſt Mr. *Hauksbee*, in *Crane-Court, Fleetſtreet*; who alſo (ſays the Doctor) makes moſt exquiſite thermometers.

5. If any other thermometer be uſed, the Doctor deſires that its ſituation, ſtructure, ſcale, and the name of the perſon who made it, may be ſet down in the diary; and that this inſtrument be placed in a room that looks north, where a fire is ſeldom or never made.

6. That they may be the better compared together, the Doctor farther deſires, that the firſt column may exhibit the day and hour of the obſervation, old ſtile; that the ſecond may ſhew the height of the mercury in the barometer, in inches of an *Engliſh* foot, and their decimal parts; that the third column may contain the degree and decimal parts of a degree of the ſpirit in the thermometer; that to the fourth be aſſign'd the wind, with its degrees of force, which may be expreſſed by the figures 1, 2, 3, 4, and 0 for a perfect calm. That the fifth may contain the appearance of the heavens, and a ſhort hiſtory of the weather. That the laſt may give the height of the rain or ſnow when diſſolved, which fell ſince the preceding obſervation, in *Engliſh* inches, and their decimal parts.

The quantity of rain, this very learned gentleman tells us, may be eaſily computed by means of a funnel two or three feet wide, ſet in a cloſe narrow-mouth'd veſſel to receive the water falling in an open place, and a cylindrical meaſure divided into inches and decimal parts. But the diameter of the cylindrical meaſure ſhould be 10 times leſs than that of the funnel; whence an inch height of water in the meaſure, will appear to have fallen to the height of $\frac{1}{100}$ inch upon the funnel; and ſo for other decimal parts of an inch. At the end of every month and year, the Doctor deſires may be ſet down the mean menſtrual and annual height of the barometer and thermometer, as likewiſe the ſum of all the heights of the rain that has fallen each month or year. See *Philoſ. Tranſ.* N° 379. p. 427.

A Specimen of this Diary take as follows.

Dies & Hora 1723. Nov. St. V.	Barom. alt. dig. dec.	Therm. alt. gr. dec.	Vent.	Tempeſtas.	Pluvia. dig. dec.
1. 8 *a. m.*	29.75	49 . 6	S. W. 1	Cœlum nubibus obduct.	0.035
4 *p. m.*	29.56.	47 . 3	S. W. 2	Imbres interrupti. Sol per vices intercurrens	0.043
2. 7½ *a. m.*	29.24	48 . 5	S. 1	Pluvia fere perpetua	0.725
3. 9 *a. m.*	29 95	49 . 7	N. 1	Cœlum ſudum	0.032
5 *p. m.*	30. 4	49 . 2	N. 1	Cœlum ſudum	0.000
4. 7 *a. m.*	29. 9	47 . 0	S. W. 1	Nubes ſparſæ	0 000
10	29. 7	46 . 2	S. W. 2	Imbres intercurrentes	0.103
12	29. 4	45 . 0	S. 3	Cœlum nubibus undique fere tectum	0.050
3 *p. m.*	28. 8	46 . 0	S. 4	Nubes ſparſæ	0.000
5	28. 6	47 . 2	S. W. 4	Eadem Cœli facies	0 000
7	28. 9	48 . 0	S. W. 2	Pluit	0.000
9	28. 9	48 . 2	0	Pluvia fere perpetua	0.305
5. 7 *a. m.*	29. 7	53 . 4	N. E. 1	Sudum. Gelu.	0.250

Fire and Flame

Weigh'd in a

BALANCE.

SECT. I.

Experiments to ſhew that flame may incorporate with ſolid bodies, ſo as to increaſe their weight and bulk.

1. A Piece of copper-plate, not near ſo thick as a half-crown, weighing two drams and 25 grains, being plac'd horizontally in a crucible, whoſe bottom had a little hole in it for the fumes to get out at, ſo that it could not be remov'd from its poſition, nor be eaſily made to drop down and loſe its level, tho' the crucible were turn'd upſide down, about an ounce and half of common ſulphur was put into a taller and broader crucible, and that wherein the copper ſtuck inverted into the orifice of it, that ſo the ſulphur being kindled the flame might reach the plate, and have ſome vent beyond it at the hole above-mention'd; the brimſtone burn'd about two hours, in which time it ſeem'd all to have been reſolv'd into flame, no flowers of ſulphur appearing to have ſublim'd into the inſide of the upper crucible; and tho' the copper-plate were at a conſiderable diſtance from the fir'd ſulphur, yet the flame ſeem'd to have actually penetrated it, and to have made it viſibly ſwell and grow thicker; which appear'd to be done by a real acceſſion of ſubſtance, ſince after we had wip'd off ſome little adhering *ſordes*, and with them ſeveral particles of copper that ſtuck cloſe to them, the plate was found to weigh near 32 grains more than at firſt, and conſequently had increas'd its former weight by above a fifth part.

2. Having, by refining one ounce of ſterling ſilver with ſalt-petre, reduc'd it to 7 drams, or ſomewhat leſs, we took a piece of it that weigh'd 58 grains, and ordering it as the copper-plate in the former experiment; after the flame of above an ounce and a quarter of ſulphur had for about an hour and half beat upon it, the ſilver plate ſeem'd

to the eye ſomewhat ſwell'd, and the lower ſurface of it next the flame was brought to a great ſmoothneſs, the weight being increas'd to one dram, five grains, and three quarters; which acquir'd weight falling ſo ſhort of that gain'd by the copper, the difference may perhaps be attributed to the cloſeneſs and compactneſs of the ſilver, argu'd by its being heavier *in ſpecie* than copper, or to the greater diſpoſition of the pores of copper to be wrought on by the fiery menſtruum *.

Nor is it ſtrange that the particles of flame, which are either of a ſaline or a piercing terreſtrial nature, being wedg'd into the pores, or being brought to adhere very faſt to the little parts of the bodies expos'd to their action, the addition of ſo many little particles, that want not gravity, ſhould, becauſe of their multitude, be conſiderable upon a balance, tho' a few of them would have no viſible effect.

That fire may do the ſame.

3. Into a very ſhallow crucible we put an ounce of copper-plates, and ſet it in a cupelling furnace, where it was kept for two hours; and then being taken out, we weigh'd the copper, which had not been melted (having firſt blown off all the aſhes) and found it had gain'd 30 grains.

4. We took an ounce of copper filings, and putting them upon a very ſhallow crucible, and under a muffler, we kept them there for about 3 hours; and afterwards taking them off, we found them of a very dark colour, not melted, but cak'd together in one lump, and increas'd in weight (the aſhes and duſt being blown off) no leſs than 49 grains; part of which increaſe, above that obtain'd by the copper-plates, may probably be due to the longer time that in this experiment the filings were kept in the fire.

5. We weigh'd out an ounce of ſmall lumps of hartſhorn, that had been burn'd till they appear'd white, and having put them into a crucible, and kept them in a cupelling furnace for 2 hours, we found when they were taken out that they had loſt 6 or 7 grains of their former weight, perhaps becauſe the internal parts of ſome of them might not be exquiſitely calcin'd, but retain'd ſome oleaginous, or other volatile ſubſtance; or becauſe having omitted to burn them well before they were weigh'd, they might ſince their firſt calcination have imbib'd ſome moiſt particles of the air; for having kept them a while in the ſcale wherein they were weigh'd, they did within 2 or 3 hours make it preponderate. At the ſame time with this hartſhorn we alſo put in one ounce of well-heated brick, and kept that likewiſe in the furnace for above 2 hours, at the end of which,

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* M. *Muſchenbroeck* counterpoizing a priſm of iron that weigh'd three pound, in ſcales which would turn with $\frac{1}{20}$ grain, afterwards heated the priſm red hot; when examining it again by the ſame balance, he ſtill found it exactly three pound; whence he juſtly concludes, that it had gain'd in weight from the fire; for the experiment being made in the open air, and the metal being expanded by heat, it is plain that the ſpecific gravity of the heated metal was decreas'd by the expanſion, ſo as to be then lighter, with reſpect to the air. *De materiâ ſubtili*, p. 8, 9.

STATICS. weighing it whilſt it continued hot, we did not find it to have either ſenſibly got or loſt; but, ſome time after, it ſeem'd, upon the balance to have imbibed a little moiſture from the air.

6. Upon a good cupel, we put an ounce of *Engliſh* tin, of the better ſort; and having placed it in the furnace, under a muffler, tho' it preſently melted, yet it did not forſake its place, but remain upon the concave ſurface of the cupel, till, at the end of about two hours, it appear'd to have been well calcin'd; and then being taken out, and weigh'd by itſelf, the ounce of metal was found to have gain'd no leſs than one dram.

7. An ounce of lead was put upon a cupel, made of calcined hartſhorn, and placed under a muffler, after the cupel was firſt made hot, and then weigh'd. This lead did not enter the cupel, but was turn'd into a kind of litharge on the top of it, and broke the cupel, whereby ſome part of the latter was loſt in the furnace; yet the reſt, together with the litharge, weigh'd ſeven grains more than the lead and heated cupel, when they were put in.

8. We took a cupel about two ounces in weight, made of ten parts of bone-aſhes, and one of charcoal-aſhes, mix'd together with ale. This was, by itſelf, put in a cupelling furnace, under a muffler; and when 'twas throughly and highly heated, we weigh'd it in that ſtate. This done, 'twas immediately again plac'd under the muffler, and kept there for about two hours; at the end of which time, 'twas taken out, red-hot, and preſently put into the ſame balance, as before; where having blown off the adhering aſhes, I found that, tho' when 'twas firſt taken from under the muffler, we had but two ounces and two grains, yet now the weight being put into the oppoſitie ſcale, it gain'd very near 21 grains. And here note, that 'twas not without ſome cauſe, that I weigh'd the cupel red-hot: for I had a ſuſpicion, that notwithſtanding the dryneſs of the bone, it might receive ſome little alteration of weight, by imbibing the moiſt particles wandering in the air; and accordingly, leaving the cupel counterpois'd to cool in the balance, in a ſhort time it began ſenſibly to preponderate; and ſuffering it to continue there for nine or ten hours, I found it about three grains heavier than before.

9. Steel being a metal that, as experience inform'd me, will very eaſily be wrought on by fluids of a ſaline nature, 'twas reaſonable to expect, that flame would have a greater operation on it, eſpecially if it were, before-hand, reduced to ſmall parts, than on any of the bodies hitherto deſcribed. And, accordingly, four drams of the filings of ſteel, being kept two hours on a cupel, under a muffler, acquired one dram, ſix grains and a quarter, increaſe of weight.

10. A piece of refined ſilver, being put upon a cupel under a muffler, and kept there for an hour and a half, was taken out and weigh'd again; and as before it weigh'd three drams, thirty-two grains and a quarter, it now weigh'd in the ſame ſcales, three drams, thirty-four grains and a half.

We repeated the experiment with half an ounce of the filings of ſilver, well refin'd with lead, in our own laboratory, and kept them about three hours upon the cupel; when taking them out, we found them of a leſs pleaſant colour than before, and melted into a lump, which weigh'd four drams ſix grains.

11. We took a dram of the filings of zink, or ſpelter, and having put it upon a cupel, under a muffler, we kept it there, in a cupelling fire, for about three hours; then taking it off, we found it to be caked into a brittle dark-colour'd lump, which look'd as if the filings had been calcined. This being weigh'd in the ſame ſcales, gain'd full ſix grains.

12. Among our various trials, upon common metals, we thought fit to make one or two upon a metal brought us from the *Eaſt-Indies*, and there called *Tutenag*. Two drams of the filings of tutenag, being put upon a cupel, and kept under the muffler for about two hours, the filings were not melted into a lump of metal; but look'd as if powder'd ceruſe and minium had been mix'd together; ſome of the parts appearing diſtinctly white, and others red; the calx being put into the balance, appear'd to have gain'd 28 $\frac{1}{4}$ grains. Another time, the experiment being repeated with the like circumſtances, we found that two drams of filed tutenag, gave almoſt the like increaſe of weight. So that this *Indian* metal, ſeems to have acquir'd more in the fire, in proportion to its weight, than any we have hitherto made trial of.

13. To confirm, by a clear experiment, that in all cupellations, the lead then employ'd, does not, together with the baſer metals, to be purg'd off from the ſilver or gold, fly away in ſmoke; we took two ounces of good lead, and one dram of filings of copper; and having caus'd a cupel to be ignited, and ſuddenly taken out of the furnace, and weigh'd whilſt 'twas very hot, we preſently put it back, together with the two metals laid on it into the furnace: where, having been kept for about two hours, it was taken out again, and found to have nothing on the ſurface of it worth weighing diſtinctly in the ſcales, wherein the cupel, with what was ſunk into it, amounted to four ounces, three drams, and eleven grains; which wanted but nine grains of the whole weight of the cupel, and the two metals, when they were, all three together, committed to the fire. So that tho' we make a liberal allowance for the increaſe of weight, that may, with any probability, be ſuppoſed to have been attain'd by the cupel, and what was put upon it, yet it will eaſily be granted, that much the greater part of the metals, was not driven off in fumes, but enter'd into the ſubſtance of the cupel.

After having ſhewn, that either flame, or the analogous effluvia of the fire, will be incorporated with metals and minerals expoſed naked to its action, I thought proper to try whether this flame, or igneous fluid, were ſubtile enough to exerciſe any ſuch operation upon the like bodies, ſhelter'd from its immediate contact, by being included in cloſe veſſels.

Even tho' the bodies be not immediately expoſed to it.

14. We took an ounce of new filings of ſteel, and having included them betwixt two luted crucibles, kept them for two hours in a ſtrong fire, and ſuffer'd them to continue there till the fire went out; the crucibles being unluted, the filings appear'd hard caked together, had acquired a dark colour, ſomewhat between black and blue, and were increaſed five grains in weight. At another time, an ounce of filings of ſteel, being put between two crucibles, in the ſame manner, after they had been kept about an hour and half in the fire, were taken out, and found to have gain'd ſix grains in weight.

15. We put two ounces of copper-plates into a new crucible, over which a leſſer was whelm'd, and luted the junctures; after the ſame manner, two ounces of tin were included betwixt crucibles; and alſo two ounces of lead: and all theſe being put into the cupelling furnace, were kept in a ſtrong fire, for about an hour and a half; and then being taken out, the copper-plates, tho' they ſtuck together, were not quite melted; and ſeem'd, ſome of them, to have acquir'd ſcales, like copper put into a naked fire; and the two ounces had gain'd eight grains in weight. The lead had broke thro' the bottom of the crucible, and thereby hinder'd the deſign'd obſervation. The tin acquired ſix grains in weight, and was, in part, brought to a pure white calx; but much more of it was melted into a lump, of a fine yellow colour, almoſt like gold, but deeper.

16. We took one ounce of very thin copper-plates, and putting them betwixt two luted crucibles, we kept them in the cupelling furnace for about three hours; and then disjoining the veſſels, we found the metal cover'd with a dark and brittle ſubſtance; which, when ſcaled off, diſcloſed a finely colour'd metal, that, together with theſe burnt ſcales, amounted to twenty-one grains above the weight firſt put in.

17. We took a piece of fine block-tin, in weight half a pound; this we put into a choice glaſs-retort, and kept it for two days in a ſand furnace; which gave heat enough to keep the metal in fuſion, without cracking the glaſs: then taking out the mixture, we carefully weigh'd it in the ſame ſcales, and found the ſuperficies a little alter'd, as if it were diſpoſed to calcination; and the weight to be increaſed above two grains.

And alſo after they have been calcined.

18. We farther try'd how much ſome bodies would increaſe in weight, by the fire, when they had already been expoſed to the vehemence of it. One ounce of calx of tin, made *per ſe*, being put in a new cupel, and kept under the muffler for about two hours, was taken out hot, and put into the ſcales, where the powder appear'd to have gain'd in weight, one dram, thirty-five grains, by the operation of the fire; which made it alſo look much whiter than it did before.

19. Out of a parcel of filings of ſteel, that had been before expoſed to the fire, and had conſiderably increas'd its weight thereby; we took an ounce, and having expoſed them at the ſame time with the calx of tin, and for the ſame time kept them in the fire, we took them out at the two hours end, and found the weight to be increaſed, two drams, twenty-two grains. The filings were very hard caked together, and the lump being broken, look'd almoſt like iron.

20. An ounce and four drams of copper, which had endured a violent fire for an hour and half, being included betwixt two crucibles, and expoſed to a ſtrong fire for two hours, and ſuffer'd to continue there till the fire went out, appear'd, thereby, to have gain'd ten grains in weight, and to have, upon the ſuperficial parts of the plates, ſeveral dark-coloured flakes; ſome of which ſtuck to the metal; but more, upon handling it, fell off.

21. We took eight ounces of block-tin, which, being cut into bits, was put into a good round vial, with a long neck; and then warily held over quick-coals, without touching them, till it was melted; after which, it was kept almoſt continually ſhaking, to promote the calcination, for near an hour; the metal being all the while in fuſion, and the glaſs kept at ſome diſtance from the coals. For the greateſt part of this time, the orifice of the vial was cover'd with a cap of paper, to keep the air, and ſteams of the coals, from getting into the neck: the vial, being afterwards remov'd from the fire, was broken, and the metalline lump had a little darkiſh calx, here and there, upon the upper ſurface; but much more beneath, where it had been contiguous to the bottom of the glaſs. Then putting all this, (carefully freed from little fragments of broken glaſs) into the ſame balance, with the ſame counterpoiſe I had uſed before, I found an increaſe of weight, amounting to eighteen grains, which the tin had acquired by this operation.

22. We then ſeparated the calx; and having melted the metal in a crucible, that, by pouring it out, it might be reduced into thin plates, capable of being cut in pieces, and put into ſuch another vial as the former, we weigh'd it again, together with the reſerved calx, but found, that notwithſtanding all our care, we had loſt three grains of the eighteen gained before. This done, we put the metal into another vial; which having a ſtopple of paper, to keep out ſmoke and air, was held at ſome diſtance from the coals, only whilſt the tin was melting; being afterwards warily laid upon them, and kept there for two hours; at the end of which, it was again taken off, and the metal weigh'd with the ſame counterpoiſe, and balance, as before; and then it amounted to eight ounces, twenty-four grains; and had much more ſeparable calx, than at the firſt time.

Nor did I much wonder, that the weight ſhould be increaſed, in this laſt operation, but nine grains, in two hours; and in the former twice as many, in half the time; ſince, during the two hours the glaſs was kept in one poſture: but, in the firſt operation, it was almoſt perpetually ſhaken, during the time it was kept in fuſion; for it is obſerved, that the agitation of melted minerals, will much promote the effect of the fire upon them, and conduce to their calcination.

23. But to prevent all ſuſpicion of any increaſe of weight, in the metals, ariſing from ſmoke, or ſaline particles, getting in at the mouth of the veſſel, I made the experiment in glaſſes, hermetically ſealed, as follows. Eight ounces of good tin, carefully weigh'd, we hermetically ſeal'd up in

STATICS. a new ſmall retort, with a long neck, by which it was held in the hand near a charcoal fire, that kept the metal in fuſion; being now and then ſhaken for almoſt half an hour; in which time, it ſeem'd to have acquir'd, on the ſurface, ſuch a dark colour, as argued a beginning calcination; and it both emitted fumes that played up and down, and alſo, afforded two or three drops of liquor, in the neck of the retort. The glaſs was, at length, laid on quick-coals, where the metal continued above a quarter of an hour longer in fuſion; but, befote the time was come, that I intended, to ſuffer it to cool, in order to its removal, it ſuddenly broke, into a great multitude of pieces, and with a noiſe, like the report of a gun. In the neck we found ſome drops of a yellowiſh liquor, which had an odious and peculiar taſte; its ſmell alſo, was very fetid, not unlike the diſtilled oil of fiſh.

24. Some tin, which had been before, partly calcined in a glaſs, being melted again in a crucible, we weigh'd out juſt eight ounces, and theſe we put into a bolt-head of white glaſs, with a neck above twenty inches long, which being hermetically ſealed, after the glaſs had been a while kept over the fire leſt it ſhould break by the rarifaction of the air, the metal was kept in fuſion for an hour and a quarter. Being unwilling to venture the glaſs any longer, it was taken from the fire, and when grown cold, the ſealed end was broken off; but before I could have the bottom cut out, I obſerved, that the upper ſurface of the metal was very darkly colour'd, and very irregularly rough; and the lower part had, between the bottom, and the under ſide of the lump, a pretty deal of looſe dark-colour'd calx, tho' the neighbouring ſurface, and ſome places of the lump itſelf, look'd, by candle-light, of a golden colour. The lump, and calx together, were weigh'd in the ſame ſcales carefully, when we found the weight to have increaſed above twenty-three grains; tho' all the calx we could eaſily ſeparate, being weigh'd by itſelf, amounted not to eighty grains.

25. Two ounces of filings of tin, were carefully weigh'd, and put into a little retort, whoſe neck was afterwards drawn ſlender to a very ſmall apex; then the glaſs was placed on kindled coals, which drove out fumes at the ſmall orifice of the neck, for a pretty while. Afterwards, the glaſs, being ſealed up at the apex, was kept in the fire for above two hours; and then being taken off, was broken at the ſame apex: whereupon I heard the external air ruſh in, becauſe, when the retort was ſealed, the air, within it, was highly rarified. Then the body of the glaſs being broken, the tin was taken out, conſiſting of a lump, about which there appear'd ſome grey calx, and ſome very ſmall globules, which ſeem'd to have been filings melted into that form. The whole weigh'd two ounces, and twelve grains. In the neck of the retort, where it was join'd to the body, there appear'd a yellowiſh, and clammy ſubſtance, thinly ſpread, which ſmelt almoſt like the fetid oil of tartar.

26. We took one ounce of the filings of zink, carefully weighed; and having, as carefully, put them into a round bolt-glaſs, we cauſed the neck

to be drawn out very ſlender, and then order'd it to be ſet upon quick coals, where being kept for four hours, there appear'd for great part of the time ſmoke to aſcend from the zink, and get out at the unſtopt apex; and I obſerv'd that the upper part of the glaſs was lined with a darkiſh grey ſublimate. The glaſs being dexterouſly cut aſunder, we took out, not only the filings of zink, ſome of which were melted into little globules, but the *flores* too; yet weighing all theſe in the ſame ſcales we had uſed before, we found above five grains wanting of an ounce.

27. Having carefully weigh'd out four ounces of good lead cut into little pieces, I put them into a ſmall retort with a long neck, wherein was afterwards left only an orifice not much bigger than a pin's-head; this glaſs was kept over and upon the coals for above two hours, and then ſuppoſing the danger of the glaſs's breaking to be over, we ſeal'd it up at the little orifice, and kept it on the coals for two hours longer; before we broke the glaſs I perceiv'd the pieces of lead to have been melted into a lump, whoſe ſurface was dark and rugged, and part of the metal to have been turn'd into a dark-colour'd powder, or calx; all this being taken out of the retort, was weigh'd in the ſame balance, whereon the lead appear'd to have gain'd by the operation ſomewhat above 13 grains.

28. To ſhew that metals are not the only bodies capable of receiving an increaſe of weight from the fire, we ſeal'd up two drams of little bits of good red coral hermetically in a thin bubble of glaſs, and kept them warily at ſeveral times over and upon kindled coals; and at length being taken out, found them of a very dark colour, and to have gain'd in weight about three half grains.

29. And having taken very ſtrong freſh quick-lime, provided on purpoſe for choice experiments, and expoſed it, before the air had time to ſlake it, upon the cupel, to a ſtrong fire, where it was kept for two hours, I found that it had increas'd in weight beyond my expectation; for being ſeaſonably put into the balance, the lumps that weigh'd when expoſed but two drams, amounted to two drams and 29 grains; hence it appears, that notwithſtanding a body may for many hours, or even for ſome days, be expoſed to a very violent fire, yet it may be ſtill capable of admitting and retaining freſh corpuſcles; ſo that tho' well made lime be uſually obſerv'd to be much lighter than the ſtones whereof it is made, yet the lightneſs does not neceſſarily prove, that becauſe a burnt lime-ſtone has loſt much of its matter by the fire, it has therefore acquir'd no matter from the fire; but only infers, that it has loſt far more than it has got; and this may give ground to ſuſpect, that in moſt of the foregoing trials, the acceſs of the fiery particles was greater than the balance diſcover'd; ſince, for ought we know, many of the leſs fix'd particles of the expoſed body might be driven away by the vehemence of the heat; and conſequently the fiery corpuſcles that faſten'd themſelves to the remaining matter might be numerous enough, not only to give the additional weight found by the ſcales, but to make amends for all the fugitive particles that had been expell'd by the fire. And ſince ſo fix'd a body as quick-lime is capable of

being wrought upon by the fiery effluvia, ſo that they come to be as it were incorporated with it, perhaps in other calcin'd or incinerated bodies, the remaining calces or aſhes may retain more than the bare impreſſion of the fire. By this experiment, and thoſe made in ſeal'd retorts, which ſhew, that what is afforded by fire, may in a corporeal way invade, adhere, and add weight even to fix'd and ponderous bodies, there is a large field opened for the ſpeculative to apply the diſcovery to many phenomena of nature and chymiſtry.

SECT. II.

Glaſs pervious to ponderous parts of flame.

TO obviate ſome ſcruples that might be entertain'd upon this circumſtance of our experiments, that the glaſſes employ'd about them were not expoſed to the action of mere flame, but held upon charcoal, I attempted to make them ſucceed with a body ſo thin, and diſengag'd from groſs matter, as mere flame is allow'd to be.

1. Suppoſing then that good common ſulphur, by reaſon of its great inflammability, and the vehemency and penetrancy of its flame, would be a very fit fewel for my purpoſe, I provided a ſmall double veſſel, ſo contriv'd, that the one part ſhould contain as many coals as were neceſſary to keep the ſulphur melted; and the other, which was much ſmaller, and ſhaped like a pan, ſhould contain the brimſtone requiſite for our trial; and that theſe two ſhould be, with a convenient lute, ſo join'd to one another, that all being cloſed at the top, except the orifice of the little pan, the fire and ſmoke of the coals having their vent another way, no fire ſhould come at the retort to be employ'd, but the flame of the burning brimſtone; then two ounces of filings of tin being carefully weigh'd out, and put into a glaſs retort provided for ſuch trials, and made fit to be eaſily ſeal'd up at the neck, the ſulphur, which ought to be of the purer ſort, was kindled, and the glaſs by degrees expoſed to it, where that continu'd near two hours before the metal melted; after which the retort was there kept on near an hour and a half more, with the metal melted in it; then perceiving a pretty deal of darkiſh calx at the bottom, and partly too upon the ſurface of the far greater portion of the metal, which now lay in one lump, the part of the retort which had been ſeal'd being broken off, we firſt took out the calx, and next the lump, and putting them into the ſcales they had been formerly weigh'd in, we found them to have acquired four grains and a half. The experiment was afterwards repeated with the ſame quantity of filings of the ſame metal, and at the end of the operation, which in all laſted ſomewhat above three hours, having broken off the ſeal'd neck of the retort, we found that a large proportion of dark-colour'd calx had been produc'd; and this being weigh'd with the uncalcin'd part of the metal, the two ounces we firſt put in appear'd to have acquired no leſs than eleven and a half.

Such ſuperſtructures may, poſſibly, in time, be built on this and the like experiments, that I ſhall here obviate even a ſcruple, which appears too ſceptical. But, conſidering that tho' it were very improbable, yet poſſible, that the increaſe of weight acquired by bodies expoſed in glaſs veſſels to the fire, might proceed not from the corpuſcles of fire, but from the particles of glaſs itſelf, looſened by the power of ſo intenſe an heat, and forcibly driven into the incloſed body; I took two glaſſes, the one ſhaped into a little retort, and having weigh'd them, kept them for a conſiderable time upon kindled coals, and then weigh'd them again, I could gather little of certainty from the experiment; (the retort at one time ſeeming to have acquired above half a grain in the fire) only that there was no likelihood at all, ſo conſiderable an increaſe of weight, as we ſeveral times obtained in cloſed veſſels, ſhould proceed from the glaſs itſelf, and not from the fire.

2. We carefully weighed out an ounce of the filings of block-tin, and put them into a glaſs retort, whoſe neck was afterwards drawn out ſlender; we alſo provided ſuch a convenient metalline lamp, that the flame of the ſpirit burnt in it, might not melt or crack the glaſs. This lamp, tho' furniſh'd with a cotton wieck, afforded no ſoot; becauſe, as long as it was well ſupplied with liquor, it remain'd unburnt. Theſe things being in readineſs, the retort was warily approached to the flame, and the metal thereby in a ſhort time melted. After which, the glaſs being kept expoſed to the ſame flame, for near two hours, the ſeal'd apex of the retort was broken off; when there appear'd to have been produced a conſiderable quantity of calx, that lay looſe about the remaining part of the tin, which upon its growing cold was harden'd into a lump. This and the calx being taken out of the retort, with care, that no little fragment of glaſs ſhould impoſe upon us, it was weigh'd in the ſame ſcales as formerly, and found to have gain'd 4 ½ grains, beſides the duſt that ſtuck to the inſide of the retort; of which we reckon'd enough to make about half a grain more: ſo that of ſuch a fine and pure flame as of this totally ardent ſpirit, enough to amount to the five grains was arreſted, and, in good meaſure, fixed, by its operation on the tin.

23. For confirmation of the laſt experiment, wherein we had employed the inflammable ſpirit of ſugar, we made the like with highly rectified ſpirit of wine; only ſubſtituting an ounce of lead inſtead of one of tin The event was, that after the metal had been, for above two hours, kept in the flame, the ſeal'd neck of the retort being broken off, the external air ruſh'd in with noiſe; and we found above ſeven ſcruples of lead turned into a greyiſh calx; which, together with the reſt of the metal, being weigh'd again, ſix grains appear'd to be gain'd by the operation. Theſe ſeven ſcruples of calx being weighed in air and water, I found, that tho' the abſolute gravity of the metal had been increaſed by the particles of flame which ſtuck faſt to it; yet this aggregate of lead, and extinguiſhed flame, had loſt much of its ſpecific gravity: for lead is to water of the ſame bulk, as about 11 ⅓ to 1; whilſt this ſubtile calx of lead was to water of the ſame bulk, but as to 9 to 1. Now

Corollaries from this diſcovery.

Now whence can this increaſe of abſolute weight, obſerv'd in the metals expoſed to the mere flame, be deduced, but from ſome ponderous parts of the flame? And how could theſe parts invade thoſe of the metal incloſed in a glaſs, otherwiſe than by paſſing thro' the pores of that glaſs?

But I do not, by theſe experiments, pretend to make out the poroſity of glaſs, any farther than with regard to ſome of the ponderable parts of flame; for, otherwiſe, glaſs is not eaſily penetrable.

That flame may act as a menſtruum, and make coalitions with the bodies it works on.

Upon the whole, it appears, 1ſt, That flame may be a menſtruum, and work on ſome bodies, not only by making a notable comminution, and diſſipation of their parts, but, alſo, by a coalition of its own particles with thoſe of the fretted body; and thereby permanently add ſubſtance and weight thereto. Nor is it repugnant to flame's being a menſtruum, that in our experiment, the lead and tin expoſed to it, were but reduced to powder, and not diſſolved in the form of a liquor, and kept in that ſtate. For beſides that the interpoſed glaſs hinder'd the igneous particles from getting thro' in plenty enough, 'tis not neceſſary that all menſtrua ſhould be ſuch ſolvents. For whether it be, that the menſtrua we think ſimple, may be compounded of very different parts, whereof one may precipitate what is diſſolved by the other, or for ſome other cauſe; it is certain, that ſome menſtrua corrode metals and other bodies, without keeping any conſiderable part diſſolved; as may be ſeen if you put tin to a certain quantity of *Aqua fortis*, which will, in a very ſhort time, reduce it, almoſt totally, to a very white ſubſtance, that, when dry, is a kind of calx. And ſo, with a due proportion of oil of vitriol, abſtracted from quick-ſilver, by a ſtrong fire, we have ſeveral times reduced the main body of the mercury into a white powder, whereof but an inconſiderable part was diſſoluble in water. And ſuch a white calx I have had, by the action of another fretting liquor on a body not metalline. But whether, as it ſeems probable, it be from the vehement agitation of the permeating particles of flame, that violently tear aſunder the metalline corpuſcles; or, from the nature of the igneous menſtruum, which being, as 'twere, ſtrain'd thro' glaſs itſelf, muſt be ſtrangely minute; 'tis worth obſerving how ſmall a proportion, in point of weight, of the additional adhering body, may ſerve to corrode a metal, in compariſon of the quantity of vulgar menſtrua requiſite for that purpoſe. For, tho' we are oblig'd to employ, to make the ſolution of crude lead, ſeveral times its weight of ſpirit of vinegar, or a large proportion of *Aqua fortis*; 'twas obſerv'd in our experiment, that tho' the lead was increaſed but ſix grains in weight, yet above a hundred and twenty of it were fretted into powder; ſo that the corroſive body appear'd to be but about the twentieth part of the corroded.

That calces of metals are the magiſteries of them.

2dly, Another conſequence deducible from our diſcovery of the perviouſneſs of glaſs to flame, may be this, that there is cauſe to queſtion the truth of what is generally taken for granted about calcination; and particularly of the notion that chymiſts have entertain'd about the calces of metals and minerals. For it does not appear, by our trials, that any proportion, worth regarding, of moiſt and fugitive parts, was expell'd in the calcination; but it appears very plainly, that, by this operation, the metals gain'd more weight than they loſt: ſo that the main body of the metal remain'd entire, and was far from being either elementary earth, or a compound of earth and fix'd ſalt. From which very erroneous hypotheſis, the chymiſts infer the ſweet vitriol of lead, which they call *Saccharum Saturni*, to be but the ſweet ſalt of it, extracted only by the ſpirit of vinegar, which does indeed plentifully concur to compoſe it. Whence, I conclude, that the calx of a metal, even made *per ſe*, that is, by fire alone, may be, at leaſt in ſome caſes, not the *Caput mortuum*, or *Terra damnata*, but a magiſtery of it. For, in the ſenſe of the moſt intelligible of the chymical writers, that is properly a magiſtery wherein the principles are not ſeparated; but the bulk of the body being preſerved, it acquires a new and convenient form, by the addition of the menſtruum, or ſolvent, employ'd about the preparation. Beſides, I have ſeveral times from the calx of lead reduced actual lead. And having once taken but about a third or fourth part of the calx of lead, produced by the flame of ſpirit of wine, I found, that without any flux-powder, or other addition, but merely by the application of the flame of highly rectify'd ſpirit of wine, there would, in a ſhort time, be obtain'd a conſiderable proportion of malleable lead; whereof the part I examin'd was truly ſo.

The effects of fire upon bodies, in cloſe glaſſes, not wholly owing to the agitation of the glaſs.

3dly, One uſe, among the reſt, we may make of the foregoing diſcovery, regards a controverſy among the corpuſcular philoſophers. For ſome of them think, that when bodies are expoſed in cloſe veſſels to the fire, tho' the igneous corpuſcles do not ſtay with the bodies they invade; yet they really get thro' the pores of the interpoſed veſſels, and permeate the included bodies in their paſſage upwards; whilſt others will not allow the igneous corpuſcles, which they take to be but vehemently agitated particles of terreſtrial matter, to penetrate ſuch minute pores as thoſe of glaſs; but ſuppoſe the operation of the fire to be perform'd by the vehement agitation made of the ſmall parts of the glaſs, and by them propagated to the included bodies; whoſe particles, by this violent commotion, are greatly alter'd.

But tho' it be probable, that the briſk agitation communicated by the ſmall parts of the glaſs to thoſe of the body contain'd in it, may contribute much to the effect of the fire; and tho', by the ſmall increaſe of weight we found in our expoſed metals, 'tis very likely that far the greater part of the flame was excluded by the cloſe texture of the glaſs; yet, on the other ſide, 'tis plain, that igneous particles paſs'd thro' the glaſs, and not only divided and agitated the included bodies, but manifeſtly and permanently adhered to them.

Particles extremely volatile may by aſſociating with others, quite loſe their volatility.

4thly, We receive this further information from our experiments, that bodies very ſpirituous, fugitive, and minute, may, by being aſſociated with proper particles, tho' of quite another nature, ſo change their former qualities, as to be arreſted by a ſolid and ponderous body, to that degree, as not to be driven away from it by a fire intenſe enough to melt and calcine metals. For the foregoing trials ſeem plainly to diſcover, that even the agitated parts of flame, minute enough to paſs through the pores of glaſs itſelf, were ſome way entangled among the metalline particles of tin and lead; and thereby brought to be ſo fix'd, as to endure the heat that kept thoſe metals in fuſion, and gradually reduced them to calces: a phenomenon that one would not eaſily look for; eſpecially conſidering how ſimple a texture that of lead or tin may be, in compariſon of the more elaborate ſtructures of many other bodies. And this phenomenon, which ſhews us what light and fugitive particles of matter may permanently concur to the compoſition of ponderous and fix'd bodies, will, perhaps, afford uſeful hints to the ſpeculative; eſpecially, if this ſtrict combination of a ſpirituous and fugitive ſubſtance with ſuch as being groſs and unweildy, are leſs fit than organized matter to entangle or detain them, be apply'd, as it may be, with advantage, to thoſe aggregates of ſpirituous corpuſcles, and organical parts, that make up the bodies of plants and animals. And this hint may ſuggeſt a conſiderable inference to be drawn from the operation of the ſun-beams on appropriated ſubjects; ſuppoſing it to prove like that of flame on tin and lead.*

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* The following queries of Sir *Iſaac Newton*, give us the beſt light we have into the nature of fire and flame. " Is not," ſays that great Philoſopher, " fire a body, heated ſo hot, as to emit light copiouſly? For what elſe is a red-hot iron than fire? And what elſe is a burning coal than red hot wood? Is not flame a vapour, fume, or exhalation, heated red hot; that is, ſo hot as to ſhine? For bodies do not flame without emitting a copious fume; and this fume burns in the flame. The *Ignis fatuus* is a vapour, ſhining without heat: and is there not the ſame difference between this vapour and flame, as between rotten wood, ſhining without heat, and burning coals of fire? In diſtilling hot ſpirits, if the head of the ſtill be taken off, the vapour which aſcends out of the ſtill will take fire at the flame of a candle and turn into flame; and the flame will run along the vapour, ftom the candle to the ſtill. Some bodies, heated by motion or fermentation, if the heat grow intenſe, fume copiouſly; and if the heat be great enough, the fumes will ſhine, and become flame. Metals, in fuſion, do not flame for want of a copious fume. Salt petre fumes copiouſly, and thereby flames. All flaming bodies, as oil, tallow, wax, wood, foſſil, coals, pitch, and ſulphur, by flaming waſte, and vaniſh into burning ſmoke; which ſmoke, if the flame be put out, is very thick, and viſible; and ſometimes ſmells ſtrongly: but, in the flame, it loſes its ſmell, by burning: and, according to the nature of the ſmoke, the flame is of ſeveral colours; as that of ſulphur, blue; that of copper, open'd with ſublimate, green; that of tallow, yellow; that of camphire, white. Smoke paſſing thro' flame, cannot but grow red-hot; and, red-hot ſmoke, can have no other appearance than that of flame. When gun-powder takes fire, it goes away into flaming ſmoke. For the charcoal and ſulphur, eaſily take fire, and ſet fire to the nitre; and the ſpirit of the nitre being thereby rarify'd into vapour, ruſhes out, with exploſion, much after the manner that the vapour of water ruſhes out of an æolipile; the ſulphur, alſo, being volatile, is converted into vapour, and augments the exploſion. And the acid vapour of the ſulphur, namely, that which diſtils under a glaſs-bell, into oil of ſulphur, entering violently into the fixed body of the nitre, ſets looſe the ſpirit of the nitre, and excites a great fermentation, whereby the heat is farther augmented, and the fixed body of the nitre is alſo rarified into fume; and the exploſion is, thereby, made more vehement, and quick. For, if ſalt of tartar be mixed with gun-powder, and the mixture be warm'd till it takes

fire, the exploſion will be more violent, and quick, than that of gun-powder alone; which cannot proceed from any other cauſe, than the action of the vapour of the gun-powder upon the ſalt of tartar, whereby the ſalt is rarified. The exploſion of gun-powder ariſes, therefore, from the violent action, whereby all the mixture, being quickly and vehemently heated, is rarified, and converted into fume and vapour; which vapour, by the violence of that action, becoming ſo hot as to ſhine, appears in the form of flame."

The following query is ſo beautiful, and opens ſuch a glorious ſcene, that I ſhould be tempted to add it, tho' it bore a leſs relation than it does, to the preſent ſubject.

"Do not" continues the ſame great author, "large bodies conſerve their heat the longeſt; their parts heating one another? And may not a great, denſe, and fixed body, when heated beyond a certain degree, emit light ſo copiouſly, as, by the emiſſion, and re-action of its light, and the reflexions, and refractions of its rays, within its pores, to grow ſtill hotter, till it comes to a certain period of heat; ſuch as is that of the ſun? And are not the ſun, and fixed ſtars, great earths, vehemently hot; whoſe heat is conſerved by the greatneſs of the bodies, and the mutual action, and re-action between them, and the light which they emit; and whoſe parts are kept from fuming away, not only by their fixity, but, alſo, by the vaſt weight and denſity of the atmoſpheres, incumbent upon them; and very ſtrongly compreſſing them, and condenſing the vapours and exhalations which ariſe from them? For if water be made warm, in any pellucid veſſel emptied of air, that water, in the vacuum, will bubble and boil, as vehemently as it would in the open air, in a veſſel ſet upon the fire, till it conceives a much greater heat. For the weight of the incumbent atmoſphere, keeps down the vapours, and hinders the water from boiling, till it grow much hotter than is requiſite to make it boil *in vacuo*. Alſo, a mixture of tin and lead, being put upon a red-hot iron, *in vacuo*, emits a fume and flame; but the ſame mixture, in the open air, by reaſon of the incumbent atmoſphere,does not ſo much as emit any fume, which can be perceived by ſight. In like manner, the great weight of the atmoſphere, which lies upon the globe of the ſun, may hinder bodies there from riſing up, and going away from the ſun, in the form of vapours, and fumes, unleſs by means of a far greater heat than that which, on the ſurface of our earth, would very eaſily turn them into vapours and fumes. And the ſame great weight may condenſe thoſe vapours, and exhalations, as ſoon as they ſhall, at any time, begin to aſcend from the ſun, and make them preſently fall back again into him; and, by that action, increaſe his heat, much after the ſame manner that, on our earth, the air increaſes the heat of a culinary fire. And the ſame weight may hinder the globe of the ſun from being diminiſh'd; unleſs by the emiſſion of light, and a very ſmall quantity of vapours and exhalations. *Newton. Optic.* p. 316,—319.

PNEUMATICS.

THE

PREFACE.

PNeumatics is that part of univerſal philoſophy, which conſiders the properties, the uſe, and effects of the air; the fluid we breathe, and which is abſolutely neceſſary to maintain the life of animals.

This is a ſubject, therefore, that cannot but be highly uſeful to inquire into: and herein Mr. Boyle *appears to have exerciſed himſelf more than any philoſopher before his time or ſince. Indeed, the antients had no notion of this part of philoſophy; and ſeem to have never dream'd of an engine wherewith to draw the air from veſſels, and prevent its return into them.* Otto Gueric, *the famous conſul of* Magdeburg, *firſt, unleſs we except* Friar Bacon, *hit upon an expedient to do it; and from a hint of this it was, that Mr.* Boyle, *improving on the deſign of the conſul, originally contrived his air-pump: a machine, to which we are indebted for the ſolution of many phenomena in nature; and momentous diſcoveries in the fluid, which of all external ones, ſeems to be the moſt familiar and uſeful to us. A man of an ordinary capacity, poſſeſs'd of ſuch an engine as this, would have been perplexed to know what uſe to make of it; but ſuch a genius as Mr.* Boyle, *could not but apply it to noble and ſerviceable purpoſes. In veſſels of glaſs, emptied of air by its means, he ſoon included a great variety of proper ſubjects, one after another; of which he had before-hand wrote down a long catalogue. And as this engine was at firſt imperfect, but afterwards received conſiderable improvements, at different times, I am induced to let all the experiments made with it by Mr.* Boyle, *ſtand in the order wherein he wrote them, rather than to range them under ſeveral heads. For thus they give the hiſtory of the air-pump and its improvements from time to time: but if the other method had been taken, the advantage of theſe experiments muſt have been loſt in their hiſtorical capacity; which would prove a greater inconvenience than the ſeeming diſorder wherein they now occur: eſpecially conſidering, that this may eaſily be remedied, by means of the index.*

Numerous are the experiments made by Mr. Boyle, *with this engine; but ſtill there may be ſomething left for future philoſophers to do with it, beſides repeating, varying, and confirming his trials. The air-pump, as 'tis now made, has many advantages over even the beſt which was employ'd by Mr.* Boyle: *it is ſo much more manageable, commodious, and exact, (eſpecially as improved by that excellent mechanic, Mr.* Haukſbee) *its* apparatus *is ſo well ſuited for all exigencies of experiments; and the whole comes on ſuch eaſy terms, in compariſon of what it* *coſt our generous philoſopher, that if it were only to be witneſs of the ſurprizing diſcoveries he made with it, 'tis pity any well-wiſher to philoſophy ſhould be without it.*

But were this inſtrument in more general uſe; and would men of invention ſet themſelves to contrive new experiments for it, who knows what farther light it might afford us; and in producing how many other effects of nature, the air would appear to be concern'd? The late Mr. Haukſbee *made ſeveral experiments with it, which, perhaps, Mr.* Boyle *himſelf never thought of; tho' he evidently contrived a large number; which, for want of a fit opportunity, convenient glaſſes, and other* apparatus, *he could never ſee the event of. The ſubject, therefore, appears far from being exhauſted: the air-pump is a fruitful engine, and ſeems fitted to examine almoſt all the productions of nature: and there are many things in philoſophy that cannot be done without it. 'Twas this which lately open'd a new field of Knowledge, and greatly aſſiſted Mr.* Boyle *to introduce or revive the mechanical philoſophy; this evidently help'd to overthrow the doctrine of nature's dread of a* Vacuum, *by which the modiſh philoſophy of his time, falſely accounted for abundance of phenomena; to this is owing the greateſt part of thoſe noble experiments, which appear under the following head of* Pneumatics; *and laſtly, to this we owe ſome ſurprizing diſcoveries of Sir* Iſaac Newton, *and a great ſhare of the preſent ſyſtem of philoſophy.*

Phyſico-mechanical EXPERIMENTS,

To ſhew the *Spring and Effects of the AIR.*

SECT. I.

The origin of the air-pump.

THE air is ſo neceſſary to life, that moſt creatures which breathe cannot ſubſiſt for many minutes without it; and moſt of the natural bodies we deal with, being, as well as our own, almoſt perpetually contiguous to it, the alterations thereof have a manifeſt ſhare in many obvious effects, and particularly in diſtempers; wherefore a farther inquiry into the nature of this fluid, will probably ſhew, that it concurs to exhibit abundance of phenomena, wherein it has hitherto ſeem'd little concern'd. So that a true account of any new experiment, upon a thing whereof we have ſuch a conſtant and neceſſary uſe, may prove advantageous to human life.

With this view, before ever I was inform'd that *Otto Gueric*, the ingenious conſul of *Magdeburg*, had practiſed a way in *Germany* of emptying glaſs veſſels of the air, I had made experiments on the ſame foundation; but as that gentleman firſt produced conſiderable effects by this means, I acknowledge the aſſiſtance and encouragement which the report of his performances afforded me.

But as few inventions happen to be compleat at the firſt, ſo the engine employ'd by the conſul, ſeem'd very defective in its contrivance; whence but little more could be expected from it, than thoſe very few phenomena obſerved by the author, and related by *Schottus*. I therefore put Mr. *Hook* upon contriving an air-pump, more manageable and convenient, that might not, like the *German*-engine, require to be kept under water; and after ſome unſucceſsful attempts, he fitted me with one, conſiſting of two principal parts, a glaſs veſſel, and a pump to evacuate the air.

The

PNEUMATICS

Fig. 30.

The air-pump deſcribed.

The firſt is a glaſs A, with a large mouth, a cover thereto, and a ſtop-cock fitted to the neck below. This would contain 30 quarts of water. B C, the mouth of it, is about four inches in diameter, and ſurrounded with a glaſs lip, almoſt an inch high, for the cover to reſt on; wherein D E, is a braſs ring, to cover, and be cemented on to the lip B C. To the internal orifice of this ring, a glaſs ſtopple is fitted, to keep out the external air. In the middle of this cover is a hole H I, half an inch in diameter, incircled with a ring, or ſocket; to which is adapted a braſs ſtopple K, to be turned round, without admiting the leaſt air. In the lower-end of this, is a hole 8, to admit a ſtring, 8, 9, 10; which alſo paſſes thro' a ſmall braſs ring L, fixed to the bottom of the ſtopple F G, to move what is contained in the exhauſted veſſel, or receiver. That the ſtop-cock N, in the firſt figure, might perfectly exclude the air, we faſtened a thin tin-plate, M T V W, to the ſhank of the cock X, all along the neck of the receiver, with a cement made of pitch, roſin, and wood-aſhes, poured hot into the cavity of the plate; and to prevent the cement from running in at the orifice Z, of the ſhank X, it was ſtopt with a cork fixed to a ſtring, that it might be drawn out at the upper orifice of the receiver; and then the neck of the glaſs, being made warm, was preſſed into the cement, which thus filled the interſtices betwixt the tin-plate and the receiver, and betwixt the receiver and the ſhank of the cock.

The lower part of our engine conſiſts of a ſucking-pump, ſupported by a wooden frame, with three legs 111, ſo contrived, that, for the freer motion of the hand, one ſide of it may ſtand perpendicular; and a-croſs the middle of the frame we nail'd a piece of board 222, to which the principal part of the pump is fixed. The pump conſiſts of an exact ſtrong concave cylinder of braſs, fourteen inches long, its cavity three inches in diameter; to which a ſucker, 4455, is adapted, made up of two parts; one of which 44, is leſs in diameter than the cavity of the cylinder, with a thick piece of tann'd leather nailed on it, whereby it excludes the air. The other part, a thick iron plate 55, is firmly joined to the middle of the former, and is a little longer than the cylinder; one edge of it being ſmooth, and the other indented, to receive the teeth of a ſmall iron-nut $\alpha \beta \gamma$, fixed by two ſtaples to the underſide of the board nailed a-croſs 22, on which the cylinder ſtands; and is turned by the handle 7.

The laſt part of the pump is the valve R, a hole at the top of the cylinder, and taper towards the cavity; to this is fitted a braſs-plug, to be taken out as occaſion requires. The engine being thus contrived, ſome oil muſt be pour'd in at the top of the receiver upon the ſtop-cock, to fill up the interſtices of its parts, and that the key S, may turn with the greater eaſe. A quantity of oil, alſo, muſt be left in the cylinder, to prevent the air from getting betwixt that and the ſucker; for the like reaſons, ſome muſt, likewiſe, be applied to the valve.

And here it is proper to obſerve, that when we uſed oil, or water, ſeparately, for this purpoſe, and they have not anſwered the end, a mixture of

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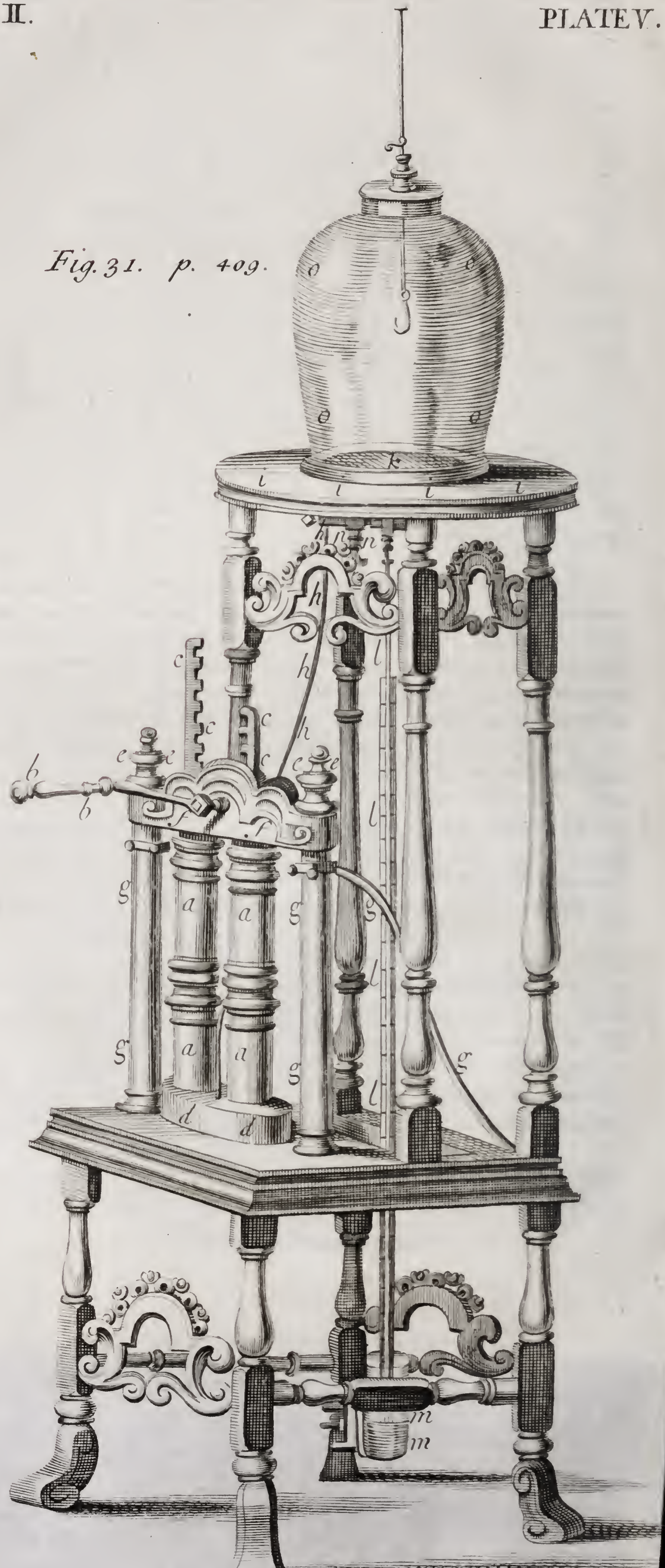
Fig. 31. p. 409.



of the two has afterwards proved effectual. And, that the air may not enter betwixt the braſs-cover and the ring, 'twill be convenient to lay ſome diachylon-plaiſter on their edges with a hot iron. That no air, alſo, may remain in the upper part of the cylinder, the handle is to be turn'd till the ſucker riſes to the top; and then, the valve being ſhut, it is to be drawn down to the bottom; by which means, the air being driven out of the cylinder, and a ſucceſſion from without prevented, the cavity of the cylinder muſt be empty of air; ſo that when the ſtop-cock is turn'd to afford a communication betwixt the receiver and the cylinder, part of the air before lodg'd in the receiver, will be drawn down into the cylinder; which by turning back the key, is kept from entering the receiver again, and may, by unſtopping the valve, and forcing up the ſucker, be driven into the open air; and ſo, by repeated exſuctions out of the receiver, and expulſions out of the cylinder, the veſſel may be exhauſted as the experiment requires*.

1. Upon

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* The air-pump has receiv'd great improvements ſince the time of Mr. *Boyle*, and ſeems brought to its utmoſt degree of ſimplicity and perfection, by the late, and the preſent Mr. *Hauksbee*. This inſtrument, as 'tis *Fig.* 31. now made, by Mr. *Hauksbee*, conſiſts of two braſs-cylinders, *a a a a*, twelve inches high, and two their internal diameter. The emboli are raiſed, and depreſſed, by turning the winch *b b*, backward and forward. This winch is faſten'd to a ſpindle, paſſing through a lanthorn, whoſe pins ſerve for cogs, laying hold of the teeth of the racks *c c c c*; ſo that one is depreſſed, and the other elevated reciprocally. By this means the valves, made of limber bladder, and fix'd on the upper part of each embolus, and at the bottom of the cylinders, mutually exhauſt and diſcharge the ſame air from the receiver: which becoming nearly empty, the preſſure of the external air on the deſcending embolus is ſo great, that the power required to raiſe the other, need but little ſurmount the friction of the moving parts; whence this pump becomes preferable to all others. The bottoms of the barrels lie in a braſs diſh *d d*, its ſides two inches high, containing water to keep the leather-collars, on which the cylinders ſtand, moiſt; whereby the air is precluded. The cylinders are ſcrew'd hereon by the nuts *e e e e*, which force the frontiſpiece *f f*, down upon them; thro' which paſs the two pillars *g g g g*. Each pillar has an iron belonging to it, paſſing from them in the form of a ſwan's neck *g g*; theſe irons being faſtened to the hind part of the frame, to prevent their ſhaking. Between the two barrels, riſes a hollow braſs wire *h h h h*, communicating with each of them, by means of a perforated piece of braſs, lying horizontally from one to the other. The upper end of this wire is faſten'd to another piece of perforated braſs, ſcrew'd on below the plate *i i i i*, which is ten inches over; having a braſs-rim ſolder'd on it, that it may contain water. Between the middle and the ſide of this plate, riſes a ſmall pipe *k*, about an inch and half high, thro' which, into the hollow wire, paſſes all the air into the barrels from the receiver. Upon the plate of the pump is always laid a wet leather, for the receivers to ſtand on. This leather prevents the air's getting into the glaſſes, whoſe edges are ground true; and ſerves for this purpoſe vaſtly beyond any cement whatever. Another excellence in this pump, is the gage *l l l l*, a glaſs tube about thirty-four inches long, ſo placed, that it cannot eaſily be damaged, or prove inconvenient. Its lower orifice is immerſed in a glaſs of quick-ſilver *m m*; on the ſurface whereof is a perforated piece of cork for the tube to paſs thro'. On this cork is placed a board of box-wood, about an inch in breadth, and grooved in the middle to receive the tube, which is looped on thereto, that it may riſe and fall as

the

Some phenomena of the engine ſolved.

1. Upon drawing down the ſucker of our engine, whilſt the valve is ſhut, the cylindrical ſpace deſerted by it will be left empty of air; and, therefore, upon turning the key, the air contain'd in the receiver ruſhes into the cylinder, till, in both veſſels, it be brought to an equal dilatation; ſo that, upon ſhutting the receiver, turning back the key, opening the valve, and forcing up the ſucker again, almoſt a whole cylinder of air will be driven out after this firſt exſuction; but, after every ſucceeding ſtroke, leſs air will come out of the receiver into the cylinder: ſo that, at length, the ſucker will riſe almoſt to the top of the cylinder, before the valve need be open'd. And if, when it is ſo exhauſted, the handle of the pump be let go, and the valve be ſtopp'd, the ſucker, by the force of the external air, which is an over-balance to the internal rarify'd air, will be forced to the upper part of the cylinder, and higher, in proportion, as the air is more exhauſted *. We obſerv'd, alſo, that, whilſt any conſiderable quantity of air remains in the receiver, a briſk noiſe is immediately produced, upon turning the key.

The ſpring and preſſure of the air explain'd.

But to render our experiments the more intelligible, we muſt premiſe, that the air abounds in elaſtic particles, which being preſſed together by their own weight, conſtantly endeavour to expand and free themſelves from that force; as wool, for example, reſiſts the hand that ſqueezes it, and contracts its dimenſions; but recovers them as the hand opens, and endeavours at it, even whilſt that is ſhut. It may be alledg'd, that tho' the air conſiſts of elaſtic particles, yet this only accounts for the dilatation of it in pneumatical engines, wherein it hath been compreſſed, and its ſpring violently bent, by an external force; upon the removal whereof, it expands, barely to recover its natural dimenſions; whilſt, in our experiments, the air appears not to have been compreſſed, before its ſpontaneous dilatation. But, we have many experiments to prove, that our atmoſphere is a heavy body, and that the upper parts of it preſs upon the lower. And I found a dry lamb's bladder, containing two thirds of a pint, and compreſs'd by a pack-thread tied about it, to loſe, in a very tender balance, $1\frac{1}{8}$ grain of

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the mercury aſcends or deſcends in the gage. To the upper part of this tube is cemented a braſs-head, that fits into the perforated braſs-piece, ſcrew'd on under the plate, and communicating both with the receiver, and the hollow braſs-wire *h h h h*. The box board is graduated into inches and quarters, from the ſurface of the quick-ſilver to twenty-eight inches high; and thence 'tis divided into tenths. By this means, the degrees of rarifaction may, at all times, be nicely obſerved in an experiment. The air-cock *n*, which lets in the air, is, likewiſe, a ſcrew on the ſame perforated braſs, in which the upper parts of the gage, and the hollow wire, are inſerted. *o o o o* repreſents a receiver, ſtanding on the plate of the pump; on whoſe upper part *p p*, thro' a box of leather-collars, paſſes a ſlip of wire, to take up, let fall, or ſuſpend any thing in the receiver, without admitting the air.

* The original air in the receiver, is always to the remainder, as the ſum of the capacity of the veſſel of the pump, raiſed to the power, whoſe exponent is equal to the number of the ſtrokes of the ſucker, to the capacity of the veſſel rais'd to the ſame power. See this demonſtrated by M. *Varignon. Memoir. de l'Academ.* A. 1705. p. 397.

its former weight, by the receſs of the air, upon pricking it. Suppoſing, therefore, that the air is not deſtitute of weight, 'tis eaſy to conceive, that the part of the atmoſphere wherein we live, is greatly compreſs'd by thoſe directly over it, to the top of the atmoſphere. And tho' the height of this atmoſphere, according to *Kepler*, ſcarce exceeds eight miles, yet later aſtronomers extend it ſix or ſeven miles farther. The learned *Ricciolo* makes it reach fifty miles high. So that a column of air, ſeveral miles in height, preſſing upon ſome elaſtic particles of the ſame fluid here below, may eaſily bend their little ſprings, and keep them bent; as if fleeces of wool were piled to a vaſt height upon one another, the hairs of the loweſt locks would, by the weight of all the incumbent parts, be ſtrongly compreſs'd. Hence it is, that upon taking off the preſſure of the incumbent air, from any parcel of the lower atmoſphere, the particles of the latter poſſeſs more ſpace than before. If it be farther objected againſt this condenſation of the inferior air, that we find this fluid readily yields to the motion of flies, feathers, *&c.* we may reply, that as when a man ſqueezes wool in his hand, he feels it make a continual reſiſtance; ſo each parcel of the air, about the earth, conſtantly endeavours to thruſt away ſuch contiguous bodies as keep it bent, and hinder the expanſion of its parts: which will fly out towards that part, where they find the leaſt reſiſtance. And, ſince the corpuſcles whereof the air conſiſts, tho' of a ſpringy nature, are ſo very ſmall, as to compoſe a fluid body, 'tis eaſy to conceive, that here, as in other fluids, the component parts are in perpetual motion, whereby they become apt to yield to, or be diſplaced by other bodies; and that the ſame corpuſcles are ſo variouſly mov'd, that, if ſome attempt to force a body one way, others, whoſe motion had an oppoſite determination, as ſtrongly preſs it the contrary way; whence it moves not out of its place; the preſſure, on all ſides, being equal. For if, by the help of our engine, the air be drawn only from one ſide of a body, he, who thinks to move that body, as eaſily as before, will, upon trial, find himſelf miſtaken.

2. Thus, when our receiver is tolerably exhauſted, the braſs ſtopple in the cover is ſo difficult to lift, that there ſeems to be ſome great weight faſten'd to the bottom of it: for the internal air being, now, very much dilated, its ſpring muſt be greatly weakned; and, conſequently, it can but faintly preſs againſt the lower-end of the ſtopple, whilſt the ſpring of the external air keeps it down, with its full natural force. And, as the air is gradually admitted into the receiver, the weight is manifeſtly felt to decreaſe; till, at length, the receiver being again fill'd with air, the ſtopple may be eaſily lifted.

It may ſeem ſurprizing, that we ſpeak of the air ſhut up in our receiver, as of the preſſure of the atmoſphere; though the glaſs manifeſtly keeps the incumbent pillar of air from preſſing upon that within the veſſel. But, let us conſider, that if a fleece of wool, by preſſure, be thus directly reduced into a narrow compaſs, and convey'd into a cloſe box, tho' the former force ceaſes to bend its numerous ſpringy parts, yet they continue

as

as ſtrongly bent as before; becauſe we ſuppoſe the including box reſiſts their expanſion, as much as the force that crowded them in. Thus the air, being ſhut up in our glaſs when its parts are bent by the whole weight of the incumbent atmoſphere, tho' that weight can no longer preſs upon it; yet the corpuſcles of the internal air continue as forcibly bent, as before they were included. If it be ſaid, that the continual endeavour it has to expand itſelf, ought then to break the glaſs, we muſt obſerve, that the expanſive force of the internal air, is balanc'd by preſſure of the external, which preſerves the glaſs intire; as, by the ſame means, thin large bubbles made with ſoapy water, will, for ſome time, continue whole in the open air.

3. And though, by help of the handle, which is a lever, the ſucker may eaſily be drawn down to the bottom of the cylinder; yet, without ſuch a mechanic power, the ſame effect could not be produced, but by a force able to ſurmount the preſſure of the atmoſphere: as in the *Torricellian* experiment, if the column of mercury be too high, it will ſubſide, till its weight be a balance to the preſſure of the air. Hence we need not wonder, that tho' the ſucker move eaſily in the cylinder, by means of the handle, yet if that be taken off, it will require a conſiderable force to raiſe or drpreſs it. Nor will it ſeem ſtrange, that if, when the valve, and ſtop-cock are exactly cloſed, the ſucker be drawn down, and then the handle let looſe, that the ſucker, as of itſelf, re-aſcends to the top of the cylinder; ſince the ſpring of the external air, finds nothing to reſiſt its preſſure upon the bottom of the ſucker. And, for the ſame reaſon, when the receiver is almoſt emptied, tho', the ſucker being drawn down, the paſſage from the receiver to the cylinder be open'd, and then ſtopp'd again, the ſucker will, upon the letting go the handle, be forcibly carried up, almoſt to the top of the cylinder; becauſe the air within the cylinder, being equally dilated and weakned with that of the glaſs, is unable to reſiſt the preſſure of the external air, till it be crowded into ſo little ſpace, that both their forces are in equilibrium. So that, in this caſe, the ſucker is drawn down with little leſs difficulty, than if, the cylinder being deſtitute of air, the ſtop-cock were exactly ſhut. It muſt alſo be obſerved, that when the ſucker hath been impell'd to the top of the cylinder, and the valve is ſo carefully ſtopp'd, that no air remains in the cylinder, above the ſucker; if, then, the ſucker be drawn to the lower part of the cylinder, no greater difficulty is found to depreſs the ſucker, when nearer the bottom of the cylinder, than when it is much farther from it. Whence it appears that the preſſure of the external air, is not increas'd upon the acceſſion of the air driven out; which, to make itſelf room, forceth the contiguous air to a violent ſub-ingreſſion of its parts, as ſome ſuppoſe; for otherwiſe the ſucker would be more reſiſted by the external air as it comes lower; more of the diſplaced air being thruſt into it, to compreſs it.

Bladders dilated by the ſpring of the air.

4. We took a large lamb's bladder, well dry'd, and very limber, and leaving in it about half the air it would contain, we ſtrongly tied the neck

of it; then conveying it into the receiver, the pump was worked; and after two or three ſtrokes, the impriſoned air began to ſwell in the bladder, and continued to do ſo, as the receiver was farther exhauſted, till at length, the bladder appeared perfectly turgid. Then, by degrees, allowing the external air to return into the receiver, the diſtended bladder ſhrunk proportionably, grew flaccid, and, at laſt, appeared as full of wrinkles as before.

And to try whether the actual elaſticity of the fibres of the bladder had any ſhare in this effect, we let down to the former, two ſmaller bladders, of the ſame kind; the one not tied up at the neck, that the air it contained might paſs into the receiver; the other, with its ſides ſtretched out, and preſſed together, that it might hold the leſs air, and then ſtrongly tied up at the neck; and, whilſt the firſt, upon working the pump, appeared every way diſtended to its full dimenſions, neither of the others were remarkably ſwelled; and that whoſe neck was left looſe, ſeemed very little leſs wrinkled than when firſt put in.

We made, likewiſe, a ſtrong ligature about the middle of a long bladder, emptied of its air in part, but left open at the neck; and, upon exhauſting the receiver, obſerved no ſuch ſwelling betwixt the ligature, and the neck, as betwixt the ligature and the bottom of the bladder, where air was included.

And burſt by the ſame.

5. We hung a dry bladder, well tied and blown moderately full, in the receiver, by a ſtring faſten'd to the inſide of the cover; and, upon exhauſting the glaſs, the included air firſt diſtended the bladder, and then burſt it, as if it had been forcibly torn aſunder.

This experiment was repeated with the like ſucceſs; and the bladder burſting, long before the receiver was fully exhauſted, gave a great report.

But it was often, in vain, that we tried to burſt bladders, after this manner, becauſe they were commonly grown dry, before they came to our hands; whence, if we tied them very hard, they were apt to fret, and ſo become unſerviceable; and, if tied but moderately hard, their ſtiffneſs kept them from being cloſed ſo exactly, that the air ſhould not get out into the receiver. We found, alſo, that a bladder moderately filled with air, and ſtrongly tied, being held for a while, near the fire, grew exceeding turgid; and, afterwards, being brought nearer to the fire, ſuddenly burſt, with ſo loud and vehement a noiſe, as made us almoſt deaf for ſome time after.*

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* M. *Amontons* ſhews, that the ſame degree of heat, how ſmall ſoever, may perpetually increaſe the force of the air's ſpring, provided that air be continually preſſed by a weight ſtill greater and greater; and that any parcel of air, how ſmall ſoever, may perpetually increaſe the force of its ſpring, by a ſmall degree of heat; provided this air be more and more preſſed continually. The ſame gentleman, alſo, found by experience, that the heat of boiling water, which he ſhews to be the greateſt

6. Having thus found, that the air hath an elaſtic power, we were deſirous to know how far a parcel of that fluid might be dilated by its own ſpring.

The dilatation of air by its ſpring, meaſured.

We thoroughly wetted a limber lamb's bladder, in water, that the ſides of it being ſqueezed together, no air might be left in its folds, and ſtrongly tied the neck of it about that of a ſmall glaſs, capable of holding five drams of water; the bladder being firſt ſo ſqueezed, that the air it contained was wholly forced into the glaſs, without being compreſſed there; then the pump being ſet on work, the air, in the vial, ſoon began to dilate, produced a ſmall tumour in the neck, and gradually came further into the bladder; elevating the ſides, and diſplaying the folds, till, at length, it ſeem'd blown up to its full extent; when the external air, being permitted to return into the receiver, the air that had filled the bladder, was thereby reduced into its former narrow receptacle, and the bladder became flaccid and wrinkled, as before. Then taking out the bladder, and glaſs, we filled them both with water, thro' a hole made in the top of the bladder; and found the weight of it to be five ounces, five drams, and a half. So that the air, at its utmoſt expanſion, poſſeſs'd above nine times the ſpace it did when firſt put into the receiver.

But to meaſure the expanſive force of the air more accurately, we took a cylindrical pipe of glaſs, its bore about a quarter of an inch in diameter, its length about ſeven inches, and left it open at one end; but the other, where it was hermetically ſealed, had a ſmall glaſs bubble, to receive the air, whoſe dilatation was to be meaſured. Along the ſide of this tube, we paſted a ſlip of parchment, divided into twenty-ſix equal parts, mark'd with black lines, to meaſure both the included air, and its expanſion. Afterwards we almoſt fill'd the tube with water; when, ſtopping the open end, and inverting it, the air was permitted to aſcend to the bubble; and, as the aſcent was very ſlow, it gave us the opportunity, to mark how much more, or leſs than one of thoſe diviſions, this air took up. Thus, after a trial or two, we conveyed to the top of the glaſs, a bubble of air, apparently equal to one of thoſe diviſions; then the open end of the tube being put into a ſmall vial, whoſe bottom was cover'd with water, we included both glaſſes in a ſmall ſlender receiver, and cauſed the pump to be work'd. The event was, that, at the firſt exſuction of the air, there ſeem'd not any expanſion of the bubble, comparable to what appear'd at the ſecond; and, after a few ſtrokes, the bubble, reaching as low as the ſurface of the ſubjacent water, gave us cauſe to think, that it would have expanded much farther, had there been room. We, therefore, took out the little tube, and found that, beſides the twenty-ſix diviſions, the glaſs bubble, and ſome part of the pipe, to which the parch-

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greateſt liquor is capable of, tho' ever ſo long detained upon a vehement fire, increaſes the ſpring of the air as much as about $\frac{1}{3}$ of the weight of the atmoſphere, ſhewn by the barometer, in ſpring, or autumn: and upon this foundation, he ingeniouſly attemps to eſtabliſh an uniformity in thermometers. See *Memoirs de l' Academ.* A. 1702. p. 204. A. 1703. p. 61, &c.

ment did not reach, amounted to ſix diviſions more. Whence it appears, that the air poſſeſſed one and thirty times more ſpace than before; and yet ſeem'd capable of a far greater expanſion. Wherefore, after the ſame manner, we let in another bubble, that ſeem'd but half as big as the former, and found, that upon exhauſting the receiver, it did not only fill up the whole tube, but, in part, broke thro' the water in the vial; and thereby manifeſted itſelf to have poſſeſs'd above ſixty times its former ſpace.

Finding, then, that our tube was ſtill too ſhort, we took a ſlender conical one, thirty inches long, hermetically ſeal'd at the ſlender end, and almoſt fill'd it with water; and conveying a bubble of air to the top of it, we put the open end in a vial, as before: then the cover, by means of a ſmall hole made in it, for the glaſs-pipe to come out at, was cemented to the receiver; and the pump being ſet on work, the air manifeſtly appear'd extended below the ſurface of the water; and ſome bubbles were ſeen to come out at the bottom of the pipe, and break thro' the water. This done, we left off pumping, and obſerved, that at unperceived leaks of the receiver, the air got in ſo faſt, that it very quickly impelled up the water to the top of the tube; excepting a little ſpace, whereinto that bubble was driven, which had before poſſeſs'd the whole tube. This air at the ſlender end, appear'd to be a cylinder of $\frac{5}{6}$ inch in length; but when the pipe was taken out and inverted, it ſeemed at the other end, leſs in bulk than a pea. Then with a ſmall pair of ſcales, weighing the tube and water, we found they amounted to one ounce thirty grains and a half; and filling the tube with water, and weighing again the pipe and water, we found the weight increaſed only by one grain. Laſtly, pouring out the water, and carefully freeing the pipe from it, we weigh'd the glaſs alone, and found it wanted two drams and thirty-two grains of its former weight. So that the bubble of air poſſeſſing the ſpace but of one grain weight of water, it appear'd that this air, by its own ſpring, was rarified to one hundred fifty-two times its former dimenſions; tho' it had been compreſſed only by the ordinary weight of the contiguous air. The experiment, indeed, was made in a moiſt night, and in a room with a large fire; which did, perhaps, ſomewhat rarify the bubble of air.

It hath ſeem'd almoſt incredible, what *Merſennus* relates, that the air by the violence of heat, may be dilated ſo as to take up ſeventy times its natural ſpace: we therefore, once more, conveyed into the tube a bubble of the ſame bigneſs with the former; and proſecuting the experiment as before, we obſerved, that the air did manifeſtly ſtretch itſelf ſo, as to appear, ſeveral times, far below the ſurface of the water in the vial; and that too, with a ſurface very convex toward the bottom of the pipe. Nay, the pump being ply'd a little longer, the air reached to that place, where the tube reſted upon the bottom of the vial, and ſeem'd to hit againſt and rebound from it. Whence it is probable, if the experiment could be ſo made that the expanſion of the air might not be reſiſted, it would yet enlarge its bounds, and perhaps ſtretch itſelf to more than two hundred times its former bulk. And this may render many phenomena of our engine credibile; ſince, of that part of the atmoſphere wherein we live, what we call the free air, and preſume to be uncompreſs'd, is crowded into ſo very ſmall a portion of the ſpace, it would, if unreſiſted, poſſeſs.

The ſtrength of glaſs, and the advantages of figure in ſuſtaining a preſſure.

7. To diſcover the ſtrength of glaſs, and what intereſt the figure of a body may have in reſiſting a preſſure, we made the following experiments.

A round glaſs bubble, capable of containing five ounces of water, being purpoſely blown very thin, and with a ſlender neck, we moderately emptied the receiver, and nimbly applied the neck of the bubble to the orifice of the bottom of it; and after turning the key of the ſtop-cock, we made a free paſſage for the air to come out of the bubble into the receiver; which it did with great celerity; leaving the bubble as empty as the receiver itſelf. We then let in the external air, which now preſſed only on the outſide of the exhauſted bubble, being prevented from getting within it; nevertheleſs, it continued as intire as before; the roundneſs of its figure enabling it, tho' almoſt as thin as paper, to reſiſt a preſſure equal to that of the whole incumbent atmoſphere. And repeating the experiment, we found again that the preſſure of the air, thruſting all the parts inwards, made them, by reaſon of their arched figure, ſo ſupport one another, that the glaſs would not break.

Fig. 32.

8. We took a glaſs alembic, containing between two and three pints; the roſtrum C, being hermetically ſeal'd; and at the top of it was a hole, wherein we cemented one of the ſhanks of a ſtop-cock; ſo that the glaſs being inverted, the wide orifice ſtood uppermoſt; and to this was cemented a cover of lead: the other ſhank of the ſtop-cock was alſo, with cement, faſten'd into the upper part of the pump, which beginning to be work'd, the remaining air became by much too weak to balance the preſſure of the external air, when the glaſs was, with a great noiſe, cracked almoſt half round, along that part of it where it began to bend inwards; as in the line A B; and upon attempting to evacuate more of the air, the crack appear'd to run further, tho' the glaſs, where it was broken, ſeem'd above twenty times as thick as the bubble employ'd in the preceding experiment. Hence it may ſeem ſtrange, that taking another glaſs bubble, alike in all reſpects, for ought appear'd, to that juſt mention'd, ſealing it up, hermetically and ſuſpending it in the receiver, the exſuction of the ſurrounding air did not enable the internal air to break or crack it: and this proved the caſe, tho' the experiment were tried ſeveral times, with bubbles of different ſizes. But, perhaps, the heat of the lamp, wherewith ſuch glaſſes are hermitically ſealed, might rarify the contained air, and weaken its ſpring.

Fig. 33.

9. Into the neck of a common four-ounce vial, we put a ſlender pipe of glaſs, and carefully faſten'd it, with a mixture of pitch and roſin, to the neck thereof. This vial, containing water that reach'd conſiderably higher than the lower end of the pipe, was put into a ſmall receiver, in ſuch manner, that the glaſs pipe, paſſing thro' a hole in the leaden cover of the receiver,

was principally without the veſſel; which being exactly cloſed, we work'd the pump; but at the very firſt ſtroke, and before the ſucker was drawn to the bottom of the cylinder, there flew out of the vial a large piece of glaſs, with a ſurprizing violence and noiſe, ſo as to crack the receiver in many places.

For farther ſatisfaction, we repeated the experiment in a round glaſs, that would contain ſix ounces of water; which we put into a ſmall receiver, ſo that the bottom of it reſted upon the lower part of the receiver, and the neck came out thro' the leaden cover of the ſame. This vial we included in a bladder, before it was put in, and the receiver being clos'd, ſo that the outward air could not enter but by breaking thro' the vial, into whoſe cavity it had free acceſs by the mouth, the ſucker was nimbly drawn down; upon which the external air immediately preſs'd forcibly, as well upon the leaden cover as the vial; and the cover happening to be in one place a little narrower than the edge of the receiver, it was depreſs'd, and thruſt into it ſo violently, that getting a little within the lip of the glaſs, it thruſt out the ſide, where it was depreſs'd, ſo as to ſplit the receiver. And having fitted a wider cover to the ſame receiver, and clos'd both that, and the crack with cement, we proſecuted the experiment in the former manner with this ſucceſs; that, upon ſuddenly depreſſing the ſucker, the external air burſt the vial into above a hundred pieces, many of them exceeding ſmall, and with ſuch violence, that we found a wide rent, and many holes made in the bladder.

And to ſhew, that theſe phenomena were the effects of a limited force, and not of ſuch an abhorrence of a vacuum, as muſt, upon occaſion, exerciſe a boundleſs power, we try'd ſeveral thicker glaſſes, and found that the experiment would not ſucceed; for the glaſſes were taken out, as entire as they were put in.

And here, by the way, we may obſerve, that every ſmall crack will not render a roundiſh receiver uſeleſs in our experiments, becauſe, upon evacuation of the internal air, the external on all ſides preſſing the glaſs towards the center, thruſts the edges of the crack cloſer together.

And, in caſe of conſiderable flaws, we ſucceſsfully apply a plaiſter, made of quick-lime, finely powder'd, and nimbly ground, with a proper quantity of the ſcrapings of cheeſe, and fair water, enough to bring the mixture to a ſoft paſte; which, when the ingredients are exquiſitely incorporated, will have a ſtrong, and fetid ſcent; and then it muſt be immediately ſpread upon a linen cloth, and applied, leſt it begin to harden.

The flame of tallow and of wax in vacuo.

10. We let down, into our receiver, a tallow-candle of a moderate ſize, and ſuſpending it, ſo that the flame appear'd in the middle of the veſſel, we preſently clos'd it up, and upon pumping, found, that within little more than half a minute after, the flame went out.

At another time, the flame laſted about two minutes, tho' upon the firſt exſuction it ſeem'd to contract itſelf in all its dimenſions, and after two or three exſuctions, it appear'd exceeding blue, and gradually receded from the tallow, till at length it ſeem'd to poſſeſs only the very top of the wiek, and there it vaniſh'd.

The ſame candle, being lighted again, was ſhut into the receiver, to try how it would burn there, without exhauſting the air; and we found that it laſted much longer than formerly; and before it went out, it receded from the tallow, towards the top of the wiek, tho' not near ſo much, as in the former experiment.

We took notice, that when the air was not drawn out, a conſiderable part of the wiek remain'd kindled upon the extinction of the flame, which emitted a ſmoke, that ſwiftly aſcended directly upwards, in a ſlender and uninterrupted cylinder, till it came to the top, from whence it return'd, by the ſides, to the lower part of the veſſel; but when the flame went out, upon the exſuction of the air, we once perceiv'd it not to be follow'd by any ſmoke at all. And at another time, the upper part of the wiek, remaining kindled after the extinction of the flame, a ſlender ſteam aſcended, but a very little way, and after ſome uncertain motions, for the greateſt part, ſoon fell downwards.

Joining together ſix ſlender tapers of white wax, as one candle, and having lighted all the wieks, we let them down into the receiver, and made what haſte we could to cloſe it up with cement. But, tho' in the mean while, we left open the valve of the cylinder, the hole of the ſtop-cock, and that in the cover of the receiver, that ſome air might get in to cheriſh the flame, and that the ſmoke might have a vent; yet the air ſuffic'd not for ſo great a flame, till the cover could be perfectly luted on; ſo that before we were ready to employ the pump, the flame was extinguiſh'd. Wherefore we took but one of the tapers, and having lighted it, clos'd it up in the receiver, to try how long a ſmall flame, with a proportionable ſmoke, would continue in ſuch a quantity of air; but we found, upon two ſeveral trials, that from our beginning to pump, the flame went out in about a minute. It appear'd indeed, that the ſwinging of the wyre, whereby the candles hung, haſten'd the extinction of the flame, which ſeem'd, by the motion of the pump, to be thrown, ſometimes on one ſide of the wiek, and ſometimes on the other. But, once refraining to pump, after a very few exſuctions, the flame laſted not much longer. And laſtly, cloſing up the ſame lighted taper, to diſcover how long it would laſt, without drawing out the air; we found, that it burnt vividly for a while; but afterwards, began to diminiſh gradually in all its dimenſions, tho' the flame did not, as before, retire itſelf by little and little towards the top, but towards the bottom of the wiek, ſo that the upper part of it, manifeſtly appear'd for ſome time, above the top of the flame; which, having laſted about five minutes, was ſucceeded by a ſtream of ſmoke, that aſcended in a ſtrait line.

Kindled charcoal.
Fig. 34.

11. A ſpiral wyre, fill'd to the height of about five inches, with wood-coals throughly kindled, being let down into the receiver, and the pump ſet to work; we obſerv'd, that upon the very firſt exſuction of the coals, the fire grew dim, and tho' the agitation of the veſſel made them ſwing; yet, when we could no longer diſcern a redneſs in any of them,

we found that, from the beginning of the pumping, that is, about two minutes after the coals had been put in, glowing, to the total diſappearing of the fire, there had paſs'd three minutes.

We then, preſently, took them out, and found there had remain'd ſome little parcels of fire, rather cover'd, than totally extinguiſh'd; for, in the open air, the coals began to re-kindle in ſeveral places. Wherefore, having, by ſwinging them about in the wire, throughly lighted them a ſecond time, we let them down again into the receiver; and cloſing it, waited till the fire ſeem'd totally extinct, without working the pump, and found that from the time the veſſel was cloſed, till no fire at all could be perceiv'd, there had elapſed four minutes.

Laſtly, having taken out the wire, and put other coals into it, we, in the ſame room where the engine ſtood, let it hang quietly by a ſtring, in the open air; and found that the fire began to go out firſt at the top, and outſides of the coals; but inwards, and near the bottom, it continu'd viſible for above half an hour; a great part of the coals, eſpecially the lowermoſt, being reduced to aſhes before the fire was extinguiſh'd.

Red-hot iron. A piece of iron, of the bigneſs of a middle-ſized charcoal, being, alſo, made red-hot throughout, we ſuſpended it in the exhauſted receiver; but could not obſerve any manifeſt change upon the exſuction of the air. The iron, indeed, began to loſe its fiery redneſs at the top; but that ſeem'd owing to the upper-end's being ſomewhat more ſlender than the lower; and the redneſs, tho' it were in the day-time, continued viſible about four minutes; and then before it quite diſappear'd, we let in the air, but no change enſued. Yet ſome little remainders of wax, that ſtuck to the wire, and were turn'd into fumes by the heat of the iron, afforded a more diffuſive ſmoke when the air was drawn out, than afterwards; tho' allowance were made for the decreas'd heat of the metal. And laſtly, notwithſtanding a conſiderable extraction of the air, and the inconſiderable diſſipation of the parts of the iron, the ſides of the receiver were very ſenſibly hot, and retain'd a warmth for ſome time after the iron was taken out.

Lighted match. 12. We ſuſpended a piece of well-lighted match, in our receiver, with the lighted end downwards, when the fumes of it, almoſt, immediately fill'd, and darken'd the receiver. Wherefore, leſt the veſſel ſhould be endanger'd, the pump was nimbly ply'd, and a great deal of air and ſmoke, mix'd together, drawn out; whereby the receiver growing more clear, we could diſcern the fire in the match, to burn, by degrees, more languidly; and, after no long time, it ceas'd to be diſcernible either by its light, or ſmoke. And tho' we continued pumping for a while longer, yet, upon admiſſion of the external air, the fire, that ſeem'd to have been long extinguiſh'd, preſently reviv'd, and began again to ſhine, and diſſipate the adjacent fewel into ſmoke, as before.

13. We, afterwards, let down into the receiver, together with a piece of lighted match, a large bladder, well tied at the neck, and containing only about a pint of air, tho' capable of containing ten times as much.

This was deſign'd to try, whether the ſmoke of the match, repleniſhing the receiver, would hinder the dilatation of the internal air, upon the extraction of the external; and to diſcover whether the extinction of the fire in the match proceeded from want of air, or, barely, from the preſſure of its own fumes.

The event was, that, at the beginning of our pumping, the match appeared well lighted, tho' it had almoſt fill'd the receiver with ſmoke; but, by degrees, it burnt more dimly; tho', by nimbly drawing out the air and ſmoke, the veſſel became leſs opake: ſo that the longer we pump'd, the leſs air and ſmoke came out of the cylinder, upon opening the valve; yet the fire in the match went out but ſlowly. And when, afterwards, we had darken'd the room, and, in vain, attempted to diſcover any ſpark of fire, we ſtill continued pumping; and, at laſt, letting in the air, the fire quickly revived, yielded light, and plenty of ſmoke. Then we fell to pumping afreſh, and continued it till long after the match went out again; ſo that in leſs than half a quarter of an hour, the fire was extinguiſhed, beyond the poſſibility of a recovery by re-admitting the air. If the cylinder were emptied, when the receiver was full of ſmoke, immediately upon turning of the ſtop-cock, the receiver would appear manifeſtly darkned, to an eye viewing the light thro' it; and this darkneſs was leſs, as the receiver contain'd leſs ſmoke: it was alſo inſtantaneous, and ſeem'd to proceed from a ſudden change of place and ſituation, in the exhalations, upon the vent afforded them, and the air they were mix'd with, out of the receiver into the cylinder. We alſo obſerv'd a kind of a halo, for a conſiderable time, about the fire, that ſeem'd to be produced by the ſurrounding exhalations. And, when the fumes ſeem'd moſt to repleniſh the receiver, they did not, ſenſibly, hinder the air, included in the bladder, from dilating itſelf, after the ſame manner it would otherwiſe have done: ſo that, before the match was quite extinct, the bladder appear'd diſtended to ſix or ſeven times its former dimenſions.

We, alſo, took a ſmall receiver, capable of containing about a pound and a half of water, and, in the midſt of it, ſuſpended a lighted match; but tho' within a minute, from putting in the match, we had cemented on the cover, yet before we began to pump, the ſmoke had ſo fill'd the receiver, as, apparently, to choke the fire. And finding it thus impoſſible to cloſe up the veſſel, and pump out the fumes ſoon enough to prevent the extinction of the fire, we uſed this expedient: as ſoon as we had pump'd once or twice, we ſuddenly turn'd the key, and thereby gave acceſs to the excluded air, which ruſhing violently in, drove away the aſhes, fill'd the glaſs with freſh air, and re-kindled the fire; and having, by this means, obtain'd a lighted match in the receiver, without ſpending time to cloſe it up, we exhauſted the receiver, and found the match then quickly ceas'd to ſmoke.

And gun-powder fired in vacuo.

14. We took a piſtol, and having firmly ty'd it to a ſtick, almoſt as long as the cavity of the receiver, we primed it with dry gun-powder; then cocking it, we faſten'd the trigger to one end of a ſtring, whoſe other end

was faſten'd to the key in the cover of our receiver. This done, we convey'd the whole apparatus into the veſſel, which being cloſed up, and emptied after the uſual manner, we turn'd the key in the cover, and thereby ſhortning the ſtring, pull'd the trigger, and obſerv'd, that the force of the ſpring of the lock was not ſenſibly abated by the abſence of the air; for, the cock falling with its uſual violence, ſtruck as many, and as conſpicuous ſparks of fire, as, for ought we could perceive, it did in the open air. Upon often repeating this experiment, we could not perceive, but that the ſparks of fire moved upwards, downwards, and ſideways, as when out of the receiver.

We, likewiſe, ſubſtituted a piece of ſteel for the flint, when, the piſtol being cock'd, and convey'd into the receiver, we pull'd the trigger, after the air was drawn out; and tho' the place were purpoſely darkned, there appear'd not, upon the colliſion of the two ſteels, the leaſt ſpark of fire. We have, indeed, found, that, by the dextrous colliſion of two harden'd pieces of ſteel, many ſparks may be ſtruck out; but that was done with ſuch a vehement percuſſion of their edges, as could not well be procured in our receiver.

But moſt of our attempts, to fire the gun-powder in the pan of the piſtol, fail'd, becauſe we were obliged to let it hang, almoſt perpendicularly, in the receiver; whereby the powder was ſhook out, before the ſparks could reach it. Once, however, the experiment ſucceeded; and the kindled powder ſeem'd to make a more expanded flame, than it would have done in the open air, and mounted upwards: upon the extinction of the flame, the receiver appear'd darkned with ſmoke, which ſeem'd to move freely up and down, and, upon letting in the air, began to circulate much faſter than before.

An attempt to kindle a combuſtible body, by the ſun's rays in vacuo.

15. We convey'd into a ſmall receiver, a piece of combuſtible, dry, black matter; and carefully cloſing the veſſel, we brought it to a window, at which the ſun ſhone in very freely; then, drawing out the air, we, with a burning-glaſs, threw the ſun's rays upon the combuſtible matter, which began immediately to ſend out a ſmoke that darkned the receiver; but, notwithſtanding all our care, the external air got in, and fruſtrated the experiment.

We, therefore, lodg'd this combuſtible matter in the cavity of our largeſt receiver, ſo that it was almoſt contiguous to the ſide next the ſun: we then endeavour'd to kindle it, but found, that by reaſon of the thickneſs of the glaſs, the ſun-beams, thrown in by the burning-glaſs, were, in their paſſage, ſo diſlocated, and ſcatter'd, that we could not, poſſibly, unite enow of them, to make the matter yield a ſenſible ſmoke.

An excited needle in vacuo *affected by the magnet.*

16. We convey'd into the receiver, a little pedeſtal of wood, in the midſt of which was, perpendicularly erected, a ſlender iron, upon the ſharp point whereof, an excited needle of ſteel, of about five inches long, was ſo placed, that, hanging in equilibrium, it could move freely every way. Then the air being pump'd out, we employ'd a load-ſtone, moderately vigorous, to the outſide of the glaſs, and found that it attracted, or

repell'd the ends of the needle, without any remarkable difference from what the ſame load-ſtone would have done, had none of the air been drawn away from about the needle; which, when the load-ſtone was remov'd, reſted, after ſome tremulus vibrations, in a poſition north and ſouth.

The Torricellian *experiment* in vacuo.

17. A ſlender, and very exact cylinder of glaſs, near three feet in length; its bore, a quarter of an inch in diameter; being hermetically ſeal'd, at one end, was, at the other, fill'd with quick-ſilver; care being taken, that as few bubbles as poſſible, ſhould be left in the mercury. Then the tube, being ſtopt with the finger, and inverted, was open'd into a long, ſlender, cylindrical box, half fill'd with quick-ſilver; when that in the tube ſubſiding, and a piece of paper being paſted level to its upper ſurface, the box and tube were, by ſtrings, carefully let down into the receiver; and the cover, by means of this hole, ſlipt along as much of the tube, as reach'd above the top of the receiver: the interval left betwixt the ſides of the hole, and thoſe of the tube, being exquiſitely fill'd up with melted diachylon; and the round chink, betwixt the cover and the receiver, likewiſe, very carefully clos'd; upon which cloſure, there appear'd no change in the height of the mercurial cylinder: whence the air ſeems to bear upon the mercury, rather by virtue of its ſpring, than of its weight; ſince its weight could not be ſuppos'd to amount to above two or three ounces; which is inconſiderable, in compariſon of ſuch a cylinder of mercury as it would ſuſtain. Now the ſucker was drawn down, and immediately, upon the evacuation of a cylinder of air, out of the receiver, the quick-ſilver in the tube ſubſided; and notice being carefully taken of the place where it ſtopt, we work'd the pump again, and mark'd how low the quick-ſilver fell at the ſecond exſuction: but, continuing thus, we were ſoon hinder'd from accurately marking the ſtages in its deſcent, becauſe it preſently ſunk below the top of the receiver: ſo that we could, from hence, only mark it by the eye. And continuing pumping, for about a quarter of an hour, we could not bring the quick-ſilver in the tube, totally to ſubſide. Then we let in ſome air; upon which, the mercury began to re-aſcend in the tube, and continu'd mounting, till having return'd the key, it immediately reſted at the height it had then attain'd. And ſo, by turning, and returning the key, we did, ſeveral times, impel it upwards, and check its aſcent; till, at length, admitting as much of the external air, as would come in, the quick-ſilver was impell'd up, almoſt, to its firſt height; which it could not fully regain, becauſe ſome little particles of air were lodg'd among thoſe of the quick-ſilver, and roſe in bubbles to the top of the tube.

It is remarkable, that having, two or three times, try'd this experiment, in a ſmall veſſel; upon the very firſt cylinder of air that was drawn out of the receiver, the mercury fell, in the tube, 18 inches and a half; and, at another time, 19 inches and a half.

We, likewiſe, made the experiment in a tube leſs than 2 feet in length; and, when there was ſo much air drawn out of the receiver, that the remaining part could not counter-balance the mercurial cylinder, it fell above a ſpan at the firſt ſtroke; and the external air being let in, impell'd it up again, almoſt to the top of the tube: ſo little matters it, how heavy or light the cylinder of quick-ſilver be, provided its gravity overpower the preſſure of as much external air, as bears upon the ſurface of that mercury into which it is to fall.

Laſtly, we obſerv'd, that if more air were impell'd up, by the pump, into the receiver, after the quick-ſilver had regain'd its uſual ſtandard in the tube, it would aſcend ſtill higher; and immediately, upon letting out that air, fall again to the height it reſted at before.

But, in order to fill the *Torricellian* tube with exactneſs, the edges of the open end ſhould be made even, and turned inwards, that ſo the orifice, not much exceeding a quarter of an inch in diameter, may be the more eaſily, and exactly ſtopp'd by the finger; between which, and the quick-ſilver, that there may be no air intercepted, it is requiſite that the tube be perfectly full, that the finger, preſſing upon the protuberant mercury, may rather throw ſome out, than not find enough to keep out the air exactly. It is, alſo, an uſeful way, not quite to fill the tube, but to leave, near the top, about a quarter of an inch empty: for, if you then ſtop the open end, and invert the tube, that quarter of an inch of air will aſcend in a great bubble to the top; and, in its paſſage, lick up all the little bubbles, and unite them with itſelf, into one great one. So that, if by re-inverting the tube, you let that bubble return to the open end of it, you will have a much cloſer mercurial cylinder than before; and need add but a very little quick-ſilver more, to fill up the tube exactly. And, laſtly, as for ſuch leſs, and inviſible parcels of air, which cannot be thus gather'd up, you may endeavour, before you invert the tube, to free the quick-ſilver from them, by ſhaking the glaſs, and gently knocking on the outſide of it, after every little parcel of quick-ſilver pour'd in; and afterwards, forcing the bubbles to diſcloſe themſelves, and break, by applying a hot iron near the top of the glaſs; which will raiſe the bubbles ſo powerfully, as to make the mercury appear to boil. I remember, that by carefully filling a ſhort tube, tho' not quite free from air, we have made the mercurial cylinder reach to thirty inches, and above an eighth; which is mention'd, becauſe we have found, by experience, that in ſhort tubes, a little air is more prejudicial to the experiment, than in long ones.

Odd phenomena of the mercurial barometer.

18. We fill'd a glaſs tube, about three feet long, with mercury; and having inverted it into a veſſel of other quick-ſilver, that in the tube fell down to its uſual height; leaving ſome little particles of air in the ſpace it had deſerted: for, by the application of hot bodies to the upper part of the tube, the quick-ſilver would be a little depreſs'd. Laſtly, having put both the tube, and the veſſel whereon it reſted, into a convenient wooden frame, we placed them together in a window of my chamber.

And during ſeveral weeks that the tube continu'd there, I obſerv'd, that the quick-ſilver did, ſometimes faintly imitate the liquor of a thermometer;

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meter; ſubſiding a little in warm, and riſing a little in cold weather; which we aſcrib'd to the greater, or leſſer preſſure of that little air, which remain'd at the top of the tube, expanded, or condens'd by the heat or cold of the ambient air. But the quick-ſilver often roſe, and fell in the tube very conſiderably, after a manner, quite contrary to that of weather-glaſſes, where air is at the top; for ſometimes I obſerv'd it, in very cold weather, to ſink much lower than at other times, when the air was comparatively warmer. And ſometimes the quick-ſilver would, for ſeveral days together, reſt almoſt at the ſame height; and at others, it would in the compaſs of the ſame day conſiderably vary its altitude; tho' there appear'd no change, either in the air abroad, or in the temper of that within my room, nor in any thing elſe, to which ſuch a change could reaſonably be imputed; eſpecially conſidering, that the ſpace wherein the mercury continued unſettled for five weeks, amounted to full two inches; deſcending in that time about $\frac{9}{10}$ of an inch from the place where it firſt ſettled, and aſcending the other inch, and $\frac{7}{10}$: and when we took the tube out of the frame, after it had ſtaid there part of *November*, and *December*, a large fire being then in the room, we found the mercurial cylinder to be above the upper ſurface of the ſtagnant mercury 29$\frac{1}{4}$ inches.*

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* That the quick-ſilver in the barometer ſhould ſtand lower, when the air is thick and moiſt, than when it is dry and clear, ſeems to overthrow the theory of the air's gravitation. Indeed, to diſcover the cauſes of all the minute variations in the air, is a very difficult task. The winds have a great ſhare herein, with the vapours, exhalations, and expirations of the earth; perhaps alſo, the changes, which happen in the adjacent regions; the flux and reflux cauſed by the moon in the air, no leſs than in the ſea, and many other particulars, are not unconcern'd. Now, the air is heavier, than the vapours it ſuſtains; its particles being more groſs, and ariſing from denſer bodies, than the particles of vapours. But, winds may change this weight of the air, in any particular region; either by bringing, and keeping up more air over it, as may eaſily happen, when two contrary winds blow; or by ſweeping it away, and affording room for the ſubjacent air to expand itſelf; as may be the caſe, when two oppoſite winds meet, or, when only one blows exceeding ſtrong. Thus, 'tis fact, that violent guſts make the mercury in the barometer greatly to ſink of a ſudden. The cold nitrous particles of the air, or the air itſelf, being condenſed by cold in the north, and blown to another quarter, may, not only condenſe the atmoſphere, but make it heavier. Moreover, heavy dry exhalations will increaſe the weight of the air, (as ſalts and metals diſſolv'd in proper menſtrua, increaſe the ſpecific gravity of them;) and perhaps, at the ſame time, add to its elaſticity. Again, the air, by theſe, or the like cauſes, being rendred heavier, is the more able to ſuſtain the vapours; which therefore coming to be intimately mix'd therewith, and floating every where uniformly therein, render it fair and clear. But, when from contrary cauſes it becomes lighter, 'tis rendred unable to ſuſtain the vapours, which always oppreſs it; ſo that being, as it were, precipitated together, they form clouds, and running into drops, fall, by their increaſed gravity, to the earth. Hence we ſee, what cauſes render the air heavier, and more able to ſuſtain the quick-ſilver in the barometer, namely, ſuch as make the air clear and dry: but the cauſes, which render the air light and unfit to ſuſtain the mercury, produce rain. When therefore the air is lighteſt, and the mercury in the barometer loweſt, the clouds appear very low, and in very ſwift motion; and the air having clear'd itſelf of its clouds by rain, becomes very bright and tranſ-

Such an inequality in the riſe, and fall of the mercury will, I fear, render it difficult to determine by the barometer, whether the moon be the cauſe of the tides, eſpecially, till the reaſon of this odd phenomenon be certainly known; which ſeems principally to depend upon conſiderable alterations in the air, in point of rarity and denſity.

A like experiment made with water.

19. We took a tube of glaſs, about four feet in length, hermetically ſeal'd at one end, fill'd it with common water, and inverted the open end, beneath the ſurface of a veſſel of water. Then this veſſel, with the tube in it, being let down into the receiver, the pump was ſet on work; when, till the receiver was moderately exhauſted, the tube continu'd quite full of water; it being requiſite, that a great part of the air contain'd in the receiver ſhould be drawn out, to bring the remaining to an equilibrium, with ſo ſhort a cylinder of water. But, when once the water began to fall in the tube, each exſuction of air made it deſcend a little lower; tho' nothing near ſo much, nor ſo unequally, as the quick-ſilver did. The loweſt, we were able to draw down the water, was, to about a foot above the ſurface of that in the veſſel. And, when the water was drawn down thus low, we found, that by letting in the outward air, it might be immediately impell'd up again, to the higher parts of the tube.

Upon making this experiment in a ſmall receiver, we obſerv'd, that at the firſt exſuction of the air, the water uſually ſubſided ſeveral inches; and at the ſecond, ſometimes near two feet; whereupon letting in the external air, the water was impell'd up, with a very great velocity.

Whether water be elaſtic?

20. That the air hath a conſiderable elaſtic power, we have abundantly proved; but, whether water participates, in any meaſure, thereof, ſeems hitherto, to have been ſcarce conſider'd.

Into a large glaſs bubble, with a long neck, we pour'd common water, till it reach'd about a ſpan above the bubble; and a piece of paper being paſted thereon, we put it, unſtopt, into the receiver; when, the pump was

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tranſparent, ſo as to afford an excellent proſpect of remote objects. But, when it is heavy, and the quick-ſilver ſtands high in the barometer, the heavens appear fair, but ſomewhat thick, by reaſon of the vapours, every where equally diſperſed therein, and is leſs fit to afford a good view of objects at a diſtance. And if any clouds are ſeen, they be very high, and move ſlow. When the air is at the heavieſt, thick clouds ſometimes cover the earth, conſiſting probably of ſuch exhalations, as the air, at that time, is unable to ſuſtain; and which, cannot float therein, when it is light. In our climate, the barometer ſtands higheſt, when the weather is coldeſt, and when the eaſt, or north eaſt winds blow; becauſe, at that time, two winds blow together, from oppoſite parts; for in the *Atlantic* ocean, at the degree of latitude anſwering to ours, the wind, almoſt continually blows weſt; and when the north-wind blows, an air condens'd by cold is brought to us. Farther, in the moſt northern regions, the height of the barometer varies more, than in the ſouthern; the winds being there more ſtrong, changeable, and contrary to one another, on a ſmall tract of land; whereby, at one time, they heap up, and condenſe the air, and at another, ſweep it away, and rarify it. Laſtly, the barometer varies leaſt between the tropics, becauſe the wind is there almoſt always gentle, and blows the ſame way. See *Clark. Annotat. in Rohault.* & *Philoſ. Tranſ.* No. 181. 292.

was work'd, after the uſual manner, and a conſiderable part of the air in the receiver drawn out, before we diſcern'd any expanſion of the water; but continuing to pump, the water manifeſtly began to aſcend in the ſtem of the glaſs, and ſeveral bubbles, from the lower parts of the veſſel, made their way thro' the liquor to the top of it, and there broke into the receiver. After the water once appear'd to ſwell, at each time the air was let out from the receiver into the pump, the water in the neck of the glaſs, ſuddenly roſe, about the breadth of a barley-corn, and ſo by degrees attain'd to a conſiderable height, above the mark. And at length, the external air, being ſuddenly re-admitted, the water immediately ſubſided, and deſerted all the additional ſpace, it had gain'd in the glaſs.

21. We convey'd into the receiver a new glaſs-vial, capable of holding about 6 or 7 ounces of water; into which we had before-hand put only 2 or 3 ſpoonfuls of that fluid, and ſtopt it cloſe with a fit cork. The receiver being emptied, there appear'd no change in the incloſed water; the air, impriſon'd with it, not having the force to blow out the ſtopple. Wherefore, we again put in the vial, leſs firmly cloſed than before; but when the air was pumped out of the receiver, that within the vial quickly found little paſſages to get out at; for when the vial was put in the time before, the water remain'd all the while perfectly free from bubbles; but now the bottom of the glaſs appear'd all cover'd with them, which, upon the return of the excluded air, preſently ſhrunk up.

Hence it ſeem'd deducible, that, whilſt the vial continu'd well ſtopt, the included water ſuſtain'd, from the air ſhut up with it, a preſſure equal to that of the atmoſphere; ſince, till the air could get out of the glaſs, there appear'd no bubbles in the water, notwithſtanding the want of preſſure in the ambient body.

But, further, we cauſed a convenient quantity of water to be hermetically ſeal'd up in a glaſs-egg, whoſe long neck was faſten'd to one end of a ſtring, the other end whereof was ty'd to the cover of our receiver; then the egg being convey'd into the receiver, and that being evacuated, we, by turning the braſs ſtopple, ſo ſhorten'd the ſtring, as to break the glaſs; whereby liberty being given to the air impriſon'd in the egg, to paſs into the receiver, its ſudden receſs made ſo many bubbles appear immediately, and aſcend ſo ſwiftly in the water, that their motion look'd like that of a violent ſhower of rain; except that the bubbles did not, like the drops of rain, tend downwards, but upwards; as happens in the diſſolution of ſeed pearl, in ſome very acid menſtruum, wherein, if a large quantity of the pearls be caſt whole, they will, at firſt, be carry'd in ſwarms from the bottom to the top of the liquor. And, without ſealing up the glaſs, this experiment may be try'd in a ſmall receiver; for the air may here be drawn out ſo ſoon, that the bubbles, lurking in the water, will, immediately, diſplay themſelves, and aſcend in throngs. So that, having made the experiment, in ſuch a receiver, with red wine, inſtead of water, the wine appear'd all cover'd with a large vaniſhing white froth.

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22. To diſcover whether the expanſion of the water really proceeded from an elaſtic power in the parts of that fluid; we fill'd a glaſs-vial, with a pound and ſome ounces of water, and then put into it a glaſs-pipe, open at both ends, and ſeveral inches in length, ſo as to reach a little below the neck; then we carefully cemented it thereto, that no air might come into the vial, nor any water get out of it, but thro' the pipe; and the pipe, being warily fill'd about half way with water, and a mark being paſted over-againſt the upper ſurface thereof, the whole was, by ſtrings, let down into the receiver; when, pumping out the air, the water in the pipe began to riſe, while ſome little bubbles diſcover'd themſelves on its ſides; and, ſoon after, the water ſtill ſwelling, there appear'd, at the bottom of the pipe, a bubble, about the bigneſs of a ſmall pea; which, aſcending thro' the tube to the top of the water, ſtaid there a while, and then broke. But the pump being nimbly ply'd, the expanſion of the water ſo increas'd, that, quickly getting up to the top of the pipe, ſome drops of it began to run down along the outſide of it; which oblig'd us to forbear pumping a while, and let it ſubſide, as it did, within leſs than two inches of the bottom of the pipe. Then the pump being again ſet on work, the bubbles began to aſcend from the bottom of the pipe; of which we reckon'd about 60 large ones, that aſcended one after another. And, at length, letting in the external air, the water, in the pipe, inſtantly fell down almoſt to the bottom of it.

When the greater part of the air had been pump'd out of the receiver, the bubbles aſcended ſo very ſlowly in the pipe, that their progreſs was ſcarce diſcernible; their magnitude not permitting them ſufficiently to expand themſelves in the cavity of the glaſs, without preſſing againſt the ſides of it. And, what ſeems ſtrange, theſe bubbles were commonly much larger than thoſe which roſe before them; ſome of them being equal in bulk to four or five peas.

And tho, in ordinary bubbles, the air, together with the thin film of water that inveſts it, commonly ſwells above the ſurface of the water, and conſtitutes hemiſpherical bodies; the little parcels of air, that came up after the receiver was tolerably emptied, did not make protuberant bubbles; but ſuch, whoſe upper ſurface was either level with, or beneath that of the water; ſo that, the upper ſurface being uſually ſomewhat convex, the leſs protuberant parts had a quantity of water above them.

We farther obſerv'd, that, in the bubbles which firſt appear'd, the aſcending air made its way upwards, by dividing the water thro' which it paſs'd; in thoſe that roſe at the latter end of the experiment, the aſcending parcels of air, having now little more than the weight of the incumbent water to ſurmount, were able to expand themſelves, ſo as to fill that part of the pipe which they pervaded, and, by preſſing every way againſt the ſides of it, to raiſe what water they found above them, without letting any conſiderable quantity glide down along the ſides of the glaſs; ſo that, ſometimes, we could ſee a bubble thruſt on before it a whole cylinder of water,

water, perhaps an inch high, and carry it up to the top of the pipe; tho', upon letting in the external air, theſe bubbles ſuddenly vaniſh'd.

Hence it appears, that the air, and other bodies under water, may be preſs'd upon as well by the atmoſphere, as by the weight of the incumbent water. Hence, likewiſe, it cannot from the preceding experiment be ſafely concluded, that water uncompreſſed, has an elaſtic power; ſince the intumeſcence, produc'd in that experiment, may be aſcrib'd to the numerous little bubbles produc'd in water, freed from the preſſure of the atmoſphere. And hence, laſtly, it ſeems probable, that, in the interſtices of water, there lie conceal'd many parcels either of air, or ſomething analogous thereto; tho' ſo very ſmall, that they have not been hitherto ſuſpected to lurk there.

23. It may, indeed, be conjectur'd, that theſe bubbles proceed not ſo much from any air in the water, as from the more ſubtile parts of the water itſelf.

We, therefore, repeated our former experiment, in a three-foot tube, fill'd with water, and in a ſmall receiver; and found, that, upon the ſubſiding of the fluid, ſo many bubbles, viſibly broke into the upper part of the tube, that, having afterwards let in the external air, the water was not thereby impell'd to the top, within more than half an inch. Then we, again, drew the air out of the receiver, and found, that, by reaſon of the body which poſſeſs'd the top of the tube, we were able, not only to make the water fall to a level, with the ſurface of that in the veſſel; but alſo a great way beneath it. Now, ſince this could not well be aſcrib'd to the bare ſubſiding of the water by its own weight, the water ſeems to have been depreſs'd by the air. And, indeed, the ſurface of the water, in the tube, was much more concave than uſual. And, by the way, when the water, in the pipe, was ſunk almoſt as low as the water without; we obſerv'd, that, by the bare application of the hand, moderately warm, to the deſerted part of the tube, the remaining water would be, ſuddenly, conſiderably depreſs'd. And having, for a while, held a kindled coal to the outſide of the tube; the air was, by the heat, ſo far expanded, that it quickly drove the water to the bottom of the tube, which reſted ſeveral inches below the ſurface of the ambient water. Hence it appears, that the air, when expanded to between ninety, and a hundred times its natural dimenſions, will, yet, readily admit of a much farther rarifaction, by heat.

But, to proceed; in caſe our bubbles were produc'd by air, lurking in the water; that air being got together at the top of the tube, I imagin'd, if the receiver were again exhauſted, bubbles would not riſe, as before; and, accordingly, the air being again pumped out, the water, in the tube, deſcended; but, for a great while, we ſcarce ſaw one bubble appear; only when the receiver had been very much exhauſted, and the water fallen very low, we diſcover'd, near the bottom of the tube, ſome little ones, which ſeem'd to conſiſt of ſuch parcels of air, as had not, by reaſon of their ſmallneſs, got up to the top of the water, with the more bulky and vigorous ſort. And having, by letting in the air, forced up the water into the tube, we could not perceive that it aſcended near the top, tho' the engine remain'd unemploy'd for two or three nights together. Having, alſo, try'd a like experiment with quick-ſilver, inſtead of water, in a tube about a foot and a half long; upon drawing down the quick ſilver as low as poſſible, and letting in the external air, we found, that ſome lurking particles of air were got up to the top of the tube, and hinder'd the quick-ſilver from riſing to that height again. And, tho' the mercury were, by this means, brought to appear as a very cloſe cylinder; yet the air, in the receiver, being again evacuated, I could perceive ſeveral little bubbles faſten'd to the inſide of the tube, near the bottom. And, having purpoſely watched one or two of the principal, I obſerv'd, that tho' they grew gradually bigger, as the ſurface of the mercurial cylinder fell nearer to them; ſo that, at length, they ſwell'd to a conſiderable bulk; yet, upon letting in the air, they did not break, but preſently ſhrunk up, till they became inviſible.

Hence, it ſeems highly probable, that, even in the cloſeſt, and moſt ponderous liquors, and therefore, much rather in water, there may lurk undiſcernible parcels of air, capable, upon the removal of the preſſure of the atmoſphere, and that of the liquor wherein it lurks, to produce conſpicuous bubbles.

From theſe ſeveral particulars, it ſeems plain, that the bubbles we have been treating of, were produced by ſuch a ſubſtance, as may be properly enough call'd air; tho' we do not, poſitively, determine, whether air be a primogenial body, that cannot be generated, or turn'd into water, or any other body. This ſeems an important queſtion, and might greatly conduce to explain the nature of the air.

Whether air may be generated, or tranſmuted.

Many naturaliſts eſteem the air to be ingenerable, and incorruptible; and plauſible reaſons may be drawn, to countenance this opinion, from the permanency required in the corporeal principles of other bodies. *Schottus* tells us, that, in the *Muſæum Kircherianum*, there is a glaſs, near half full of ordinary ſpring-water, which, having been hermetically ſeal'd up by the famous *Clavius*, is, to this day, preſerv'd not only clear and pure, but without, in the leaſt, turning into air, tho' it has ſtood for fifty years.

Nor doth it appear, in thoſe glaſſes which are hermetically ſeal'd for chymical uſes, that the included air, during its long impriſonment, notwithſtanding the alteration it receives from various degrees of heat, diſcernibly alters its nature; whilſt we plainly perceive, in digeſtions and diſtillations, that, tho' water may be rarified into vapours; yet it is not, really, changed into air, but only divided by heat, and diffuſed into very minute parts; which, meeting together, preſently return to ſuch water as they conſtituted before. And even ſpirit of wine, and other ſubtile and fugitive ſpirits, tho' they readily fly into the air, and mingle with it, do yet, in the glaſſes of chymiſts, eaſily reſume the form of liquors. And ſo volatile ſalts, tho' they will readily diſperſe themſelves in the air, and play up and down the capacity of a receiver; yet, after a while, faſten themſelves to the inſide thereof, in the form of ſalts.

And the experiment made in our engine, with a piece of match, ſeems to ſhew, that even thoſe light and ſubtile fumes, into which the fire itſelf ſhatters dry bodies, have no ſuch ſpring as that of the air; ſince they were unable to hinder the expanſion of the air, included in a bladder they ſurrounded. *Joſephus Acoſta*, indeed, tells us, that he ſaw, in the *Weſt-Indies*, ſome grates of iron ſo ruſted and conſumed by the air, that the metal crumbled between the fingers, was like parch'd ſtraw. *Varenius*, alſo, tells us, that in the iſlands call'd *Azores*, the Air is ſo ſharp, as, in a ſhort time, to fret not only iron-plates, but the very tiles upon the roofs of houſes, and reduce them to duſt. But it may be ſaid, that theſe authors aſcribe ſuch effects, chiefly, to the winds; and that the corroſion of the iron may proceed not from the air itſelf, or any of its genuine parts; but from ſome ſaline corpuſcles diſperſed thro' it, and driven, by the winds, againſt the bodies it is preſumed to fret.

But, to try whether water could be turn'd into air, we fill'd an æolipile therewith; and placing it upon kindled coals, when the heat forc'd out a vehement ſtream of aqueous vapours, we ty'd an empty bladder about the neck of it; and finding the æolipile, after a while, to blow up the bladder, we carefully ty'd it again, that the included ſubſtance might not get away. Then ſlipping it off from the æolipile, we convey'd it into our exhauſted receiver, and found, that the included ſubſtance expanded to a much greater bulk than before. And, having again taken out the bladder, we ſuffer'd it to remain ty'd up till the next morning, when it appear'd little leſs tumid: but, upon repeating the experiment, I found it very difficult to make it ſo accurately, as to ſhew, that water may be rarify'd into true air.

On the other hand, we found, by experience, that water, rarify'd into vapour, may, for a while, reſemble the elaſtic power of the air. For, if you fill a convenient æolipile with water, and lay it upon quick-coals, you may, after a while, obſerve ſo great a preſſure of ſome of the parts, contain'd in it, upon others; that the water will, ſometimes, be thrown up into the air, above three or four feet high. And, if you then take the æolipile, almoſt red-hot, from the fire, you may perceive, that the water will, for a conſiderable time, be ſpouted out in a violent ſtream. And, if there remains but little water in the æolipile, when 'tis thus taken from the fire; immerſing the neck of it into cold water, you will find, that, after it begins to draw ſome of it in, there will be generated, from time to time, many large bubbles in that water wherein the neck was plunged. Theſe bubbles ſeem manifeſtly to proceed from hence, that, for a while, the heat, in the æolipile, continues ſtrong enough to rarify part of the water that is ſuck'd in, and expel it, in the form of vapours, thro' that incumbent on the pipe. If, alſo, when the æolipile is almoſt full of water, you hold a fire-brand in that ſtream of vapours which iſſues out of the narrow mouth thereof, it will be very ſtrongly blown with a conſiderable noiſe. And it has been obſerved, that, by placing the brand almoſt at the mouth of the æolipile, the wind appear'd more vehement, than if it were held ſome inches from it.

The elaſtic power of this ſtream, indeed, ſeems manifeſtly owing to the heat that expands, and agitates the aqueous particles thereof; and ſuch rapid winds ſeem to be but water broke into little parts, and put in motion; ſince, by holding a ſolid, ſmooth, and cloſe body againſt it, the vapours condenſing thereon, will preſently cover that body with water.

But *Kircher* relates a remarkable experiment, which ſeems to ſhew, that water is convertible into air. He tells us, that he made an hydraulic organ, which was ſupplied with wind after the following manner. "There was built a little chamber A H, five feet high, and three broad, with two tranſverſe partitions C D, and E F, perforated like a ſieve; under theſe ran a pipe G, which carried the water that, by a ſtop-cock, was let out at H: the water, therefore, ruſhing in violently at G, excited a very great wind within; which bringing too much moiſture along with it, the partitions were contrived to purge it therefrom, that it might be convey'd more pure thro' the pipe A: but to render the air ſtill more pure, we made a ſpiral tube of lead Q R, and inſerted it into the veſſel S: by which means the air arrived at the organ, thro' the orifice Z, as dry as if it had come out of an oven." *Fig.* 35.

Now, if the wind that blows the organ here, doth not, upon the ceſſation of its unuſual agitation, gradually relapſe into water, I ſhould ſtrongly ſuſpect, that 'tis poſſible for water to be eaſily turn'd into air; for it can ſcarce ſeem probable, that ſo little air, as is commonly contain'd in water, ſhould be able, in ſo ſmall a quantity of water, as ſeems here employ'd, to make ſo violent a wind as our author ſpeaks of. I, therefore, ſuſpect that the wind, in this caſe, may be produced by ſmall particles of the water itſelf, forcibly expell'd out of the chamber into the organ. And tho' no heat intervenes, perhaps, motion alone, if vehement, may ſuffice to break water into very minute parts, and make them aſcend upwards, if they cannot, otherwiſe, more eaſily, continue their agitation. For, I remember, that betwixt *Lyons* and *Geneva*, where the *Rhone* is ſuddenly ſtraitned by two rocks, exceedingly near each other, that rapid ſtream, daſhing, with great impetuoſity, againſt them, breaks part of its water into ſuch minute corpuſcles, and gives it ſuch a motion, that a miſt, as it were, may be obſerv'd at a conſiderable diſtance, ariſing from the place, and aſcending high into the air. But, it ſeems odd, that aqueous vapours ſhould, like a dry wind, paſs thro' ſuch a long winding pipe of lead, as that deſcribed by our author; ſince we ſee, in the heads of ſtills, and in the necks of æolipiles, ſuch vapours are preſently, even by a very little cold, condenſed into water.

We took a clear glaſs bubble, capable of containing three ounces of water, with a long and wide cylindrical neck; this we fill'd with oil of vitriol, and fair water, of each almoſt a like quantity; and caſting in ſix ſmall iron nails, we ſtopt the mouth of the glaſs, which was now full of liquor, with a piece of diapalma, and ſpeedily inverting the bubble, we put the neck of it into a ſmall wide-mouth'd glaſs, with more of the ſame liquor in it; and as ſoon as the neck had reach'd the bottom of the liquor, there appear'd, at the upper-part of the vial, a bubble, about the bigneſs of a pea, which ſeem'd rather to conſiſt of new ſmall bubbles, produc'd by the action of the diſſolving liquor upon the iron, than any parcel of the external air, that might be ſuſpected to have got in upon the inverſion of the glaſs; eſpecially ſince we allow'd time to thoſe little particles of air, which were carried down with the nails, to fly up again; and, ſoon after, we perceiv'd the bubbles, produc'd by the action of the menſtruum upon the metal, aſcending in ſwarms to the former; and breaking into it, they ſoon exceedingly increas'd it, and, by degrees, depreſs'd the water, till, at length, the ſubſtance contain'd in theſe bubbles, poſſeſs'd the whole cavity of the vial, and moſt of its neck too; reaching much lower therein, than the ſurface of the ambient liquor, wherewith the open-mouth'd glaſs was, by this means, almoſt repleniſh'd. We ſuffer'd both the vial, and the open-mouth'd glaſs, to remain as they were, in a window, for three or four days and nights together; but often looking upon them, during that time, as well as at the expiration of it, the whole cavity of the glaſs bubble, and moſt of its neck, ſeem'd to be poſſeſs'd by air; ſince, by its ſpring, it was able, for ſo long, to hinder the expell'd liquor that ſurrounded it, from regaining its former place. And juſt before we took the vial out of the other glaſs, upon the application of a warm hand to the convex part of the bubble, the impriſon'd ſubſtance readily dilated itſelf, like air, and broke thro' the liquor in ſeveral ſucceeding bubbles.

Having alſo, at another time, made the like experiment, with a ſmall vial, and nails diſſolv'd in *Aqua fortis*, we found it ſucceeded as the foregoing. And here we obſerv'd, that the ſteams newly generated, did not only poſſeſs almoſt the whole cavity of the glaſs, but ſeveral times, of themſelves, broke away in large bubbles, thro' the ambient liquor into the open air; whence theſe experiments ſeem'd, manifeſtly, to prove, that, in general, air may be generated *de novo*.

And if, according to the mechanical hypotheſis, the difference of bodies proceeds but from the various magnitudes, figures, motions, and textures of the ſmall parts they conſiſt of; there appears no reaſon why the minute parts of water, and other bodies, may not be ſo agitated, or connected, as to deſerve the name of air.

24. We choſe a glaſs-egg, half an inch in diameter at the top, and an inch at the bottom; and filling it with common water, to the height of about a foot and a half, ſo that the upper part remain'd empty, we encloſed it in the receiver; and, upon pumping, obſerved bubbles at the bottom and ſides of the glaſs; and, increaſing as the air was drawn away, they, from time to time, plentifully aſcended to the top of the water, where they quickly broke; but the wideneſs of the glaſs allowing them free

paſſage thro' the water, they did not, as in the former experiments, ſeem to make it ſwell; and upon the return of the external air, the water appear'd to have loſt of its firſt extent, by the avolation of the air interſperſed.

We put about two ounces of rain-water, carefully diſtilled, into a round glaſs-bubble with a very ſmall neck, which was thereby fill'd half way to the top, and then conveyed it into the receiver; and, tho' we drew out more air than ordinary, there appear'd not the leaſt intumeſcence of the water, nor any aſcending bubbles. But ſuſpecting that either the ſmall quantity of the water, or the figure of the veſſel, might affect the experiment, we took the former glaſs egg, and another, not much different from it, and fill'd the firſt, with diſtill'd rain-water, to the old mark, and into the latter, put a long cylinder of ſolid glaſs, to ſtraiten the cavity of the neck; and then pouring ſome diſtill'd water into that alſo, till it reached near the top, they were both let down into the receiver: but here the air was ſo far exhauſted, before there appear'd any bubble in either of the glaſſes, that the difference betwixt this and common water, was very manifeſt. But, at length, when the air was almoſt quite drawn out, the bubbles began to diſcloſe themſelves, and to increaſe, as the preſſure of the air, in the receiver, decreaſed. But, in the firſt egg, the bubbles were very ſmall, and never able to ſwell the water above the mark; in the other, whoſe neck was ſtraitned, great numbers of large ones faſten'd themſelves to the lower-end of the ſolid piece of glaſs, and gather'd to ſuch a degree, between it and the ſides of the neck, that the water ſwell'd a finger's breadth above the mark, tho', upon admitting the external air, it relaps'd to the former mark, or rather fell below it: upon which, all the bubbles preſently diſappear'd in the former veſſel; whilſt ſeveral remain'd faſten'd to the lower-part of the glaſs cylinder, and continued there for above an hour after, but contracted in their dimenſions.

And having ſuffer'd theſe glaſſes to remain above twenty-four hours in the receiver, we, afterwards, repeated the experiment; but tho' the receiver was carefully exhauſted, yet we ſcarce ſaw a bubble in either of the glaſſes; yet the water roſe the breadth of a barley-corn in the neck of that glaſs wherein the ſolid cylinder had been placed; the liquor, in the other, not being ſenſibly ſwelled. And, laſtly, upon letting in the air, the water in the ſtraitned neck, ſoon ſubſided to the mark, above which it had ſtretched.

The air contain'd in oil.

25. We took a glaſs egg, with a long neck, of about $\frac{1}{3}$ inch in diameter, and pouring in ſallad-oil till it reach'd above half-way to the top, we incloſed it in the receiver, together with ſome common water in a ſimilar veſſel. The pump being ſet on work, there began to appear bubbles in the oil, much ſooner than in the water; and afterwards they, alſo, aſcended more plentifully in the former, than in the latter; and when the receiver was well exhauſted, the bubbles aroſe almoſt as numerous as ever: ſo that none of the various liquors, we have try'd, ſeem'd to abound more with aerial particles, than this oil. And here 'twas remarkable, that between the time

it was put into the receiver, and that before we could work the pump, it ſubſided about half an inch below the mark it at firſt reach'd to.

Oil of turpentine. Common oil of turpentine, being put into a ſmall glaſs bubble with a ſlender neck, ſo as to fill it about two inches from the top, preſented us, upon evacuating the receiver, with numerous bubbles; moſt of which, riſing from the bottom, expanded themſelves exceedingly in their aſcent, and made the liquor, in the neck, to ſwell ſo much by degrees, that at length, it ſeveral times ran over at the top: whereby we were hindered from diſcerning, upon letting in the air, how much the ſinking of the oil, below the firſt mark, was due to the receſs of the bubbles.

Oil of tartar. Having filled a glaſs egg with a very ſtrong ſolution of ſalt of tartar in fair water; tho' this, except quick-ſilver, is reckon'd the heavieſt of liquors; we try'd, whether it would afford any bubbles; and putting it into the receiver, along with other liquors, we found that they yielded many bubbles, long before any appeared in that: and upon proſecuting the experiment, it ſeemed, of all the liquors whereof we made trial, this afforded the feweſt and ſmalleſt bubbles.

Spirit of vinegar, red wine, and milk. Spirit of vinegar, examined after the ſame manner, exhibited a moderate quantity of bubbles. In red wine, we found nothing very remarkable: for tho' upon the exſuction of the air, the bubbles aſcended in it, as it were in ſhoals, and ſhifted places, among themſelves, in their aſcent; yet the intumeſcence of the whole bulk of the liquor was ſcarce ſenſible; the bubbles moſt commonly breaking very ſoon after their arrival at the top; where during their ſtay, they compoſed a kind of ſhallow froth, which, alone, appear'd higher, in the neck of the glaſs, than the wine, when it was firſt let down. Milk conveyed into our receiver, preſented us with nothing conſiderable, except that the bubbles, not eaſily breaking at the top, and thruſting up one another, made the intumeſcence appear much greater than that of common water.

Eggs. We likewiſe convey'd hens eggs into the receiver, but after the exſuction of the air, took them out whole again.

Spirit of urine and of wine. We put ſome ſpirit of urine into a glaſs egg, fill'd another glaſs to about two thirds of its neck, with rectified ſpirit of wine, and a third with common water, till it reach'd to the middle of the neck, and then poured to it of the ſame ſpirit of wine, till it reach'd about an inch higher. Theſe glaſſes, having marks ſet on them, over againſt the tops of the contain'd liquors, were put into the receiver, and that beginning to be evacuated, bubbles began to appear in all three. The mixture of ſpirit of wine and water, diſcloſed numerous bubbles, eſpecially towards the top, and the ſpirit of urine appear'd to ſwell near an inch and a half above the mark, and yielded plenty of bubbles, which made a kind of froth at the upper part of it; and above that, there appear'd eight or ten great bubbles, one higher than another, each of them conſtituting, as it were, a cylinder of about half an inch high, and as broad as the internal cavity of the neck; ſo that all the upper part of the neck ſeemed to be divided into equal parts, by tranſverſe partitions, conſiſting of the coats of the bubbles, whoſe edges appear'd like ſo many rings, ſuſpended one above another.

In the ſpirit of wine, there aroſe a great multitude of bubbles, all the while the experiment was in hand, which aſcended with a great velocity, and being arrived at the top, made no ſtay there; yet, notwithſtanding the great fluidity and volatility of the liquor, before they broke they lifted up the upper ſurface of it, and for a moment or two, formed thereof, a thin film, which appear'd protuberant, above the reſt of the ſuperficies, like a ſmall hemiſphere: theſe alſo aſcended in ſtrait lines, whilſt thoſe produced at the lower part of the veſſel, containing the mixture of the water and ſpirit of wine, aſcended with a wavering motion, deſcribing an indented line. Laſtly, it was obſervable in the ſpirit of wine, as alſo in the oil of turpentine, lately mention'd, that not only the bubbles ſeem'd to riſe from determinate places, at the bottom of the glaſs; but that, in their aſcent, they kept an almoſt equal diſtance from each other, and ſucceeded in a certain order, whence they ſeem'd part of ſmall bracelets, conſiſting of equally ſmall ſeparate beads; the lower end of each bracelet being, as it were, faſtened to a point, at the bottom of the glaſs.

The air being ſparingly let into a receiver, the great bubbles incumbent upon one another, in the glaſs that contain'd the ſpirit of urine, were by regular degrees leſſen'd, till at length they wholly ſubſided. Notwithſtanding the receſs of ſo many bubbles as broke on the top of the ſpirit of urine, during all the time of the experiment, yet it ſcarcely appear'd at all ſunk below the mark. Nor did the mixture of ſpirit of wine and water conſiderably ſubſide. But the ſpirit of wine, not only viſibly expanded itſelf in the neck of the veſſel, that contain'd it, whilſt the bubbles broke at the top of it, almoſt as ſoon as they arrived there; but upon the re-admiſſion of the external air, it retain'd its new expanſion. And, tho' we let it alone, for near an hour together, yet when we took it out, it ſtill ſwell'd between a quarter and half an inch above the mark. Repeating the experiment with freſh ſpirit of wine, it ſwell'd in the neck as formerly; and leaving it all night in the receiver, and allowing free acceſs to the external air at the ſtop-cock, I found it, the next day, ſtill expanded, as before; only it ſeem'd a little lower; which decreaſe, perhaps, proceeded from the avolation of ſome of the fugitive parts of the liquor. And for farther ſatiſfaction, having taken out the glaſs, and conſider'd it in the open air at a window; I could not find, that there was any remaining bubble to occaſion the continuance of this ſtrange expanſion.

The gravity of air expanded under water.

26. We took two very ſmall vials, of the ſize and ſhape expreſſed in *Fig.* 36. and into one of them, put ſo much of a certain ponderous mercurial mixture, that, the mouth being ſtopp'd with a little ſoft wax, the glaſs would but juſt ſink in water: this we let fall to the bottom of a wide-mouth'd cryſtal jar, fill'd with about half a pint of common water; and into the ſame veſſel, we ſunk the other glaſs, unſtopp'd, with as much water in it as was more than ſufficient to make it ſubſide. Both theſe ſunk with their mouths downwards; the former being about three quarters full of air,

and

and the latter containing in it a bubble of air as big as half a pea; then the wide-mouth'd glaſs was let down into the receiver, and the engine being work'd, the bubbles began to appear in the water, as in the former experiments; but continuing long to ply the pump, that little glaſs, whoſe mouth was open'd, came to the top of the water; being, as it were, buoyed up by a great number of bubbles, that had faſten'd themſelves to the ſides of it; and ſwimming thus, with the mouth downward, we could eaſily perceive, that the internal air above-mention'd, had much dilated itſelf, and thereby ſeem'd to have contributed to the emerging of the glaſs, which remain'd floating, notwithſtanding the breaking, and vaniſhing of moſt of the contiguous bubbles. And perſiſting in pumping, we obſerv'd, that at each time the key was turn'd, the air, in the little glaſs, manifeſtly expanded itſelf, and thruſt out the water; generally retaining a very protuberant ſurface, where it was contiguous to the remaining water. And when, after ſeveral exſuctions of the air in the receiver, that in the vial ſo dilated itſelf, as to expel almoſt all the water, it turn'd up its mouth towards the ſurface of the water in the jar, and there deliver'd a large bubble, and then relaps'd into its former floating poſture.

This experiment taught us, that it was a work of more time and labour, than we imagin'd, to exhauſt our receiver as much as it may be exhauſted; for tho' before the ſmall vial emerged, we thought the receiver conſiderably emptied, becauſe there ſeem'd to come but very little air at each exſuction, out of the cylinder; yet, afterwards, the air included in the vial, manifeſtly dilated itſelf upon each ſtroke, ſo long, that for nine times it turn'd its mouth upwards, and diſcharg'd a bubble about the bigneſs of a pea. But that vial which had the weight in it, roſe not at all: then leiſurely letting in the air, that within the vial ſhrinking into a very narrow compaſs, the glaſs fell down to the bottom of the jar.

But being deſirous to try once more, whether the little glaſs with the weight in it, might not alſo be rais'd; after we had ſuffer'd the engine to remain clos'd, as it was, for five or ſix hours, the pump was again ply'd ſo vigorouſly, that not only about the upper-part of the jar, there appear'd a large number of ſmall bubbles; but afterwards, there came from the bottom of the jar, ſome as large as ſmall peas, which, the pump being ſtill kept going, follow'd one another, to the number of forty, coming from the ſtopp'd vial; whoſe mouth, it ſeems, had not been ſhut ſo cloſely, but the included air found a paſſage betwixt the wax and the glaſs. After this, the unſtopp'd glaſs began to float again; the air ſhut up in it, being ſo dilated as to expel a large part of the water, but not ſo much as to break quite thro'. And, at length, the heavier of the two vials began to riſe, but immediately ſubſided again: which ſeem'd owing to the air within it, whoſe bulk and ſpring being weakned by the receſs of the forty bubbles, it was no longer able to break thro' the incumbent water; but forming a bubble, at the mouth of the glaſs, buoy'd it up towards the top, and there getting away, left it to ſink again; till the preſſure of the air in the receiver, being farther taken off, the air, in the vial, was permitted to expand itſelf farther, and create another bubble, by which it was again, for a while, carry'd up. And though, after having empty'd the receiver as far as we well could, we ceaſed from pumping; yet the veſſel, continuing more ſtanch than uſual, this aſcent and fall of the vial were repeated to the ninth time; the included air, by reaſon of the ſmallneſs of the vent at which it muſt paſs out, being not able to get away, otherwiſe than by ſmall degrees, and, conſequently, in ſeveral ſuch parcels as were able to conſtitute bubbles, each of them big enough to raiſe the vial, and keep it ſuſpended till the bubble flew off. Hence it may appear, that a body lighter than an equal bulk of water, will float in that fluid, when the preſſure of the atmoſphere is, in very great meaſure, taken off from the liquor and the body: tho' it were worth inquiring what it is that ſo plentifully concurs to fill the bubbles made in our experiment, by the air ſo much expanded.

In this experiment, as in the former, the external air being let in, ſoon precipitated the floating veſſel. And the water which, in the heavier vial, ſucceeded in the room of thoſe forty, or more, great bubbles of air, which, at ſeveral times, got out of it; was of a very inconſiderable bulk.

A pendulum made to ſwing in vacuo.

27. It having been obſerv'd, that pendulums vibrate more ſlowly, and that their motion ſooner ceaſes in a thicker, than in a thinner medium; we thought proper to try if a pendulum would move faſter, or vibrate longer, in our exhauſted receiver, than out of it. We therefore took two round poliſhed ſteel-pendulums, of equal bigneſs, each of them weighing twenty drams, bating ſo many grains. One of theſe we ſuſpended in the cavity of the receiver, by a very ſlender ſtring, about ſeven inches and a half in length, from the cover of the receiver whereto it was faſten'd. Then we made the pendulum ſwing, and, counting the returns of the other that hung in the open air, by a ſtring about the ſame length, we ſhorten'd and lengthen'd this, till it appear'd to keep the ſame pace with that in the receiver. Then, having carefully drawn away the air, we again made the pendulum in the receiver vibrate; and, giving to the other ſuch a motion, as caus'd it to deſcribe an arch, apparently equal to that of the included pendulum, we counted the recurſions of both; and we reckon'd two and twenty vibrations of the included pendulum, whilſt but twenty were obſerv'd of the other. And at another time, alſo, the former was found to have made twenty-one returns, whilſt the other made but twenty. Yet this experiment ſeem'd to teach us little, except that the difference betwixt the motion of ſuch a pendulum, in common air, and in a medium exceedingly rarify'd, is ſcarce ſenſible in veſſels no bigger than our receiver; eſpecially, ſince we could not ſuppoſe that to be altogether free from air. We obſerv'd, alſo, that when the receiver was full of air, the included pendulum continu'd its recurſions about fifteen minutes, before it left off ſwinging; and that, after the exſuction of the air, the vibration of the ſame pendulum appear'd not to laſt ſenſibly longer.

PNEUMATICS

A watch and a bell in the exhauſted receiver.

28. That the air is the medium whereby ſounds are convey'd to the ear, was a current opinion, till ſome pretended, that if a bell, with a ſteel clapper, be faſten'd to the inſide of a tube, upon making the experiment *de vacuo*, with it, the bell remaining ſuſpended in the deſerted ſpace, at the upper end of the tube; if a vigorous load-ſtone be apply'd on the outſide of the glaſs, it will attract the clapper; which, upon the removal of the load-ſtone, falling back, will ſtrike againſt the bell, and thereby produce a vey audible ſound: whence, ſeveral have concluded, not the air, but ſome more ſubtile body, to be the medium of ſounds. But ſuſpending a watch, freed from its caſe, in the cavity of our receiver, by a packthread; and then, cloſing up the veſſel with melted plaiſter; we liſten'd near the ſides of it, and plainly heard the balance beat, and obſerv'd, that the noiſe ſeem'd to come directly in a ſtreight line, from the watch to the ear. We found, alſo, a manifeſt difference in the noiſe, by holding our ears near the ſides of the receiver, and near the cover of it; which ſeem'd to proceed from the difference between the glaſs, the cover, and the cement, thro' which the ſound was propagated. But, upon working the pump, the ſound grew gradually fainter; ſo that, when the receiver was emptied as much as uſual, we could not, by applying our ears to the very ſides of it, hear any noiſe from within; tho' we could eaſily perceive, that, by the motion of the hand which mark'd the ſeconds, and by that of the balance, the watch neither ſtood ſtill, nor ſeem'd irregular. And, to ſatisfy ourſelves farther, that it was the abſence of the air about the watch, that hinder'd us from hearing it, we let in the external air at the ſtop-cock; and then, tho' we turn'd the key, and ſtopp'd the valve, yet we could plainly hear the noiſe made by the balance; tho' we held our ears, ſometimes, at the diſtance of two feet from the outſide of the receiver. And this experiment, being repeated, ſucceeded after the like manner: which ſeems to prove, that the air is, at leaſt, the principal medium of ſounds. And, by the way, it is very well worth noting, that, in a veſſel ſo exactly cloſed as our receiver, ſo weak a pulſation as that of the balance of a watch, ſhould propagate a motion to the ear, in a ſtreight line, notwithſtanding the interpoſition of glaſs, ſo thick as that of our receiver. We afterwards took a bell of about two inches in diameter at the bottom, which was ſupported in the midſt of the cavity of the receiver by a bent ſtick, preſſing with its two ends againſt the oppoſite parts of the inſide of the veſſel; which, being cloſed up, we obſerved the bell to ſound more dead than in the open air. And yet, when we had emptied the receiver, we could not diſcern any conſiderable change in the loudneſs of the ſound: whereby it ſeem'd, that tho' the air be the principal medium of ſound; yet either a more ſubtile matter may be, alſo, a medium of it; or elſe that an ambient body, that contains but few particles of air, is ſufficient for that purpoſe. Whence, perhaps, in the above-mention'd experiment, made with the bell and the load-ſtone, there might, in the deſerted part of the tube, remain air enough to produce a ſound.

But as in making the experiment of firing gun-powder with a piſtol in our evacuated receiver, the noiſe made by the flint, ſtriking againſt the ſteel, was exceeding languid, in compariſon of what it would have been in the open air: ſo, on ſeveral other occaſions, it appear'd, that the ſounds produced there, if they were not loſt, ſeem'd to arrive at the ear very much weakned.

A fuming liquor in the receiver.

29. We have a liquor which, tho' moſt of its ingredients be metals, and all of them ponderous, is yet of ſuch a nature, that, whilſt the vial wherein it is kept, remains ſtopp'd, appears tranſparent, as alſo the upper part of the glaſs, to which the liquor reacheth not; but as ſoon as ever the ſtopple is taken out, and full acceſs given to the external air; both the under part of the cork, and the liquor itſelf, preſently ſend upwards, and diffuſe a fume, as thick and white, as if a quantity of alabaſter-duſt were thrown up into the air. And this ſmoking of the liquor laſts, till the vial be ſtopp'd again; and then the aſcent of the fumes ſuddenly ceaſes.

To a vial of this fuming liquor, we faſten'd a weight of lead; and, having ty'd to the ſtopple one end of a ſtring, whilſt the other was made faſt to the cover of the receiver, the liquor was carefully cloſed up; and the air being diligently pump'd out, we unſtopp'd the vial. And tho' immediately upon drawing out the cork, there appear'd ſome white fumes, which ſeem'd to proceed from the air being impriſon'd in the vial, and diffuſing itſelf ſuddenly into the receiver; yet we afterwards obſerv'd, that the fumes did not mount and diſperſe themſelves, as they uſed to do in the open air; but, aſcending to the lip of the vial, they ſtopp'd there, and ran down along the outſide, and thence along an inclining piece of lead, on which the vial reſted, like a little ſtream, that quitted not the vial, till it was come to the bottom of it, and there forſook it, like a ſtream of water of the ſame bigneſs. Then letting in ſome of the external air, the ſtream run a-freſh, tho' not altogether ſo large: and after the receiver was fill'd with air, I found, to my ſurprize, that tho' the ſtream diſappear'd, yet no white fumes aroſe, either from the cork or out of the vial; no, not when the cover was removed from the receiver: tho' after a while there aſcended white fumes from the receiver. But having immediately taken out the vial into the open air, it emitted white exhalations as before; and having preſently after unſtopp'd it in an open window, we found both it and the cork immediately yielded a much more plentiful ſmoke; tho' it were now ſeveral years ſince this parcel of liquor was prepared.

Smoke in vacuo.

30. Into one of our ſmall receivers, we convey'd a piece of well-lighted match; and letting it remain there till it had fill'd the receiver with ſmoke, we took it out, and immediately cloſed the receiver again, that the ſmoke might not get away. Then ſtaying to let theſe fumes leiſurely ſubſide, we found, that after ſome time, they ſettled themſelves in the lower half of the receiver, in a darkiſh body; leaving the upper half tranſparent, and, as to ſight, full only of clear air. And inclining the veſſel that contain'd this ſmoke, ſometimes to one ſide and ſometimes to the other; we obſerv'd the fume to keep its ſurface almoſt horizontal, as water,

ter, or any other liquor, would have done in the like caſe. And if, by a quicker rocking of the engine, the ſmoke were more ſwiftly ſhaken, it would, like water, either vibrate from one ſide to the other of the glaſs, or elſe have its ſurface manifeſtly curl'd, like waves, and preſerve itſelf in an entire and diſtinct body from the incumbent air; and, being permitted to reſt a while, would ſoon recover its former ſmooth and level ſuperficies. If, alſo, the key were turn'd, and the valve unſtopp'd, ſo that there was a free paſſage open'd betwixt the external air and the cavity of the receiver; then would ſome of this ſmoke fall down, as it were, in a ſtream, into the ſubjacent cylinder; and a proportionate quantity of the outward air, would manifeſtly aſcend thro' it into the incumbent air; after the ſame manner, as when a vial, with a long neck, fill'd with red wine, being inverted into a glaſs of fair water, the water and wine, by degrees, mix, as it were, in little curl'd ſtreams with each other; the one falling down, and the other aſcending in its place. And if, when the ſuperficies of our ſmoke lay ſmooth and horizontal, a hot iron were held near the outſide of the receiver; the adjacent part of the included fumes, being rarify'd by the heat, would readily aſcend in a large pillar of ſmoke, to the very top of the receiver; yet, without ſeeming to loſe its diſtinct ſurface, or to be confounded with the air, below which, upon the receſs of the adventitious heat, it would again ſubſide.

Since then there is ſo vaſt an inequality in the denſity and weight of liquors; we may conſider the atmoſphere as a peculiar kind of thin fluid, much lighter than ſpirit of wine. And as waves appear'd upon the ſurface of our agitated ſmoke; ſome ſuch thing may, poſſibly, happen on the ſuperficies of the atmoſphere: as may be conjectur'd from thoſe ſtrange inequalities that often appear, eſpecially when the air abounds with exhalations and vapours, upon the limb of the ſun in its riſing and ſetting. And if this phenomenon be owing to the refraction, which the ſun's rays ſuffer in our air; 'tis eaſy to ſuppoſe the ſurface of the atmoſphere to be often, as we ſaid, exceedingly curl'd or wav'd. And certainly it is ſurprizing to ſee how, thro' a good teleſcope, there will not only appear inequalities in the edge of the ſun, which often ſeems to be indented, but thoſe inequalities vaniſh in one place, and preſently appear in another, and ſeem perfectly to move, like waves ſucceeding and deſtroying one another: only their motion frequently ſeems to be quickeſt; as if, in that vaſt ſea, they were carry'd on by a current or a tide. And this alſo appears to the eye, when a large, and well defined image of the ſun, is, by the teleſcope, caſt upon white paper.

The coheſion of poliſh'd marbles in vacuo.

31. It hath been thought ſtrange, that the perfectly poliſh'd ſurfaces of two flat pieces of marble being apply'd to each other, they ſhould ſtick ſo faſt together, that the lower may be raiſed by taking hold of the upper. But, as this ſeems owing to the unequal preſſure of the air upon the undermoſt ſtone, the lower ſuperficies of that being freely expoſed thereto, and preſs'd upon by it, whilſt the upper ſurface is defended therefrom; which, conſequently, preſſing the lower ſtone againſt the upper, hinders

it from falling; we therefore conjectur'd, that two marbles, being exactly ground to one another, and together ſuſpended in our evacuated receiver, the lower ſtone would fall from the upper: but we could not procure marbles to be ground ſo true, as to ſuſtain one another in the air for above a minute or two, which is a much ſhorter time, than is requir'd to empty our receiver. We did, indeed, try to make our marbles ſtick cloſe together, by moiſtning their ſurfaces with rectified ſpirit of wine; but to little purpoſe; for having convey'd into the receiver two black ſquare marbles, the one with its ſide two inches and a third, and ſomewhat more than half an inch in thickneſs, the other of equal ſurface, but not above half ſo thick, faſtned together by means of that ſpirit; and having ſuſpended the thicker by a ſtring from the cover, we found not, that the exſuction of the air would ſeparate them, tho' a weight of four ounces were faſten'd to the lower, to facilitate its falling.

I would gladly have the experiment try'd with marbles, ſo well poliſh'd, as to need no liquor to make them cohere, and in a veſſel, out of which, the air may be more perfectly drawn, than it was out of ours. But, tho' we will not determine, whether the ſpirit of wine contributed to the ſtrong coheſion of theſe ſtones, otherwiſe than by keeping the ſubtileſt parts of the air from getting in between them; yet it ſeem'd, that the reaſon, why the lower marble fell not, was, probably, becauſe the preſſure of the air remaining in the receiver; which, as we formerly noted, being able to ſuſtain a cylinder of water, of above a foot in height, may be ſuppoſed capable of keeping ſo broad a marble from deſcending. And, tho' this may ſeem a ſtrange proof of the ſtrength of the ſpring of the air, even when rarify'd; yet it will ſcarce appear incredible to him, who hath obſerv'd, how exceeding ſtrong a coheſion may be made, betwixt broad bodies, only by immediate contact. A notable inſtance of this, is given us by the learned *Zucchius*, who tells us, that "a young fellow, bragging "of his ſtrength, ſome body ſet him to pull at the ring in the middle of a "braſs-plate, that lay upon a poliſh'd marble, whereto, it was exactly "ground: this he thought a trivial matter; but after his utmoſt endeavour, "found it impoſſible to ſeparate them by direct pulling; which made him "imagine they were faſtned together, by means of ſome vehement ſtrong "glew, till he ſaw the plate, afterwards lifted by another, who, firſt ſlipt it "along the marble."

An erect preſſure exercised by the atmoſphere.
Fig. 37.

33. Our receiver being exquiſitely clos'd, and the air in a good meaſure drawn out, we remov'd it from the pump, and to the lower branch of the ſtop-cock, ſpeedily apply'd a tapering valve of braſs, made fit to go with its narrower end into the cavity of the branch, and to fill the orifice of that cavity with its broader part. And, that the air might not get in at the little intervals, between the convex ſurface of the ſtopple, and the internal edge of the branch, they were ſtopp'd with diachylon. And, to the door of the valve, there was, at a button of braſs, faſten'd a broad ſcale, wherein weights were to be put. This done, the key of the ſtop-cock was turn'd, and the external air beating like a forcible ſtream upon the valve to get in

there, it ſuddenly ſhut the valve, and kept it ſo cloſe, that we had time to caſt in ſeveral weights, one after another, into the ſcale, till at length, the weight overpowering the preſſure of the atmoſphere, drew down the valve by the ſtrings that ty'd the ſcale to it, and gave liberty to the outward air to ruſh into the receiver. Tho', another time, when the valve had but little weight hanging to it, being, by accident, drawn down beneath its former place, it was, by the impetuous current of the external air, ſuddenly impell'd up into it again, and kept there. But, in the former experiment, tho' the receiver were not well exhauſted; tho' it leak'd whilſt the reſt of the experiment was in hand; and tho' the valve, whereon the cylinder of the atmoſphere could preſs, were not above an inch and a half in diameter; yet the whole weight, ſupported by the air, amounted to about ten pounds, of ſixteen ounces each: ſo that, had the experiment been made with favourable circumſtances, the air endeavouring to preſs in, at the orifice of the ſtop cock, would very probably have kept a much greater weight from falling out of it.

The preſſure of the atmoſphere computed.

33. But our pump, alone, may afford us a nobler inſtance of the force of the air; ſo that, by means of this part of our engine, we may conjecture at the ſtrength of the atmoſphere, computed as a weight. For, firſt, the ſucker, brought to move eaſily up and down the cylinder, being impell'd to the top of it, and the receiver taken off from the pump, that the upper orifice of the cylinder remaining open, the air may freely ſucceed the ſucker, and therefore readily yield to its motion downwards; and there being faſten'd to one of the iron teeth of the ſucker, ſuch a weight, as may juſt ſuffice to draw it to the bottom of the cylinder; we may hence find the weight neceſſary to draw down the ſucker: and when the atmoſphere makes the ordinary reſiſtance againſt its deſcent, the ſucker being again forc'd to the top of the cylinder, whoſe upper orifice muſt now be exactly clos'd; we may eaſily, by hanging a ſcale to the above-mention'd iron, that makes part of the ſucker, caſt in known weights, till the ſucker be drawn down; then, to theſe weights in the ſcale, that of the ſcale itſelf being added, the ſum will give us the weight of a column of air, equal in diameter to the ſucker, or to the cavity of the cylinder, and, in length, to the height of the atmoſphere.

According to this method, we attempted to meaſure the preſſure of the atmoſphere, but found it more difficult than we expected, to perform it accurately; for tho', by the help of the handle, the ſucker mov'd up and down with great facility; yet, when it came to be mov'd by a dead weight, we found, that the little inequalities, and, perhaps, the unequal preſſure of the leather againſt the cavity of the cylinder, now and then ſtopp'd the deſcent or aſcent of the ſucker; tho' a very little external help would eaſily ſurmount that impediment. We found then, that a weight of twenty-eight pounds, being faſten'd to one of the teeth of the ſucker, drew it down cloſe, when the upper orifice of the cylinder was left open; but, by the help of oil and water, and the frequent working of the ſucker with the handle, its motion in the cylinder had been before purpoſely facilitated.

cilitated. Then the upper orifice of the cylinder was very carefully ſtopp'd; the valve heing likewiſe ſhut, with its ſtopple well oiled, after the ſucker had been again impell'd up to the top of the cylinder. To the former weight we now added a hundred and twelve pounds, which forcing down the ſucker, though but leiſurely, we took off the 28 pound weight, and hung on, inſtead of it, fourteen pound; but found that, with the reſt, unable to carry down the ſucker. And to ſatisfy ourſelves it was the reſiſtance of the ambient air, that hinder'd the deſcent of ſo great a weight; after we had try'd, that upon unſtopping the valve, and thereby opening an acceſs to the external air, the ſucker would be immediately drawn down, having forcibly depreſs'd the ſucker, to the bottom of the cylinder, and then faſten'd weights to the iron, the preſſure of the external air, finding little reſiſtance, in the cavity of the cylinder, preſently began to impel the ſucker, with the weights that clogg'd it, towards the upper part of the cylinder, till ſome ſuch accidental impediment, as we formerly mention'd, check'd its courſe; and when that was remov'd, it would continue its aſcent to the top. And tho', poſſibly, there might remain ſome particles of air in the cylinder, after the ſucker was drawn down; yet the preſſure of a cylinder of the atmoſphere, ſomewhat leſs than three inches in diameter, uncompreſs'd, not only ſuſtain'd, but drove up a weight of a hundred and odd pounds: for, beſides the weight of the whole ſucker itſelf, which amounts to ſome pounds, the weights annex'd to it, made up a hundred and five pounds; yet all this falls ſhort of the weight juſt ſaid to be ſuſpended, by the reſiſtance of the air, in the cavity of the cylinder. This experiment was made in the winter, the weather neither froſty nor rainy, about the change of the moon; and at a place whoſe latitude is about 51 degrees and a half: for, perhaps, the force or preſſure of the air may vary according to the ſeaſons of the year, the temperature of the weather, the elevation of the pole, or the phaſes of the moon; any of them ſeeming able to alter either the height, or conſiſtence of the atmoſphere. And therefore, it would not be amiſs, if this experiment were try'd carefully, at ſeveral times and places, with variety of circumſtances. It might alſo be try'd with cylinders of ſeveral diameters, exquiſitely fitted with ſuckers; that we might know what proportion ſeveral pillars of the atmoſphere, bear to the weight they are able to ſuſtain, or lift up; and conſequently, whether the increaſe, or decreaſe of the reſiſtance of the ambient air, can be reduced to any regular proportion, to the diameter of the ſuckers. Theſe, and other experiments, which may be made with this cylinder, might, moſt of them, be more exactly try'd by the *Torricellian* tube; if glaſs could be blown, and drawn perfectly cylindrical.

The nature of ſuction.

Here we may obſerve, that as many other phenomena of our engine, ſo eſpecially the two laſt experiments, ſeem to ſhew the nature or cauſe of ſuction. It's true, indeed, in ſucking, we commonly uſe ſome manifeſt endeavour, by a peculiar motion of our mouths, cheſts, &c. yet it appears not how the upper-part of the emptied cylinder, that remains at reſt all the while, or any part of it, endeavours to draw the depreſs'd

Pneumatics ſucker, and the annex'd weights to it; tho' ſuch as behold the aſcent of the ſucker, without conſidering the cauſe of it, readily conclude it to be rais'd by ſome ſecret thing, that powerfully ſucks or attracts it. Whence it ſeems not abſolutely neceſſary to ſuction, that there be in the body, which is ſaid to ſuck, an endeavour, or motion in order thereto; but rather that ſuction may be reduced to truſion, and its effects aſcribed to a preſſure of the neighbouring air, upon the bodies contiguous to that which is ſaid to attract them. To object here, that ſome particles of air, remaining in the emptied cylinder, attracted this weight, to obviate a vacuum, is to no purpoſe; unleſs it can clearly be made out, by what grappling inſtruments the external air could take hold of the ſucker; how ſo little of it obtain'd the force to raiſe ſo great a weight; and why, upon letting a little more air into one of our evacuated veſſels, the attraction is much weakned. For that ſtill there remain'd in the exhauſted cylinder many little empty ſpaces, may appear by the great violence wherewith the air ruſheth in, if it be permitted to enter. In the next place, theſe experiments may teach us, what to judge of the vulgar axiom, That nature utterly abhors a vaccuum; ſo that no human power is able to make one *And vacuity.* in the univerſe. For, if by a vacuum we underſtand a place perfectly free from all corporeal ſubſtance, it may be plauſibly maintain'd, that there is no ſuch thing in the world. But the generality of the pleniſts take not the word in ſo ſtrict a ſenſe. For when they alledge, that by ſucking water thro' a long pipe, the liquor, contrary to its nature, aſcends into the mouth, only to fill up that ſpace, made by the dilatation of the breaſt and lungs, which would otherwiſe, in part, be empty; and when they tell us, that the reaſon why in a gardener's watering-pot, conically ſhaped, and filled with water, none falls thro' the numerous holes at the bottom, whilſt the orifice at the top is clos'd; muſt be, that if in caſe the water ſhould deſcend, the air being unable to ſucceed it, there would be left a vacuum at the upper part of the veſſel, they ſeem to mean by a vacuum, any ſpace here below that is not fill'd with a viſible body, or, at leaſt, with air, tho' it be not quite deſtitute of all bodies whatſoever.

Taking then, a vacuum in this vulgar and obvious ſenſe, the common opinion about it ſeems liable to ſeveral exceptions, whereof ſome of the chief are ſuggeſted by our engine.

It ſeems unintelligible, how hatred or averſion, which is a paſſion of the ſoul, can either for a vacuum, or any other object, be ſuppoſed in water, or any inanimate body, which cannot be preſum'd to know when a vacuum would enſue, if they did not attempt to prevent it; nor to act contrary to what is moſt conducive to their own particular preſervation, for the good of the univerſe. The meaning, therefore, of this metaphorical expreſſion ſeems to be, that by the wiſe author of nature, the univerſe, and the parts of it, are ſo contriv'd, that it is as hard to make a vacuum in it, as if they ſtudiouſly conſpired to prevent it.

But our experiments teach, that this ſuppoſed averſion of nature to a vacuum, is merely accidental, or conſequent upon the weight, fluidity, or fluxility of the bodies here below; and, perhaps, principally of the ſpring of the air, whoſe conſtant endeavour to expand every way, makes it either ruſh, or compel the interpoſed bodies, into all ſpaces where it finds no greater reſiſtance than it can ſurmount; and ſhew, that the power, exciſed by nature, to avoid, or repleniſh a vacuum, is limited, and may be determined even to pounds and ounces.*

And the experiment we are now upon affords us a notable proof of the unheeded ſtrength of the preſſure ſuſtained by the free air, which we preſume to be uncompreſs'd: for hence we ſee, that even in our climate, and without any other compreſſion than what is natural, or ordinary, it bears ſo ſtrongly upon contiguous bodies, that a cylinder of it, not exceeding three inches in diameter, is able to raiſe, and carry up a weight, amounting to between ſixteen and ſeventeen hundred ounces. In more northern countries, the air may be much thicker, and able to ſupport a greater weight; ſince the *Hollanders*, who were forced to winter in *Nova Zembla*, found the air there ſo condenſed, that they could not make their clock go, by a very great addition to the weights that uſed to move it.

Bodies of different ſpecific gravities, loſe their equilibrium in vacuo.

34. We took a dry bladder, ſtrongly ty'd at the neck, and about half filled with air, and faſtening it to one part of a very exact balance, we put a metalline counterpoiſe into the oppoſite ſcale; and ſo the two weights being brought to an equilibrium, the balance was conveyed into the receiver, and ſuſpended from the cover of it: when we obſerv'd that preſently after laying on the cover, the bladder appear'd to preponderate; whereupon the ſcales being taken out, and reduced very near to an equilibrium, yet ſo, that a little advantage remain'd on that ſide to which the metalline weight belong'd; they were again let down into the receiver, which was preſently cloſed. Soon after this, before the pump was work'd, the bladder ſeem'd again a little to preponderate; and the air in the glaſs beginning to be drawn out, the bladder expanded itſelf, and greatly raiſed the oppoſite weight, by drawing down the ſcale to which it was faſtened, eſpecially when the air had ſwell'd it to its full extent. This done, we, very leiſurely let in the external air, and obſerv'd that, upon the flagging of the bladder, the ſcale whereto it was faſten'd, not only, by degrees, returned to an equilibrium with the other; but, at length, was a little outweigh'd by it; tho' the bladder, after a while, began again to preponderate, and, by degrees, to ſink lower for ſeveral hours: wherefore, leaving the veſſel cloſed up all night, we, next morning, found the bladder fallen

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* "All the parts of ſpace," ſays Sir *Iſaac Newton*, "are not equally full; for if they "were, the ſpecifick gravity of the fluid, "which would fill the region of the air, "could not, by reaſon of the exceeding "great denſity of its matter, give way to "the ſpecific gravity of quick-ſilver, gold, "or any body how denſe ſoever; whence "neither gold, nor any other body, could "deſcend in the air. For no bodies can "deſcend in a fluid, unleſs they be "ſpecifically heavier than it. But, if a "quantity of matter may, by rarifaction, be diminiſhed in a given ſpace, why "may it not diminiſh *in infinitum?* If "all the ſolid particles of bodies, are "of the ſame denſity, that is, have their "*vires inertiæ* as their magnitudes, and "cannot be rarified, without leaving pores, "there muſt be a vacuum." *Newton. Prin.* p. 368.

yet lower: as if the very ſubſtance of it, had imbibed ſome of the moiſture wherewith the air then abounded; as the ſtrings of muſical inſtruments, are known to ſwell ſo much in rainy weather, as to break. This conjecture is the more to be regarded, becauſe having a little warm'd the bladder, we found it lighter than the oppoſite weight. And, without removing the ſcales, or the cover of the receiver, we again caus'd the air to be drawn out; the weather continuing very moiſt; but found not any manifeſt alterations in the balance.

But to make the experiment with a body, leſs apt to be alter'd by the temperature of the air, than a bladder, we brought the ſcales again to an equilibrium with two weights, the one lead, and the other cork. And, having exhauſted the receiver, obſerv'd, that both upon the exſuction, and after the return of the air, the cork manifeſtly preponderated: and much more, a while after the air had been let in again, than whilſt it was kept out. Wherefore, for the cork, we ſubſtituted a piece of charcoal, as leſs likely to imbibe any moiſture from the air; but the event proved much the ſame; ſo that this experiment ſeems very liable to caſualties.

The aſcent of liquors in ſiphons, and filtres, whence.

35. The true cauſe of the aſcent of liquors, in ſiphons and filtres, remaining unknown; we were deſirous to try whether the preſſure of the air might reaſonably be ſuppoſed to have any conſiderable ſhare in it. But, becauſe we could not ſo far evacuate our receiver, but the remaining air would impel the water to a greater height than is uſual in filtrations; inſtead of a liſt of cotton, or the like filtre, we made uſe of a ſiphon of glaſs, conſiſting of three pieces, two ſtrait, and the third crooked, to join them together; whoſe junctures were carefully cloſed, that no air might find entrance at them: one of the legs of this ſiphon was ſomewhat longer than the other, and pervious at the bottom of it, only by a hole almoſt as ſlender as a hair, that the water might drop very gently out of it. The ſhorter leg of the ſiphon was quite open at the end, and of the ſame diameter with the reſt of the pipe; that is, about a fourth of an inch. The whole ſiphon was deſign'd to be about a foot and a half long, that the remaining air, when the veſſel was exhauſted, might not impel the water to the top of it: then the ſiphon, being inverted, was fill'd with water, and the ſhorter leg let down, two or three inches, into a glaſs-veſſel; whilſt the upper part remain'd faſten'd to the inſide of the cover of the receiver.

Fig. 38.

And, till a conſiderable quantity of the air had been evacuated, the water dropp'd freely out at the lower end of the lower leg of the ſiphon; as if the experiment had been made in the free air: but, afterwards, the bubbles began to appear in the water; and, aſcending to the top of the ſiphon, run into one, which was gradually augmented by the riſing of other bubbles, that, from time to time, broke into it, but much more by its own dilatation, which increaſed, proportionably, as the receiver was evacuated, ſo that, at length, the water, in the ſhorter leg, was reduced, by the extraction of the ambient air, and the expanſion of the great bubble, at the upper part of the ſiphon, to the height only of a foot; whence, the courſe of the water, in the ſiphon, was interrupted, and that which remain'd in the longer leg of it, continued ſuſpended there, without dropping any longer. But, upon turning the ſtop-cock, the external air got into the ſiphon, by the little hole at which the water formerly dropp'd out: and, traverſing all the incumbent cylinder of water, in the form of bubbles, joined itſelf with that air which before poſſeſſed the top of the ſiphon.

Fig. 39.

To prevent the inconveniences ariſing from theſe bubbles, two glaſs pipes, like the former, were ſo placed, as to terminate together in the midſt of the belly of a glaſs-vial, into whoſe neck they were cemented; and then both the vial, and the pipes, being filled with water, the ſiphon was placed with its ſhorter leg in the glaſs of water, as before; and the experiment being proſecuted after the ſame manner, much more air was now drawn out before the bubbles cauſed any diſturbance; becauſe there was room enough in the vial for them to ſtretch, without depreſſing the water below the ends of the pipes; and during this time, the water continued to drop out of the lower leg of the ſiphon. But, at length, the receiver being very much emptied, the water ceaſed to run thro' the ſiphon; the upper ends of the pipes beginning to appear above the remaining water in the vial; the dilated air wherein, ſeem'd likewiſe to preſs down the water in the pipes, and fill the upper part of them.

Hence, the experiment being interrupted, we let in the air again, which, according to its various proportions of preſſure, to that of the air in the vial, and the pipes, exhibited a pleaſing variety of phenomema. And upon the whole, there ſeem'd little cauſe to doubt, if the bubbles had not diſturbed the experiment, that the courſe of water, thro' ſiphons, would have appear'd to depend upon the preſſure of the air.

Their aſcent in capillary tubes.

An eminent mathematician lately told me, ſome *French* gentlemen had obſerv'd, that, if one end of a ſlender open pipe, of glaſs, be dipp'd in water, the liquor will aſcend to ſome height in the pipe, tho' held perpendicular to the plain of the water; and, ſoon after, brought me two or three ſmall pipes of glaſs, which gave me the opportunity of trying it: tho' I had often before, in the long and ſlender tubes of ſome weather-glaſſes, made after a peculiar manner, taken notice of the like aſcent of liquors; but, preſuming it to be caſual, I made little reflection upon it. But, after this trial, ſuppoſing that tho' the water, in theſe pipes, roſe not above a quarter of an inch; yet if the tubes were ſlender enough, it might aſcend to a much greater height; I cauſed ſeveral of them to be dextrouſly drawn at the flame of a lamp, in one of which, that was almoſt incredibly ſlender, we found, the water aſcended five inches, tho' the pipe were held erect: but, if it were inclined, the water would fill a greater part thereof. We alſo found, that, when the inſide of the pipe was wetted before-hand, the water would riſe much better than otherwiſe. And ſome of theſe ſlender pipes, being bent, like ſiphons, we immerſed the ſhorter leg of one in a glaſs of fair water; and found, that the water, riſing to the top of the ſiphon, of itſelf, ran down the longer leg, and continued running, like an ordinary ſiphon. The cauſe of this aſcent of the water, appears very difficult to diſcover.* We try'd, indeed, by conveying a very ſlender pipe, and a ſmall veſſel of water, into our engine, whether the exſuction of the ambient air would aſſiſt us herein; but, tho' we employ'd red wine, inſtead of water, yet we could ſcarce certainly perceive, thorough ſo much glaſs as was interpoſed betwixt our eyes and the liquor, what happen'd in a pipe ſo ſlender, that the redneſs of the wine was ſcarce viſible in it. But, as far as we could diſcern, there happen'd no great alteration to the liquor; which ſeem'd the leſs ſtrange, becauſe the ſpring of that air, which might depreſs the water in the pipe, was equally debilitated with that which remain'd to preſs upon the ſurface of the water in the little glaſs. Wherefore, in favour of that conjecture, which aſcribes this phenomenon to the greater preſſure upon the water by the air, without the pipe, than by that within, it was ſhewn, that, in caſe the little glaſs-veſſel of water were ſo cloſed, that the air might, by the mouth, be ſuck'd out of it, the water would immediately ſubſide in the ſmall pipe. Hence, we might infer, that it aſcended before, by the preſſure of the incumbent air; only it may be objected, that this, perhaps, would not happen, were the upper end of the pipe in a vacuum; as alſo, that, 'tis very probable, the water may ſubſide, not becauſe the preſſure of the internal air is taken off by ſuction, but becauſe the ſpring of the external air impels the water in its way to the cavity, deſerted by the other air; and would as well impel the ſame water upwards, as make it ſubſide, were it not for the accidental poſture of the glaſſes. 'Twere here, likewiſe, proper to enquire, why the ſurface of water, in pipes, ſhould be concave; and, on the contrary, that of quick-ſilver, convex; and why if the end of a ſlender pipe be dipp'd in the latter, the ſurface of that fluid will be lower within the pipe, than without.

A parcel of air weighed.

36. We cauſed a glaſs-bubble to be blown at the flame of a lamp, about the ſize of a ſmall hen-egg, and of an oval form; only, at one end, there was

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* This Phenomenon, the ſuſpenſion of water in capillary tubes, is, with great ſagacity, accounted for by Dr. *Jurin*; who proves it owing to the attraction of the upper periphery, or ſection, of the concave ſuperficies of the tube; that is, a ſmall ſurface, or annulus, whoſe baſe is that periphery, and heighth the diſtance, whereto the attractive power of the glaſs extends. For the gravity of the water that enters the orifice of the tube, upon its immerſion, being immediately taken off, by the attraction of the annulus, wherewith its upper ſurface is in contact, the water muſt neceſſarily riſe higher, by reaſon of the preſſure of the ſtagnant fluid, and the attraction of the periphery immediately above that whereto the upper ſurface of the water is already contiguous. The conſideration of this phenomenon, and the experiments made with relation to it, both in water and quick-ſilver; thoſe made with the latter proving exactly the reverſe of the former; led the Doctor to clear the whole matter, by ſhewing, that the particles of water attract each other; that the particles of quick-ſilver attract each other; that water is attracted by glaſs; that quick-ſilver is attracted by glaſs; that the particles of water are more ſtrongly attracted by glaſs, than by one another; and, laſtly, that the particles of quick-ſilver are more ſtrongly attracted by each other, than by glaſs. See all theſe proved in the *Philoſ. Tranſ.* N°. 355. p. 739.

Fig. 34 p. 418.

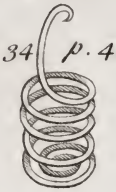


Fig. 40. p. 453.

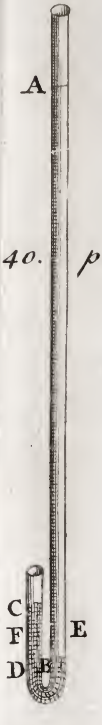


Fig. 36. p. 435.

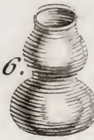


Fig. 33. p. 416.

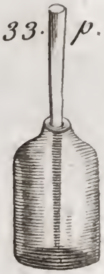


Fig. 32. p. 416.

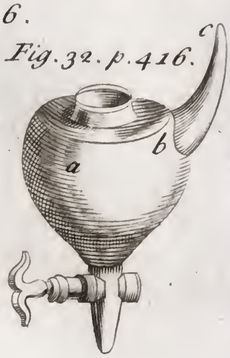
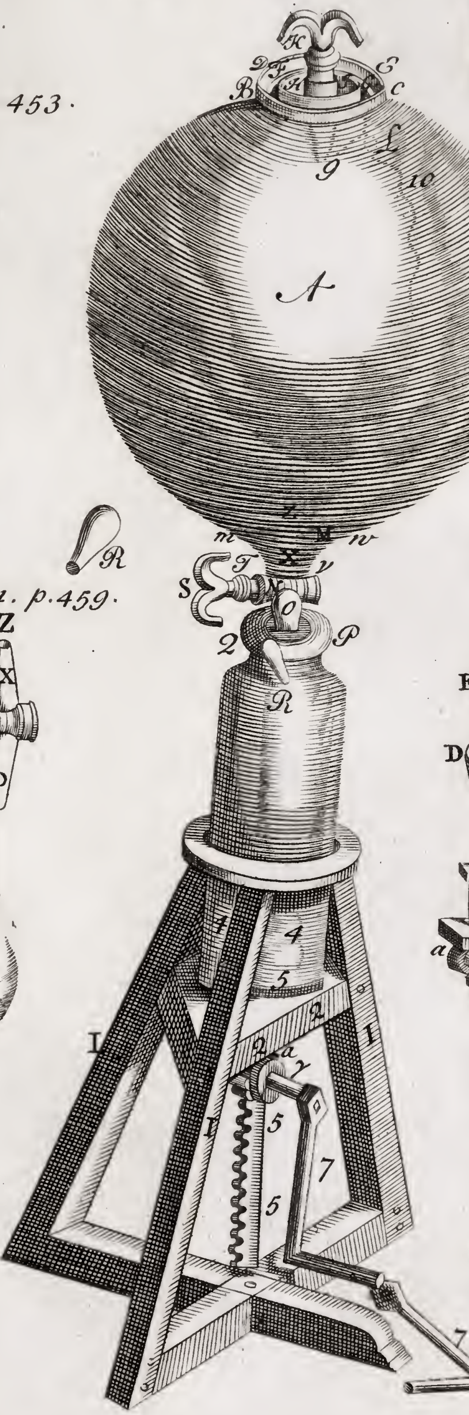
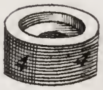
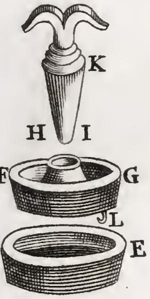
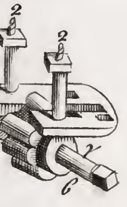


Fig. 30. p. 408.









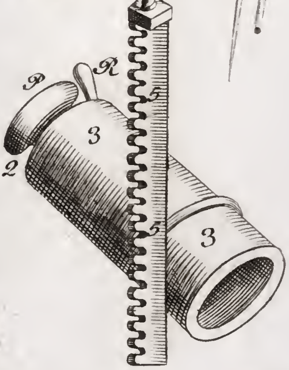


Fig. 35. p. 431.

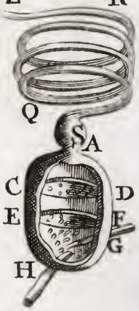


Fig. 41. p. 459.

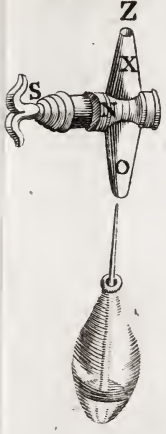


Fig. 39. p. 447

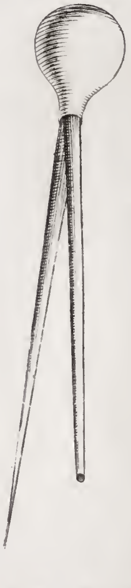


Fig. 38. p. 446.

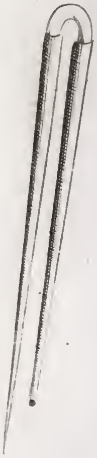
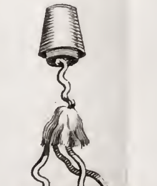


Fig. 37. p. 441.



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was drawn out an exceeding ſlender pipe, that the bubble might be ſeal'd up, with as little rarifaction, of the air included in the great cavity, as poſſible. This glaſs, being ſeal'd, was faſten'd to one of the ſcales of an exact balance; and, being counterpois'd with a weight of lead, was convey'd into the receiver; and cloſed up in it. The beam appearing to continue horizontal, the pump was ſet on work; and, after two or three exſuctions, the balance inclined to that ſide on which the bubble hung; which, as the air was farther drawn out, preponderated more manifeſtly; at length, the air being gradually let in again, the ſcales, by degrees, return'd to an equilibrium. Then we took them out, and caſting into that ſcale, to which the lead belong'd, three fourths of a grain, we again plac'd them in the receiver; which, being cloſed and exhauſted as before, as the air was drawn out, ſo the glaſs-bubble came nearer to an equilibrium with the other weight, till the beam ſtood horizontal; which, by another trial, we could not bring it to do, when one fourth of a grain more was added to the ſcale whereto the lead belong'd. Tho', without doubt, if we could have perfectly evacuated the receiver, the air included in the bubble, would have weigh'd above a grain; tho' it were ſomewhat rarify'd by the flame wherewith the bubble was ſeal'd. And, upon the return of the excluded air, the lead, and the weight caſt into the ſame ſcale, did again very much preponderate.

We, likewiſe, convey'd into the receiver, the ſame bubble, open'd at the end of the ſlender pipe above-mention'd; but, having drawn out the air as uſual, we found not, as before, the bubble to out-weigh the oppoſite lead; ſo that by the help of our engine, we can weigh the air, as we weigh other bodies, in its natural or ordinary conſiſtence, without condenſing it. Nay, having convey'd a lamb's bladder, half full of air, into the receiver, we obſerv'd, that tho' upon working the pump the impriſon'd air expanded, till it ſeem'd ready to burſt the bladder; yet this rarified air manifeſtly depreſs'd the ſcale whereto it was annex'd.

Whether glaſs be pervious to air.

And, having once caus'd the pump to be obſtinately ply'd, in repeating the former experiment, the impriſon'd air broke the containing glaſs-bubble, and threw the greateſt part of it againſt the ſide of the receiver, whereby it was ſhatter'd into a multitude of pieces. Hence we may diſcern, of how cloſe a texture glaſs is, ſince ſo very thin a film of it, as this bubble was, prov'd ſo impervious to the air, that it could not get away thro' the pores, but was forced to break the glaſs in pieces, to free itſelf; and this, notwithſtanding the time, and advantage it had, to force thro' the pores. This I mention, that our experiments may receive no prejudice from one I happen'd to make long ſince; which might be drawn to countenance their opinion, who would perſuade us that glaſs is pervious to air, properly ſo call'd; for, in diſtilling a certain ſubſtance, greatly abounding with ſubtile ſpirits, and a volatile ſalt, in a ſtrong earthen veſſel, of an unuſual ſhape, to which was luted a large receiver of green glaſs; the fire was, by accident, ſo exceſſively increas'd, that we found the ſpirituous and ſaline corpuſcles, thrown over ſo hot, and in ſuch plenty, into

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the receiver, as to render it all opake, and likely to fly in pieces. We ventur'd, however, to approach it, and obſerv'd, on the outſide thereof, at a great diſtance from the juncture, there was ſettled a round, whitiſh ſpot, or two, which, at firſt, we thought might be ſome ſtain upon the glaſs; but after finding it, in ſeveral qualities, like the oil and ſalt of the concrete diſtill'd, we ſuſpected, that the moſt ſubtile, and fugitive parts of the impetuous ſteams, had penetrated the ſubſtance of the glaſs, and, by the cold of the ambient air, were condenſed on the ſurface of it. And, indeed, upon examining the whole matter, a number of us unanimouſly concluded, that the ſubtile parts of the diſtill'd matter, being violently agitated by the exceſſive heat, had paſs'd thro' the pores of the glaſs, made wide by the ſame heat. But this having never happen'd, more than once, in any of the diſtillations we have either made, or ſeen, it is much more reaſonable to ſuppoſe, that the perviouſneſs of our receiver, to a body much more ſubtile than air, proceeded from the looſer texture of that particular parcel of metal, the receiver was made of; for all glaſs is not equally compact, and ſolid; and from the prodigious heat, which, together with the vehement agitation of the ſubtile ſpirits, open'd the pores of the glaſs; than to imagine, that ſuch a ſubſtance as air, ſhould be able to permeate the body of glaſs, contrary to the teſtimony of a thouſand chymical and mechanical experiments; and, of many made in our engine.

The penetrating power of air, compar'd with that of water.

And, the following experiment ſeems to teach, that tho' air, when ſufficiently compreſs'd, may, perhaps, get entrance into ſmaller cavities, than water; yet, unleſs the air be forc'd in, it will not paſs them, whilſt they may admit of water. I took a glaſs ſiphon, the lower end of whoſe longeſt leg was drawn ſo ſlender, that the orifice, at which the water was to fall out, would hardly admit a very ſmall pin. This ſiphon being inverted, we ſo order'd it, that a little bubble of air was intercepted in the ſlendereſt part, betwixt the little orifice, juſt mention'd, and the incumbent water; whence the air, being not to be forced thro' ſo narrow a paſſage, by ſo light a cylinder of water, as reſted upon it, hinder'd the farther efflux of the water, as long as we let it ſtay in that narrow place; but when, by blowing a little at the wider end of the ſiphon, that ſmall parcel of air was forc'd out, with ſome water; the remaining water, that before continu'd ſuſpended, began freely to drop down again, as before. And if a glaſs pipe, either in the form of a ſiphon, or otherwiſe, half an inch in diameter, but at one end ſo ſlender, as to terminate in an orifice almoſt as ſmall as a horſe-hair, be fill'd with water, it will drop down freely thorough the ſlender extremity. But if the pipe be inverted, the air will not eaſily get in at the ſmall hole, thro' which the water paſs'd. For, in the ſharp end of the pipe, ſome inches of water will remain ſuſpended; which, probably, would not happen, if the air could get in to ſucceed it; ſince, if the orifice were a little wider, the water would immediately ſubſide. And tho' when the pipe is many inches long, a great part of the water will run down at the wider orifice; yet that ſeems to happen for ſome other reaſon, than becauſe the air ſucceeds it at the upper and narrow one; ſince all the ſlender part of the pipe, and, perhaps, ſome inches more, will continue full of water.

And, tho' we have formerly ſhewn, that the aerial corpuſcles cannot paſs thro' the pores of a lamb's bladder; * yet, particles of water will; as may eaſily be try'd, by very cloſely tying a little alkaline ſalt in a fine bladder, and dipping its lower end in water; for, if it be held there for a competent time, there will ſtrain thro' the pores of the bladder, water enough to diſſolve the ſalt into a liquor.

But, to return to our bubble; we endeavour'd to meaſure its capacity by filling it with water, to find how much water anſwer'd, in weight, to $\frac{3}{4}$ of a grain of air; but all the diligence we uſed to preſerve ſo brittle a veſſel, could not prevent its breaking, before we had gain'd our point.

But, there occurs a problem, upon occaſion of the ſlow breaking of the glaſs bubble in our evacuated receiver. For, it might ſeem ſtrange, ſince the air, as we have ſeen, expands itſelf by its own internal ſpring, twice as much as *Merſennus* was able to rarify it by a red-hot æolipile; that yet, the ſpring of the air was ſcarce able to break a very thin glaſs bubble; and utterly unable to break one ſomewhat thicker, within whoſe cavity it was impriſon'd; whereas, air pent up, and agitated, is able to perform effects ſo much more conſiderable, that the learned jeſuit *Cabeus* tells us, he ſaw a vaſt ſtrong marble pillar quite broken off in the middle, by the heat proceeding from wood, which happen'd to be burnt juſt by it; which ſo rarified ſome air or ſpirituous matter ſhut up in the cavities of the marble, that it burſt thro' the ſolid body of the ſtone by the force of expanſion. But, probably, the reaſon why the included air did not break the ſeal'd bubbles, in our exhauſted receiver, was, that being ſomewhat rarified by the flame employ'd to ſeal the glaſs, its ſpring upon the receſs of the heat grew weaker than before. Yet, this will not, alone, ſerve the turn, becauſe, much ſmaller glaſs bubbles, exactly cloſ'd, will by the included air be made to fly in pieces.

The proportion of the weight of air to that of water.

We took an æolipile of copper, weighing ſix ounces, five drams, and forty-eight grains; and being made hot, we remov'd it from the fire, and immediately ſtopt it with hard wax, that no air might get in at its orifice. Then the æolipile, being ſuffer'd leiſurely to cool, 'twas again weigh'd, together with the wax, and found to be ſix ounces, ſix drams, and thirty-nine grains. Laſtly, the wax being perforated, without taking any of it out of the ſcale, the external air was ſuffer'd to ruſh in; and then the æolipile and wax being again weigh'd, amounted to ſix ounces, ſix drams, and fifty grains. So that the æolipile, freed as far as our fire could free it, from its air, weigh'd leſs than when repleniſh'd with air, full 11 grains; that is, the air containable within the cavity of the æolipile, amounted to 11 grains, and ſomewhat more. And, by the way, if there be no miſtake in the obſervation of *Merſennus*, it may ſeem ſtrange that it ſhould ſo much differ from two or three of ours; in none of which we could rarify the air in an æolipile, tho' made red-hot, almoſt all over, and immediately plung'd into cold water, to half that degree which he mentions, *viz.* ſeventy times its natural extent; unleſs the æolipile, he employ'd, was able to ſuſtain a more vehement heat than ours. [*]

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* M. *Homberg* is of opinion, that water enters ſuch narrow pores of animal ſubſtances, as will not admit the air, only becauſe it moiſtens and diſſolves the glutinous matter of the fine fibres of the membranes, and alſo renders them more pliable and diſtractile; which are things, that the air, for want of a wetting property, cannot do. As a proof of this doctrine, he fill'd a bladder with air, and compreſs'd it with a ſtone, and found no air to come out; but, placing the bladder, thus compreſs'd, in water, that air eaſily eſcaped. *Hiſt. de l'Acad.* A. 1700. p. 17.

This way of weighing the air, by the help of an æolipile, ſeems ſomewhat more exact, than that which *Merſennus* uſed, becauſe we weigh'd not the æolipile till it was cold; whereas he weigh'd it red-hot, whereby it is ſubject to loſe of its weight in cooling; for, copper heated red-hot, throws off, in the cooling, little thin ſcales in ſuch plenty, that, having purpoſely watch'd a copper æolipile, during its refrigeration, we have ſeen the place round about it, almoſt cover'd with them every way. Perhaps, too, the æolipile, in cooling, may not receive ſome little increaſe of weight, either from the vapid, or ſaline ſteams that float in the air. We employ'd, to weigh our æolipile, both when fill'd with air, and when repleniſh'd with water, a pair of ſcales that would turn with the fourth part of a grain. As to the proportion of weight betwixt air and water, ſome learned men have attempted to ſettle it, by ways ſo inaccurate, that they ſeem to have been much miſtaken. *Ricciolus* having purpoſely endeavour'd to diſcover this proportion, by means of a thin bladder, eſtimates the weight of the air, to that of the water, as about 1 to 10,000; and indeed, having once weigh'd a large bladder full of air, and found it to contain 14 grains; the ſame bladder, afterwards fill'd with water, contain'd near 14 pounds; whence the proportion of air to water, ſeem'd almoſt as a grain to a pound, that is, as 1 to above 7600. On the other hand, *Galileo* makes the air to water, as 1 to 400. But our way of weighing the air by an æolipile, ſeems by much the more exact. And, according to our obſervations, the water it contain'd amounting to 21 ounces and a half; and as much air as was requiſite to fill it, weighing 11 grains; the proportion in gravity of air, to water of the ſame bulk, will be as 1 to 938. And tho' we could not fill the æolipile with water very exactly, yet as we neither could perfectly drive the air out of it by heat, we think the proportion may hold good: however, in a round ſum, we may ſay, water is near 1000 times heavier than air. And accordingly, having, at another time, put ſome water in the æolipile, before we ſet it on the fire, that the vapours of the rarified liquor might the better drive out the air, we found, upon trial carefully made, that when the æolipile was refrigerated, and the included vapours, by the cold, turned again into water; the air being let in, increas'd the weight of the æolipile, eleven grains, as before; tho' there were already in it, twelve drams, and 32 grains of water, which remain'd of that we had put into it, to drive out the air. *Merſennus*, indeed, tells us, that, by his account, air is in weight to water, as 1 to 1356; and adds, that we may, without any danger, ſuppoſe, the gravity of water to that of air of a like bulk, as 1300 to 1; and, conſequently, that a quantity of air, to a quantity of water, equiponderant thereto, is as 1300 to 1. But why we ſhould relinquiſh our own carefully repeated trials, I ſee not; yet I am unwilling to reject thoſe of ſo accurate and uſeful a writer; and therefore ſuſpect, that the difference in our obſervations, proceeds from the different conſiſtence of the air at *London*, and at *Paris:* for, our air being more cold and moiſt than theirs, may be ſuppoſed, alſo, to be a fourth, or a fifth part heavier. Perhaps it may be of moment, too, that our obſervations were made in the midſt of winter, whilſt his might be made in ſome warmer part of the year.

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[*] It may be pretended, that 'tis not the air, but ſome vapour, or exhalation, contain'd in it, that here weighs upon the balance. To obviate this objection, M. *Muſchenbroeck* contriv'd the following experiment. It is a known thing in chymiſtry, that dry alkaline ſalts attract, and abſorb the moiſture of the air, and thereby run, *per deliquium*, as it is call'd. That philoſopher, therefore, having exhauſted a proper veſſel of its air, fitted another veſſel, wherein was lodg'd a large quantity of very dry ſalt of tartar, reduc'd to fine powder, and made hot, to the neck of the former; ſo that the external air muſt paſs ſlowly thro' this ſalt, before it could poſſibly get into the exhauſted veſſel; whereby the air that entred, was ſtrained, and perfectly freed from any moiſture that might have been lodg'd therein. The veſſel being thus fill'd with pure air, and put into the ſcale, was found to weigh as much as when fill'd with unpurg'd air. *De Mater. ſubtil.* p. 7.

The height of the atmoſphere computed.

It might be expected, that we ſhould, from theſe and other obſervations, decide the controverſy about the height of the atmoſphere; but, tho' it ſeems eaſy to ſhew that many famous writers have been miſtaken in aſſigning this height; yet, 'tis very difficult, preciſely to define its extent.

Fig. 40.

Now, we have already made it appear, that at leaſt about *London*, the proportion of ſpecific gravity betwixt water and air, is, as 1000 to 1. And, to determine the difference in weight betwixt water and quick-ſilver, we took a glaſs-pipe, in the form of an inverted ſiphon, and pouring into it a quantity of quick-ſilver, we held it ſo, that the ſuperficies of the liquor, both in the longer and ſhorter leg, lay in the horizontal line E F; then pouring water into the longer leg of the ſiphon, till that was almoſt fill'd, we obſerv'd the ſurface of the quick-ſilver in that leg to be, by the weight of the water, depreſs'd from E, to B, and in the ſhorter leg, to be as much impell'd upwards from F, to C. And having, before-hand, made marks, as well at the point B, as at the oppoſite point D, we meaſur'd both the diſtance D C, to have the height of the cylinder of quickſilver, which was rais'd above the point D, by the weight of the water, and the diſtance B A, which gave us the height of the cylinder of water. So that the diſtance D C, being $2\frac{13}{54}$ inches, and the height of the water $30\frac{45}{54}$ inches, the proportion appear'd to be as 121 to 1665, or as 1 to $13\frac{92}{121}$.

We alſo meaſured the proportion betwixt quick-ſilver and water by the help of a balance, which would turn with the hundredth part of a grain. But, becauſe an over-ſight is uſually committed in weighing quick-ſilver, and water; eſpecially if the orifice of the containing veſſel be wide; ſince the ſurface of water in veſſels, will be concave, but that of quick-ſilver conſiderably convex; to avoid this inconvenience, we made uſe of a glaſs-bubble, blown very thin, that it might not be too heavy for the balance, and terminating in a very ſlender neck, wherein the concavity or convexity of a liquor could not be conſiderable. This glaſs weighing $23\frac{1}{2}$ grains, we almoſt fill'd it with quick-ſilver, and faſtning a mark over againſt the middle of the protuberant ſuperficies, we found that the quick-ſilver alone, weigh'd $299\frac{7}{12}$ grains; then the quick-ſilver being pour'd out, and the ſame glaſs fill'd to the ſame height with common water, we found it to weigh $21\frac{7}{8}$ grains: whereby it appear'd, that the weight of water to quick-ſilver, is, as 1 to $13\frac{19}{26}$; tho' the illuſtrious *Verulam*, merely for want of exact inſtruments, makes the proportion betwixt them greater than of 1 to 17. And, by the way, ſince quick-ſilver, and well rectified ſpirit of wine are accounted, one the heavieſt, and the other the lighteſt of fluids; with the ſame glaſs, and ſcales, we found, the difference betwixt them, to be, as 1 and $16\frac{641}{1084}$; whence, the difference betwixt highly rectified ſpirit of wine, and common water, is, as betwixt 1 and $1\frac{44}{171}$. But to avoid fractions, let us ſuppoſe quick-ſilver is fourteen times as heavy as water. We have then given us, the proportion of air to water, and of water to quick-ſilver; from whence it is eaſy to find the proportion betwixt air and quick-ſilver; if we ſuppoſe the atmoſphere to be uniformly of ſuch a conſiſtence as here below. For, ſince our engine hath ſufficiently manifeſted, that 'tis the equilibrium with the external air, that, in the *Torricellian* experiment, ſuſtains the quick-ſilver; and, ſince by our accurate experiment, formerly mention'd, a cylinder of mercury, able to balance a cylinder of the whole atmoſphere, amounted to about thirty inches; and ſince, conſequently, we may aſſume the proportion of quick-ſilver to air, to be as 14000 to 1; it will follow, that a cylinder of air, able to maintain an equilibrium with a mercurial cylinder of two feet and a half in height, muſt amount to 35000 *Engliſh* feet, and conſequently to ſeven miles.

But we cannot ſafely conclude, that the air is every where of the ſame conſiſtence we found it, near the ſurface of the earth; not only becauſe, as *Seneca* ſays, "the air is more groſs, the nearer it comes to the earth, as "the feces fall to the bottom in water"; but becauſe the ſpringy texture of the aerial corpuſcles makes them capable of a very great compreſſure, which the weight of the incumbent part of the atmoſphere exerts upon the undermoſt, near the ſurface of the earth. And as we have ſeen, that air, much rarify'd without heat, may eaſily admit a farther rarifaction with it; and that, even without being expanded thereby, it is capable of being rarify'd to above a hundred and fifty times the extent it uſually poſſeſſeth here below; perhaps the atmoſphere may riſe to the height of ſome hundred miles: nay, exhalations may aſcend much higher, if there was no miſtake in that ſtrange obſervation made at *Toulouſe*, in a clear night in *Auguſt*, by the diligent *Emanuel Magnan*; who, as *Ricciolus* tells us, "ſaw, "from eleven a-clock at night, till twelve, while the moon was under the

" horizon, a little lucid cloud, near the meridian, and almoſt in the " zenith, which could be illumin'd by nothing but the ſun; and, there- " fore, muſt have been higher than the whole ſhadow of the earth. " And," ſays *Ricciolus*, " the like phenomenon was obſerv'd by the " great mathematician *Riccius*."

Various obſervations, made at the feet, tops, and interjacent parts of high mountains, might, perhaps, aſſiſt us to make an eſtimate in what proportion the higher air is thicker than the lower; and to gueſs at the different conſiſtence, as to laxity and compactneſs of the air, at ſeveral diſtances from us. And, if the difficulties about the refractions of the celeſtial luminaries were ſatisfactorily determined, that might alſo conduce to aſſign proper limits to the atmoſphere. But, at preſent, we dare not pronounce any thing, peremptorily, concerning the height of it.

Odd phenomena of light produced in the receiver of the air-pump.

37. We have often obſerv'd, that, when the ſucker of our pump was drawn down, immediately upon turning the key, there appear'd a kind of light in the receiver, almoſt like a faint flaſh of lightning, in the day-time; and almoſt as ſuddenly did it appear, and vaniſh. When we firſt took notice of this phenomenon, the day was clear, the hour about ten in the morning, and the only window in the room faced the north; and we found that, by interpoſing any opake body between the receiver, and the window, tho' the reſt of the room were ſufficiently enlighten'd, yet the flaſhes did not appear as before. As ſoon as night was come, we made the room very dark, and plying the pump, as in the morning, could not find, upon turning the key, the leaſt glimmering of light. Whence we inferr'd, that the flaſh, appearing in the receiver, did not proceed from any new light, generated there; but from ſome reflections of the light of the ſun, or other luminous bodies, placed without: tho', whence the reflection ſhould happen, was hard to ſay.

Wherefore, the next morning we went about to repeat the experiment; but tho' we could, as well as formerly, exhauſt the receiver; tho' the place wherein we made the trial, was the very ſame; and tho' other circumſtances correſponded; yet we could not diſcover the leaſt appearance of light all that day, no more than on ſeveral others: nor can we, to this very time, be ſure, a day before-hand, that theſe flaſhes will appear in our great receiver. Nay, having once found the engine diſpoſed to exhibit this phenomenon, we ſent notice of it to Dr. *Wallis*, who was then very near at hand, and made haſte to ſatisfy his curioſity; yet, by that time he arrived, the appearance was ceaſed: and having long, in vain, endeavour'd to exhibit it again, we were, after all, unexpectedly preſented with a few flaſhes.

And this contingency, whereto our experiment is liable, being ſuch, that in all ſuch conſtitutions of the weather, times of the day, &c. it will ſometimes anſwer, and ſometimes diſappoint our expectations; we are much diſcouraged from framing an hypotheſis to ſolve it; tho' it might be attempted from conſidering the following phenomena. (1.) The appearance may as well be exhibited by candle-light, as by day-light, and in whatever poſition the candle be held to the receiver, provided the rays of light be not hinder'd from falling upon the veſſel. (2.) The flaſh appears immediately upon turning the key, to let the air out of the receiver into the empty'd cylinder; ſo that, I remember not, that the flaſh appear'd, when at any time, in our great receiver, the ſtop-cock was open'd, before the cylinder was exhauſted. (3.) When, inſtead of the great receiver, we made uſe of a ſmall glaſs, not containing above a pound and half of water; the phenomenon might be exhibited, tho' the ſtop-cock remain'd open, provided the ſucker was drawn nimbly down. (4.) When we began to empty the veſſel, the appearances of light were much more conſpicuous, than towards the latter end, when little air at a time, could paſs out of the receiver. (5.) When the ſucker had lately been well oil'd, and, inſtead of the great receiver, the ſmaller veſſel, above-mention'd, was emptied; upon opening the ſtop-cock, as the air deſcended out of the glaſs into the emptied cylinder, there aſcended out of the cylinder into the veſſel a certain ſteam, which ſeem'd to conſiſt of very little bubbles, or other minute corpuſcles, thrown up from the oil, rarify'd by the attrition it ſuffer'd in the cylinder. For, at the ſame time that theſe ſteams aſcended into the glaſs, ſome of the ſame kind manifeſtly iſſued out, like a little pillar of ſmoke, at the orifice of the valve, when that was occaſionally open'd. And theſe ſteams, frequently preſenting themſelves to our view, we found, by expoſing the glaſs to a clear light, that they play'd up and down in it; and by their whitiſhneſs, in ſome meaſure reſembled the appearance of light. (6.) For, when the flaſh was great, the receiver, at the very inſtant, loſt of its tranſparency, by appearing full of ſome kind of whitiſh ſubſtance; and, for a ſhort time after, the ſides of the glaſs continu'd opake, and ſeem'd to be darken'd, as if ſome whitiſh ſteam adhered to the inſide of it.

But he who would fairly account for the phenomenon, whereof theſe are not all the circumſtances, muſt ſhew from whence the apparent whiteneſs proceeds; and why that whiteneſs ſometimes appears, and ſometimes not. Now, had our phenomenon been conſtant and uniform, we ſhould ſuſpect it to have been produc'd after the following manner; for tho' what we ſaw in our receiver, ſeem'd to be a kind of light, yet it was, indeed, but a whiteneſs, which render'd the inſide of the glaſs opake.

Now our common air abounds with particles, able to reflect the rays of light, as appears ſtom that vulgar obſervation, the motes in the air, when the ſun-beams ſhooting into any ſhady place, diſcover them, tho', otherwiſe, the eye cannot diſtinguiſh them from the air. And, I particularly remember, that being at ſome diſtance from *London*, at a time whem numerous bonfires happen'd to be made there; tho' we could not ſee the fires themſelves, yet we could plainly perceive the air all enlightned near the city: which argued, that the rays, ſhot upwards from the fires, met, in the air, with corpuſcles opake enough to reflect them to our eyes.

White may be produc'd, when the continuity of a tranſparent body happens to be interrupted by a great number of ſurfaces, which, like ſo many little looking-glaſſes, confuſedly repreſent a multitude of ſmall and ſeemingly contiguous images of the lucid body. For, water, or the whites of eggs, beaten to a froth, loſe their tranſparency, and appear white. And having, out of one of our ſmall receivers, carefully drawn out the air, and left a very little hole, by which the water was to get in, we obſerved that the neck, being held under water, and the little hole open'd, the water that ruſh'd in, was ſo broken, and acquired ſuch a multitude of new ſurfaces, that the receiver ſeem'd to be full rather of milk, than water. And farther, by heating a lump of cryſtal, and quenching it in fair water, it will be diſcontinued by ſuch a multitude of cracks, which create new ſurfaces within it, that tho' it will not fall aſunder, yet it loſes its tranſparency. and appears white.

Hence we might imagine, that upon the ruſhing of the air out of the receiver, into the empty'd cylinder, the air in the receiver, being ſuddenly, and vehemently expanded, the texture of it was as ſuddenly alter'd; and the parts made ſo to ſhift places, and, perhaps, ſome of them, to change poſtures, as during their new and vehement motion, and their varied ſituation, to diſturb the uſual continuity, and thereby, the tranſparency of the air; which ceaſing to be a tranſparent body, muſt eaſily degenerate into white.

Several things there are which make this conjecture ſeem the more probable; as, firſt, the whiteneſs always appear'd greater, whilſt there was much air in the receiver, than when the air was in great part drawn out. Secondly, having exhauſted the receiver, and applied to the hole in the ſtop-cock, a large bubble of clear glaſs; ſo that we could, at pleaſure, let the air paſs out, at the ſmall glaſs, into the great one, and eaſily fill the ſmall one with air again; we obſerved, that upon opening the communication betwixt the two glaſſes, the air, in the ſmaller, finding ſo much room in the greater, to receive it, flew out with ſuch force, that the ſmall vial ſeem'd to be full of milk: and this experiment we repeated ſeveral times. And, thirdly, having provided a ſmall receiver, with its upper orifice ſo narrow, that I could ſtop it with my thumb, I obſerv'd, that when, upon the exſuction of the air, the capacity of the glaſs appear'd white; if, by a ſudden removal of my thumb, I let in the outward air, that whiteneſs would immediately vaniſh. It may, indeed, be objected, that when water turns from tranſparent to white, the air intervenes, which converts it into bubbles. To this I reply, there are two very volatile liquors, which being gently put together, are as clear as rock-water, and yet will inſtantly, without the help of air to turn them into bubbles, ſo alter the diſpoſition of their inſenſible parts, as to become a white conſiſtent body. And this happens not as in the precipitation of benjamin, and ſome other reſinous ſubſtances; which being diſſolv'd in ſpirit of wine, may, by the affuſion of fair water, be turn'd into a milky ſubſtance: for this whiteneſs belongs not to the whole liquor, but to the corpuſcles of the diſſolv'd gum, which, after a while ſubſiding, leave the liquor tranſparent, themſelves only remaining white. But, in our caſe, 'tis from the varied texture of the whole tranſparent fluid; and not from any particular part, that this whiteneſs reſults: for the body is white throughout, and will long continue ſo; and yet may, in proceſs of time, without any addition, be totally reduced into a tranſparent body, as before.

Another conjecture, we grounded upon this obſervation: having convey'd ſome ſmoke into our receiver, placed againſt a window, we obſerv'd, that, upon the exſuction of the air, the corpuſcles floating in it, manifeſtly enough made the receiver ſeem more opake, at the very inſtant the air ruſhed out. For, conſidering that the whiteneſs, whoſe cauſe we enquire after, did but ſometimes appear, it ſeem'd not impoſſible, that, at ſuch times, the air in the receiver, might abound with particles capable of reflecting the light, in the manner requiſite to exhibit a white colour, by being put into a certain unuſual motion; as the new motion of their former fumes, made the inſide of the receiver appear darker than before; and as our ſmoking liquor, formerly mention'd, whoſe parts, tho' they ſeem'd tranſparent, whilſt they compos'd a fluid; yet when the ſame corpuſcles, upon unſtopping the glaſs, were put into a new motion, and diſpos'd after a new manner, they render'd that part of the air opake, wherein they mov'd, and exhibited a greater whiteneſs than ſometimes appears in our receiver.

But as to the reaſon why our phenomenon appears not conſtantly, I remember not that we ever made the experiment in a ſmall veſſel, without finding the expected whiteneſs. But it remains to be explained, why in our great receiver, the phenomenon ſhould ſometimes be ſeen, and often not. All I have to ſay on this head is, that the air about us, and much more that within the receiver, may be much alter'd by ſuch caſes, as few are aware of. The learned *Joſephus Acoſta* tells us, that "in *America* there "are winds which naturally trouble the water of the ſea, making it green "and black, and others as clear as cryſtal." And, tho' we convey'd into the receiver, the ſcales and the pendulums, formerly mention'd, clear and bright; yet, after the veſſel had been emptied, and, the air let in again, the luſtre of both appear'd tarniſh'd by a beginning ruſt. And, laſtly, having, with pure ſpirit of wine, drawn a tranſparent tincture out of a certain concrete, commonly reckon'd among minerals, we put it into a cryſtal-vial, carefully ſtopp'd it, and lock'd it up in a preſs; and this liquor, being a chymical rarity, and of a pleaſing golden colour, we had often occaſion to view it; and took notice that once it ſeem'd to be very thick: whereupon, we imagin'd it poſſible, that ſome of the mineral corpuſcles were then precipitating. But finding, after ſome days, that tho' no precipitation had been made, and that the liquor, retaining its former vivid colour, was grown clear again, as before; we lock'd it up again in the ſame preſs, and reſolv'd to obſerve whether the like changes would again appear in our tincture; and, in caſe they ſhould, whether they might be aſcribed to the alterations of the weather. But tho' during the greateſt part of a winter, and a ſpring, we obſerv'd the liquor would often grow turbid; and after a while, clear again; yet we could not find, that it depended upon any manifeſt changes in the air; which would be often dark and cloudy, when the tincture was clear and tranſparent; as, in clear weather, the liquor would, ſometimes, appear troubled, and more opake.

Water made to freeze in vacuo.

38. Into a glaſs vial, open at the top, we put a mixture of ſnow, and common ſalt; and in the midſt of this mixture, ſet a cylindrical glaſs, cloſely ſtopp'd at the lower-end, and open at the upper, where we fill'd it with common water; then let them all down into the receiver; and the pump being ſet on work, the ſnow began to melt faſter than we expected. However, by that time the receiver had been conſiderably exhauſted, which it was in leſs than a quarter of an hour, we perceived the water, near the bottom of the glaſs cylinder, to freeze; and the ice, by a little longer ſtay, ſeem'd to increaſe, and to riſe ſomewhat higher than the ſurrounding ſurface of the liquor whereinto, almoſt all the ſnow and ſalt were diſſolved. The glaſs being taken out, it appeared that the ice, was as thick as the inſide of the veſſel it filled; tho', into that, I could put my thumb. The upper ſurface of the ice was very concave, and held againſt the light, appear'd not deſtitute of bubbles; tho' they were fewer than if the water had been frozen in the open air. The like experiment we made, alſo, in one of our ſmall receivers, with like ſucceſs.

But, whence proceeds that ſtrange force, we may ſometimes obſerve in frozen water, to break the bodies that impriſon it, tho' hard and ſolid? A ſtone-cutter, lately complained to me, that, ſometimes, thro' the negligence of his ſervants, the rain being ſuffer'd to ſoak into marble, the violent froſts coming on, would burſt the ſtones. And, another tradeſman complain'd, that, even implements made of bell-metal, being careleſly expos'd to the wet, have been broken and ſpoil'd by the water, which, having entred at the little cavities of the metal, was there, afterwards froze and expanded into ice. And *Cabeus* tells us, that he ſaw a huge veſſel of exceeding hard marble ſplit aſunder, by congeal'd water. I know it will be ſaid, to ſolve this problem, that congelation doth not reduce water into leſs ſpace, than it before poſſeſs'd, but, rather makes it take up more. But, tho' we grant, that water ſwells in freezing; yet how cold, which, in weather-glaſſes, manifeſtly condenſeth air, ſhould expand either the water or the intercepted air, ſo forcibly as to perform what we have here related, remains to be diſcover'd.

A water thermometer in vacuo.

39. We took an oval glaſs, clear, and pretty ſtrong, with a ſhort neck at the obtuſer end, thro' which we thruſt, almoſt to the bottom, a pipe of glaſs, and cloſely cemented it to the neck: the upper part of the pipe was drawn, in ſome parts, more ſlender than a crow's quill, that the changes of the air in the glaſs-egg, might be the more conſpicuous; then we conveyed into the glaſs, five or ſix ſpoonfuls of water, part of which, by blowing air into the egg, was rais'd into the ſlender part of the pipe; ſo that the water was interpoſed between the external air, and that included in the egg. This weather-glaſs, was ſo placed, and cloſed up in

Fig. 41.

in the cavity of a ſmall receiver, that only the ſlender part of the pipe, to the height of four or five inches, paſſing thro' a hole in the cover, remain'd expos'd to the open air.

In evacuating the receiver, the water, in the pipe, deſcended about a quarter of an inch; and this upon two or three repeated trials; which ſeem'd to argue, that there was no heat produced in the receiver, upon the exſuction of the air: for even a little heat would probably, have been diſcover'd by that weather-glaſs; ſince, by the bare application of my hand to the outſide of the receiver, the warmth, after ſome time, having been propagated thro' both the glaſſes, and the interval betwixt them, to the impriſon'd air, ſo rarify'd it, that, by preſſing upon the ſubjacent water, it impell'd that in the pipe much higher than it had fallen downwards, upon the exſuction of the air.

Yet we do not hence conclude, that in the cavity of the receiver the cold was greater after the extraction of the air, than before.

If it be demanded, what then could cauſe the water to ſubſide; we anſwer, that, probably, it was the ſtretching of the glaſs-egg, which, upon the exſuction of the ambient air, was unable to reſiſt, as formerly, the preſſure of the included air, and of the atmoſphere, which, by the intervention of the water, preſs'd upon its concave ſurface. This ſeems probable, as well from the experiment about breaking a glaſs, by the force of the atmoſphere, as becauſe, when by drawing the air out of the receiver, the water in the pipe, was ſubſided, upon the re-admiſſion of the external air, to preſs againſt the convex ſurface of the egg, the water was preſently re-impell'd to its formerly height: for, if a glaſs-egg be blown exceeding thin, and afterwards broken, you may, by degrees, conſiderably bend ſome narrow parts of it; and upon the removal of what kept it bent, it will readily recover its former ſtate. From our experiment, then, it appears either that there ſucceeds no body in the room of the air drawn out of the receiver; or, that every ſubſtance is not ſubtile enough, readily to paſs the pores of glaſs tho' always ſufficiently agitated to produce heat, wherever it is found in plenty. So that if we admit no vacuum, this experiment requires us to allow a great diſparity, either as to bulk, or agitation, or both, betwixt ſome parts of the ætherial ſubſtance, and thoſe which, here below, produce heat and fire.

We try'd, alſo, what operation the extraction of the air would have upon camphire; which conſiſts of ſuch volatile parts, that they will exhale without any greater agitation, than that of the open air. But we found not, that even this looſe body, was ſenſibly alter'd thereby.

Inſects in vacuo.

40. We convey'd a large fleſh-fly into a ſmall receiver; and, at another time, ſhut into a great receiver, a humming-bee, that appear'd ſtrong and lively; we alſo procured a white butter-fly, and incloſed it in a ſmall receiver; where, though at firſt, he fluttered about, yet, preſently, upon the exſuction of the air, he fell down, as in a ſwoon; retaining no other motion, than ſome little trembling of the wings. The fly, after ſome exſuctions of the air, dropp'd down from the ſide of the glaſs, whereon ſhe

was walking: but, that the experiment of the bee might be more inſtructive, we convey'd in with her a bundle of flowers, which remain'd ſuſpended by a ſtring, near the upper-part of the receiver; and having provoked the bee, we excited her to fly up and down the veſſel, till, at length, ſhe lighted upon the flowers, when we preſently began to draw out the air, and obſerv'd, that tho', for ſome time, ſhe ſeem'd to take no notice of it, yet, within a while after, ſhe fell down from the flowers, without making any uſe of her wings.

Birds and mice in the exhauſted receiver.

41. To ſatisfy ourſelves, in ſome meaſure, why reſpiration is ſo neceſſary to the animals, that nature hath furniſh'd with lungs, we took a lark, one of whoſe wings had been broken by a ſhot; but, notwithſtanding this hurt, the bird was very lively; and put her into the receiver, wherein ſhe, ſeveral times, ſprung up to a conſiderable height. The veſſel being carefully cloſed, the pump was diligently ply'd, and the bird, for a while, appear'd lively enough; but, upon a greater exſuction of the air, ſhe began manifeſtly to droop, and appear ſick; and, very ſoon after, was taken with as violent and irregular convulſions, as are obſerv'd in poultry, when their heads are wrung off, and died; (tho' when theſe convulſions appear'd, we let in the air,) with her breaſt upward, her head downward, and her neck awry; and this within ten minutes, part of which time had been employ'd in cementing the cover to the receiver. Soon after we put a lively hen-ſparrow, which was not at all hurt, into the receiver; and proſecuting the experiment, as with the former, ſhe appear'd to be dead within ſeven minutes; one of which was employ'd in cementing on the cover: but, upon ſuddenly turning the key, the freſh air, flowing in, began ſlowly to revive her; ſo that, after ſome pantings, ſhe open'd her eyes, and regain'd her feet, and, in about a quarter of an hour after, attempted to eſcape at the top of the glaſs, which had been unſtopp'd to let in the air upon her: but the receiver being clos'd the ſecond time, ſhe died, violently convuls'd, within five minuts from the firſt ſtroke of the pump.

Then we put in a mouſe, newly caught, and, whilſt he was leaping up very high in the receiver, we faſten'd the cover to it; expecting that an animal uſed to live with very little freſh air, would endure the want of it better than the birds; but tho', for a while after the pump was ſet on work, he continu'd leaping up, as before; yet 'twas not long e'er he began to appear ſick, giddy, and to ſtagger; after which, he fell down as dead, but without ſuch violent convulſions as the birds had: when, haſtily letting in ſome freſh air upon him, he recover'd his ſenſes, and his feet, but ſeem'd to continue weak and ſick; at length, growing able to ſkip, as formerly, the pump was ply'd again, for eight minutes; about the middle of which ſpace, a very little air, by miſchance, got in at the ſtop-cock; and about two minutes after that, the mouſe, ſeveral times leap'd up lively; tho', in two minutes more, he fell down quite dead; yet with convulſions far milder than thoſe wherewith the birds expired. This alacrity, ſo little before his death, and his not dying ſooner than at the end of the eighth minute, ſeem'd owing to the air that paſs'd into the receiver: for, the firſt time, the convulſion ſeiz'd him, in ſix minutes after the pump began to be work'd. Theſe experiments ſeem'd the more ſtrange, becauſe, during a great part of thoſe few minutes, the engine could but inconſiderably rarify the air, and that too by degrees; and at the end thereof, there remain'd in the receiver, a large quantity: for, as we formerly ſaid, we could not draw down water in a tube, within much leſs than a foot of the bottom. And, by the exſuction of the air, and interſperſed vacuities, there was left in the receiver, a ſpace ſome hundreds of times exceeding the magnitude of the animal, to receive the fuliginous ſteams, from which, expiration diſcharges the lungs, and which, in the other caſes, may be ſuſpected, for want of room, to ſtifle thoſe animals that are cloſely pent up in too narrow receptacles.

Having cauſed theſe three creatures to be open'd, I could diſcover little of what we ſought for, and might poſſibly have found in larger animals: for tho' the lungs of the birds appear'd very red, and, as it were, inflamed; yet that colour is uſual in the lungs of ſuch winged animals: but in almoſt all the deſtructive experiments, made in our engine, the animals appear'd to die with violent convulſive motions. From whence, whether phyſicians can deduce any thing towards the diſcovery of the nature of convulſive diſtempers, I leave to them to conſider.

And, to obviate objections, and remove ſcruples, about the fuliginous ſteams of pent up animals, which are ſuppoſed to kill them; we ſhut up another mouſe, as cloſe as poſſible, in the receiver, where it liv'd about three quarters of an hour; and might, probably, have done ſo, much longer, had not a perſon of quality deſir'd to ſee whether the mouſe could be kill'd by the exſuction of the ambient air. Upon this, we open'd, for a while, an intercourſe betwixt the air in the receiver and that without, whereby the mouſe might be refreſh'd, tho' without uncementing the cover at the top; to avoid the objection that, perhaps, the veſſel was more cloſely ſtopp'd for the exſuction of the air than before.

The event was, that after the mouſe had liv'd ten minutes, the pump being a little out of order, he died with convulſive motions; wherein he made two or three bounds into the air, before he fell down dead.

I alſo cauſed a mouſe, that was very hungry, to be ſhut up all night into a well-cloſed receiver, with a bed of paper for him to reſt on; and caus'd the engine to be placed by the fire-ſide, to keep him from being deſtroy'd by the immoderate cold of a froſty night; and, the next morning, I found that he had devour'd a large part of the cheeſe that had been put up with him. And having thus kept him alive full twelve hours, we, by ſucking out part of the air, brought him to droop, and to appear ſwell'd; but, by letting it in again, we ſoon reduc'd him to his former livelineſs.

The nature of reſpiration conſider'd.

It may be here expected, I ſhould attempt to clear the nature of reſpiration; but I pretend to go no farther in it than our engine leads me.

'Tis alledged by thoſe who would have the lungs rather paſſive than active, in reſpiration, that as the lungs, being deſtitute of muſcles and fibres, are unfit to dilate themſelves; ſo, without the motion of the thorax, they would not be fill'd with air: ſince, as Dr. *Higmore* hath well obſerv'd, if a live dog hath a great wound made in his cheſt, the lobes of the lungs, on that ſide of the mediaſtinum, will collapſe and lie ſtill; whilſt the thorax, and the lobes on the other ſide of the mediaſtinum, continue their former motion. And if, at once, the muſcles of the cheſt be on both ſides diſſected; upon the ingreſs of the air, the whole lungs, tho' untouch'd, will remain without motion, at leaſt as to any expanſion, or contraction of their ſubſtance.

And *Bartholine* affirms, that if the diaphragm be wounded, the lungs will fall together, and reſpiration ceaſe; which appears to be true, provided the wound be large. And, indeed, the diaphragm ſeems the principal inſtrument of ordinary reſpiration; tho' the intercoſtal muſcles, and, perhaps, ſome others, may be allow'd eminently to concur in extraordinary caſes. But it is not yet decided, what conveys air into the lungs; for 'tis demanded, what ſhould bring the air into the lungs, if they do not attract it? To this queſtion, ſome of the beſt modern philoſophers anſwer, that, by the dilatation of the cheſt, the contiguous air is thruſt away; and that preſſing upon the next air to it, and ſo onwards, the propulſion is continued, till the air be drawn into the lungs, and ſo dilates them. It is again objected by *Bartholine*, that, according to this doctrine, a man could not fetch his breath from a great veſſel, with a ſlender neck, full of air; becauſe, when his mouth covers the orifice of the neck, the dilatation of his thorax could not propel the air of the veſſel into his lungs; being ſeparated by the incloſing veſſel, from the ambient air: and yet, it will be ſaid, experience witneſſeth, that out of ſuch a veſſel a man may ſuck air. But this difficulty our engine can eaſily ſolve; ſince many of the preceding experiments ſhew, that, in this caſe, there needs no propulſion of the air, by the ſwelling thorax, or abdomen, into the lungs: ſince, upon the bare dilatation of the thorax, the ſpring of that internal air, which poſſeſſes as much of the cavity of the cheſt, as the lungs fill not, being much weaken'd, the external and contiguous air, muſt neceſſarily preſs thro' the open wind-pipe into the lungs, as finding there the leaſt reſiſtance.

And hence, by the way, we are aſſiſted to judge of that famous controverſy, among naturaliſts and phyſicians, ever ſince the time of *Galen*; ſome maintaining that the cheſt with the contain'd lungs reſembles a pair of bellows, which are therefore fill'd, becauſe dilated: and others pleading that the compariſon ſhould be made with a bladder, which is therefore dilated, becauſe it is fill'd. For, as to the thorax, it ſeems evidently like a pair of bellows, to be partly fill'd with air, becauſe it was dilated; but as for the lungs, which want fibres to diſtend them, they may fitly be compared to a bladder; ſince they are dilated, by being fill'd with that air which ruſheth into them, upon the dilatation of the cheſt, in the cavity whereof, it finds leſs reſiſtance to its ſpring than elſewhere. And this calls to mind that ſtrange obſervation of *Nicholaus Fontanus*, a phyſician at *Amſterdam*, who declares, that, in a boy of the ſame city, four years old, there was found, inſtead of lungs, a certain membranous bladder, which, being fill'd with air, and furniſh'd with little veins, had its origin from the wind-pipe. This being ſuppoſed true, I leave it to be conſider'd, how well it will agree with moſt of the opinions, as to reſpiration.

And thus may the grand objection of *Bartholine*, and others, be anſwer'd; but I leave anatomiſts to conſider what is to be ſaid to ſome obſervations, that ſeem to contradict thoſe anatomical experiments above-mention'd: ſuch was, particularly, that in *Sennertus*, of a melancholy ſtudent, who, having ſtabb'd himſelf, and pierc'd the diaphragm in the tendinous part, lived ſeven months after the wound was made; but, dying at length, it appear'd ſo great, being, perhaps, dilated by his ſtraining to vomit, that the whole ſtomach was found to have got by it into the left ſide of the thorax. And ſuch alſo was the accident which happen'd to a nobleman whom I have ſeen, and who is yet alive; in whoſe cheſt, there hath, for theſe many years, remain'd a hole ſo great, that the motion of his heart may be perceiv'd thro' it. An ingenious conjecture hath been made, at the cauſe of the ſudden death of animals in the exhauſted receiver; which ſuppoſes it to be, not the want of air that deſtroys them, but the preſſure of that in the cavity of the cheſt; as if the ſpring thereof, being no longer balanced by the ambient air, thereby becomes ſo ſtrong, as to keep the thorax forcibly diſtended, hinder its wonted contraction, and ſo compreſs the lungs and their veſſels, as to obſtruct the circulation of the blood. But *Wallæus* relates, that he often obſerv'd, in the diſſection of live bodies, the membrane which inveſts the lungs, had pores in it, as big as the larger ſort of peas: which agrees with the obſervations of chirurgeons and phyſicians, that matter, collected in the thorax, hath penetrated into the lungs, and been diſcharged by coughing. And moſt of the animals, kill'd in our engine, were birds; whoſe lungs, Dr. *Harvey* has obſerv'd, very manifeſtly to open, at their extremities, into the abdomen: and, by ſuch perforations, we may well ſuppoſe the paſſage free, betwixt the external air, and that in the abdomen. Beſides, to ſhew that the animals, which expired in our glaſſes, need not be ſuppoſed to have been kill'd by the want of air; we foreſee another argument which ought not to be conceal'd. The poſſibility of a vacuum is frequently deny'd; and the ſpaces void of air, and other groſſer bodies are, all of them, ſuppoſed exactly repleniſhed with a certain ethereal matter, ſo thin and ſubtile, that it can freely penetrate the pores of the moſt compact, and cloſe bodies, even glaſs itſelf. Hence it may be ſaid, that the animals included in our receiver, died not ſo much for want of air, as becauſe the air pumped out, was neceſſarily ſucceeded by an ethereal ſubſtance, which, conſiſting of parts vehemently agitated, and ſo very ſmall, as, without reſiſtance, to paſs in and out, thro' the pores of glaſs; a conſiderable quantity of this reſtleſs matter, meeting together in the receiver, may be quickly able, by the exceſſive heat of it, to deſtroy a little animal, or, at leaſt, make the air too hot to be fit for reſpiration.

But we have already anſwer'd this objection, by the late experiments; which ſhew no heat to be generated in our exhauſted receiver.

It might, alſo, ſeem probable, that, upon the ſudden removal of the wonted preſſure of the ambient air, the warm blood of our animals was ſo vehemently expanded, as to diſturb the circulation, and ſo diſorder the whole œconomy of the body; did ſuch animals, alone, as are of a hot conſtitution, loſe their lives in the exhauſted engine. But as to the uſe of air, in reſpiration, 'tis known to ſerve in the production and modulation of the voice; the expulſion of excrements, by coughing; the conveying in of odours, by inſpiration, *&c.* which are rather convenient for the well-being of an animal, than neceſſary to life. *Hippocrates* ſays, of the air in animals endow'd with lungs, that "tis the cauſe both of life, and diſeaſes; "that 'tis ſo neceſſary, a man cannot live part of a day without it; and "that reſpiration, alone, is the action which can never be ſuſpended." But, as to the reaſon why the inſpiration, and expiration of air, are ſo very neceſſary to life, both naturaliſts, and phyſicians, differ ſo widely, that it will be very difficult, either to reconcile their opinions, or determine their controverſies.

Many ſuppoſe the chief uſe of reſpiration, is to cool and temper that heat in the heart and blood, which would, otherwiſe, be immoderate. They, alſo, ſuppoſe, that the air is neceſſary, by its coldneſs, to condenſe the blood that paſſeth out of the right ventricle of the heart into the lungs; whereby it may gain ſuch a conſiſtence, as is requiſite to make it fit fewel for the vital flame in the left ventricle of the heart. And indeed, fiſh, and other cold creatures, whoſe hearts have but one cavity, are unprovided of lungs. But, tho', poſſibly, the air inſpir'd, may, ſometimes, be of uſe in refrigerating the heart; yet, it may be objected, that ſeveral cold creatures, as, particularly frogs, ſtand in need of reſpiration; which ſeems unneceſſary for refrigeration to them, who are deſtitute of any ſenſible heat, and live in the cold water; that even decripid old men, whoſe natural heat is very languid, and almoſt extinguiſh'd, have, yet, a neceſſity of frequent reſpiration; that a temperate air, is fitteſt for the generality of breathing creatures; and as an air too hot, ſo alſo, an air too cold, may be inconvenient for them; that in ſome diſeaſes, the natural heat is ſo weaken'd, that were the uſe of reſpiration to cool, it would be more hurtful than beneficial, *&c.* Theſe, and other objections, might be oppos'd, and preſs'd, againſt the recited opinion; but, we ſhall only add, that it appears not, by our foregoing experiments, in the exhauſted receiver, where animals die ſo ſuddenly, for want of reſpiration, that the ambient body is ſenſibly hotter than the common air.

Others will have the very ſubſtance of the air to get, by the veſſels of the lungs, to the left ventricle of the heart, not only to temper its heat, but to provide for the generation of ſpirits. And theſe alledge the authority of the ancients, among whom, *Hippocrates* ſeems, manifeſtly, to favour their opinion; and both *Ariſtotle* and *Galen* ſometimes appear inclinable to it. But, it ſeems very difficult to ſhew, how the air is convey'd into the left ventricle of the heart; eſpecially, ſince the ſyſtole and diaſtole of the heart and lungs are very far from ſynchronal; beſides, the ſpirits appearing to be, but the moſt ſubtile and unctuous particles of the blood, ſeem of a very different nature from that of dry incombuſtible corpuſcles of air.

Another opinion of reſpiration makes the genuine uſe of it to be the ventilation of the blood, in its paſſage thro' the lungs; whereby it is diſburthen'd of thoſe excrementitious ſteams, proceeding, for the moſt part, from the ſuperfluous ſeroſities of the blood and chyle. But, this hypotheſis may be explain'd two ways. For the neceſſity of air in reſpiration, may be ſuppos'd to proceed from hence; that, as a flame cannot long ſubſiſt in a narrow and cloſe place, becauſe the fuliginous ſteams, it continually throws out, cannot be long receiv'd into the ambient body, which, after a while, growing too full of them, to admit any more, ſtifles it; ſo the vital fire in the heart requires an ambient body of a yielding nature, to receive into it the ſuperfluous ſeroſities, and other recrements of the blood; the ſeaſonable expulſion whereof, is requiſite to depurate the maſs, and make it fit, both to circulate, and to maintain the vital heat reſiding in the heart. The other way, is, by ſuppoſing, that the air doth, not only as a receptacle, admit into its interſtices the excrementitious vapours of the blood, when they are expell'd thro' the wind-pipe; but alſo conveys them out of the lungs; becauſe the inſpired air, reaching to all the ends of the *Aſpera Arteria*, there aſſociates itſelf with the exhalations of the circulating blood; and when it is exploded, carries them away with itſelf, as winds ſpeedily dry up the ſurfaces of wet bodies.

Now, to the firſt of theſe two ways, our engine affords us this objection; that upon the exſuction of the air, the animals die a great deal ſooner, than if it were left in the veſſel; though, by that exſuction, the ambient ſpace is left much more free to receive the ſteams, that are either breath'd out of the lungs of the animal, or diſcharg'd by inſenſible tranſpiration.

But, if the hypotheſis be taken in the other ſenſe, it ſeems agreeable to that grand obſervation, which the phenomena of our engine, and the relations of travellers ſuggeſt, that there is a certain conſiſtence of air requiſite to reſpiration; ſo that, if it be too thick, and already over-charg'd with vapours, it will be unfit to unite with, and carry off thoſe of the blood; as water will diſſolve, and aſſociate, but a certain proportion of ſaline particles; and, if it be too thin, the number or ſize of the aerial particles is too ſmall to receive, and carry off the excrements of the blood in due quantity.

Now, that air too much thicken'd with ſteams, is unfit for reſpiration, appears by what happens in the lead-mines of *Devonſhire*, and, perhaps, of ſome other countries; for, I am credibly inform'd, that damps often riſe here, which ſo thicken the air, as ſuddenly to ſtifle the workmen. And, that this proceeds, not from any arſenical, or poiſonous exhalation contain'd in the damp; but, from too great a condenſation of the air;

ſeems probable, becauſe it often leiſurely extinguiſhes the flames of their candles, or lamps; and alſo, becauſe in thoſe cellars, where large quantities of new wine are ſet to work, men have been ſuffocated by the ſteams exhaling from the muſt, and too much thickning the air; for this reaſon, in ſome hot countries, thoſe who have occaſion to go into ſuch cellars, carry with them a quantity of well-kindled coals, which they hold near their faces, whereby the fumes being diſſipated, and the air rarified, the ambient body is reduc'd to a conſiſtence fit for reſpiration.

And, by way of confirmation hereof, we may add, that in a ſmall receiver we carefully clos'd a bird, which, tho' for a quarter of an hour, he ſeem'd not much prejudic'd, by the cloſeneſs of his priſon, he, afterwards, began to pant vehemently, keep his bill open, and appear very ſick; and at length, after ſome long and violent ſtrainings, he caſt up a little matter out of his ſtomach; and this he did ſeveral times, till growing ſo ſick, that he ſtagger'd, and gaſp'd, and was ready to expire. Now, we perceived, that within three quarters of an hour, from the time he was put in, he had ſo thicken'd, and tainted the air, with the ſteams of his body, that it was become altogether unfit for the uſe of reſpiration; which is no wonder, ſince, according to *Sanctorius*, that part of our aliment, which goes off by inſenſible perſpiration, exceeds, in weight, all the viſible, and groſſer excrements, both ſolid, and liquid.

That air too much dilated, is unfit for reſpiration, the ſudden death of animals kill'd in our exhauſted receiver, ſufficiently manifeſts. And it may well be doubted, whether if a man were rais'd to the very top of the atmoſphere, he would be able to live there many minutes. *Joſephus Acoſta* tells us, that when he himſelf paſs'd the high mountains of *Peru*, to which, he ſays, the *Alps* ſeem'd but as ordinary houſes, compar'd with high towers; he and his companions were ſurpriz'd with extreme pangs of ſtraining, and vomiting blood, and with ſo violent a diſtemper, that he concludes, he ſhould undoubtedly have died, but, that this laſted not above three or four hours, before they came into a more natural temperature of air. Our author adds, that he is therefore perſuaded, "the element of the "air is there ſo ſubtile and delicate, as to be inconſiſtent with the reſpira- "tion of man, which requires a more groſs and temperate air."

But, perhaps, the air doth ſomething more, than barely help to carry off what is thrown out of the blood, in its paſſage thro' the lungs, from the right ventricle of the heart to the left. For in phlegmatic conſtitutions, and diſeaſes, the blood will circulate tolerably well, notwithſtanding its being exceſſively ſerous; and in aſthmatical caſes, tho' the lungs be greatly ſtuff'd with viſcid phlegm, yet the patient may live for ſome years; whence it is ſcarce probable, that either the detention of the ſuperfluous ſerum of the blood, for a few moments in the lungs, ſhould be able to kill a perfectly ſound and lively animal; for we commonly found, upon repeated trials, in a ſmall receiver, that, within half a minute, a bird would be ſurpriz'd by mortal convulſions, and within a minute more, would die, beyond a poſſibility of recovery from the air, tho' never ſo haſtily let in.

And, what ſhews it was not the cloſeneſs of the veſſel, but the ſudden exſuction of the air, that kill'd thoſe creatures ſo ſoon; we once inclos'd a bird in a ſmall receiver, where, for a while, he eat very chearfully ſome ſeeds that we convey'd in with him; and not only liv'd ten minutes, but had, probably, ſurviv'd much longer, tho' he had not been reſcu'd. Another bird being, within half a minute, caſt into violent convulſions, upon the exſuction of the air; we haſtily turn'd the ſtop-cock, to let it in again, whereby the gaſping animal was preſently recover'd. And at another time, we, at night, ſhut up a bird in one of our ſmall receivers, and obſerv'd, that, for a while, he was ſo inſenſible of the alteration of the air, that he fell aſleep, with his head under his wing; and tho' he afterwards awak'd ſick, yet he continu'd upon his legs for above 40 minutes; and then ſeeming ready to expire, we took him out, and ſoon found him lively. Upon the whole, there appears reaſon to ſuſpect, that there is ſome uſe of the air, which we do not yet thoroughly underſtand, that makes it ſo neceſſary to the life of animals.

Paracelſus indeed tells us, that "as the ſtomach concocts the aliment, "and makes part of it uſeful to the body, rejecting the other; ſo the "lungs conſume part of the air, and reject the reſt." Whence, according to him, we may ſuppoſe a little vital quinteſſence in the air, which ſerves to refreſh and reſtore our vital ſpirits; for which purpoſe, the groſſer, and far greater part of the air, being unſerviceable, it is not ſtrange that an animal ſhould inceſſantly require freſh air. This opinion indeed is not abſurd, but it requires to be explain'd and prov'd; beſides, ſome objections may be made to it, from what has been already argu'd againſt the tranſmutation of air into vital ſpirits. Nor is it probable, that the bare want of the generation of the uſual quantity of vital ſpirits, for leſs than one minute, ſhould be able to kill a lively animal, without the help of any external violence. And, upon this ſuppoſition, *Cornelius Drebell*, is affirm'd, by many credible perſons, to have contriv'd a veſſel to be row'd under water; for *Drebell* conceiv'd, that it is not the whole body of the air, but a certain ſpirituous part of it, that fits it for reſpiration; which being ſpent, the remaining groſſer body of the air, is unable to cheriſh the vital flame reſiding in the heart. So that, beſides the mechanical contrivance of his boat, he had a chymical liquor, which, by unſtopping the veſſel wherein it was contain'd, the fumes of it would ſpeedily reſtore to the air, foul'd by reſpiration, ſuch a proportion of vital parts, as would make it again fit for that office; and having made it my buſineſs to learn this ſtrange liquor, his relations conſtantly affirm'd, that *Drebell* would never diſcloſe it, but to one perſon, who himſelf told me what it was. I have therefore been ſometimes inclin'd to ſuppoſe, the air neceſſary to ventilate and cheriſh the vital flame, which ſome imagine to be continually burning in the heart; for that, in our engine, the flame of a lamp will vaniſh almoſt as ſoon after the exſuction of the air, as the life of an animal. We have made a hard body, in the form of a clove, but twice as long, and proportionably thick, of ſuch a compoſition, that if it be kindled at the upper end, it will moſt certainly burn away to the bottom, much better than a match: this we often convey'd, kindled at the upper end, into a ſmall receiver; but ſtill found, that tho' preſently, upon the exſuction of the air, it would leave ſmoking, and ſeem quite gone out; and again begin to ſmoke, as ſoon as the air was let in upon it; yet if the air were kept out but four or five minutes, the fire would be totally and irrecoverably extinguiſh'd. And, conveying a ſmall lamp into a large receiver, with highly rectified ſpirit of wine, we could not, upon ſeveral trials, make the flame laſt two minutes after the air was began to be drawn out. This latter opinion, however, has its difficulties: for tho', in the hearts of many animals, the blood be a warm liquor, and, in ſome, even hot; yet it is hard to conceive either how the air can get thither; or how, in caſe it could, it ſhould increaſe the heat: ſince, however the air may increaſe the heat of a coal, by blowing off the aſhes, and making the active corpuſcles penetrate farther into the kindled body, and ſhatter it the more; yet hot liquors have their heat allay'd, by air blown on them. And, ſince ſome naturaliſts think the heat reſiding in the heart, to be a true flame, but temperate as the flame of ſpirit of wine; which will long burn upon fine linen, or paper, without conſuming them; I wiſh they had been more curious to make different trials with that liquor. For the flame of highly rectified ſpirit of wine will not only conſume paper, and linen; but I have uſed it in lamps, to diſtil liquors out of tall cucurbits, and found that it gave, at leaſt, as great a heat as oil: nay, I have readily melted crude gold, with the bare flame of this ſpirit.

Dr. *Harvey* demands, "why a fœtus, even out of the womb, if involv'd "in the ſecundines, may live, for a conſiderable time, without reſpiration; "yet, if after having once began to breathe, its reſpiration be ſtopp'd, it "preſently dies?" We pretend not to ſolve this problem, but made the following experiment with a view to it. We caus'd a bitch to be ſtrangled, that was almoſt ready to whelp; and preſently opening her, found four puppies; one of which we freed from the coats that involv'd him, and from the liquor wherein he ſwam, and obſerv'd, that he quickly open'd his mouth very wide, mov'd his tongue, and exercis'd reſpiration. Then we open'd both his abdomen, and cheſt, and cut the diaphragm aſunder; notwithſtanding which, he ſeem'd often to endeavour at reſpiration, and remarkably mov'd the intercoſtal muſcles, part of the diaphragm, the mouth and tongue. But being deſirous to try whether the other young ones, that had not yet breath'd at all, would long ſurvive this; we took them out, and having opened them, found none of them ſo much alive as to have any perceptible motion in their hearts; whereas the heart of that which had once enjoy'd the benefit of reſpiration, continued its motion ſo long, that we obſerved the auricle to contract, after five or ſix hours; and it continued about two hours longer.

It is much doubted, whether fiſh breathe under water. That ſuch as are not of the whale kind, have no reſpiration, as 'tis exerciſed by beaſts and birds, may be argued from their having no cavity in their hearts, and from their want of lungs, whence they are obſerv'd to be mute; unleſs we ſay, that their gills anſwer to lungs. But that air is neceſſary even to the lives of fiſh; and that therefore, 'tis probable, they have ſome obſcure kind of reſpiration, ſeems manifeſt from obſervations, and experiments. Several authors tell us, that fiſh ſoon die in ponds, and glaſſes quite fill'd with water, if the one be ſo frozen over, and the other ſo cloſely ſtopp'd, that they cannot enjoy the benefit of the air. And our engine hath taught us, that many little parcels of interſperſed air lurk in water; and this, perhaps, fiſh may make ſome uſe of.

Removing a large eel, out of a veſſel of water, into our great receiver, we cauſed the air to be evacuated, and obſerv'd, that after ſome motion in the glaſs, ſhe ſeem'd ſomewhat diſcompoſed, and, at length, turn'd up her belly, and afterwards lay altogether moveleſs, as if quite dead; but upon taking her out of the receiver, ſhe ſhew'd herſelf as much alive as before.

But, indeed, a large grey houſe-ſnail, being clos'd up in one of our ſmall receivers, neither fell down from the ſide of the glaſs, upon drawing out the air; nor was ſo much as depriv'd of progreſſive motion thereby: tho', except this, we never put any living creature into our exhauſted receiver, but what gave ſigns of death.

Hippocrates, and ſome learned phyſicians of late, ſuppoſe, that a fœtus reſpires in the womb; but it ſeems very difficult to conceive how air ſhould traverſe the body of the mother, and the teguments of the child: and ſince nature hath, in new-born infants, contrived peculiar temporary veſſels, that the blood may circulate thro' other paſſages, than it does in the ſame individuals, when they come to have the free uſe of their lungs, 'tis improbable that the fœtus in the womb ſhould properly reſpire: but, then, ſince our experiments have manifeſted, that almoſt all kinds of liquors, as well as water, abound with interſperſed corpuſcles of air, it ſeems not altogether abſurd, that when the fœtus is grown big, it may exerciſe ſome obſcure reſpiration; eſpecially ſince children have been heard to cry in the mother's womb. And I know a young lady, whoſe friends, when ſhe once went with child, complain'd to me, that ſhe was ſeveral times much frighted with ſuch cries; which, till I diſabuſed her, ſhe, and her friends, look'd upon as portentous. And 'tis no very unfrequent thing, to hear the chick pip in the egg, before the ſhell is broken. This, however, I only bring as a probable argument, till I can diſcover whether the motion of a rarified ſubſtance, tho' no true air, may not, at the top of the larynx, produce a ſound; ſince the blade of a knife, held in ſeveral poſtures, in the ſteam of the vapours that iſſues out of an æolipile, will afford various and very audible ſounds. I have, alſo, had thoughts of trying to make a large receiver, with little glaſs windows, capable of holding a man, who may obſerve ſeveral things as to reſpiration, *&c.* and, in caſe of fainting, may, by giving a ſign, be immediately relieved with freſh air. And it ſeems not impoſſible, that ſome men, by uſe, may bring themſelves to ſupport the want of air a pretty while; ſince we ſee that ſeveral will live much longer than others under water. Thoſe who dive for pearls in the *Weſt-Indies*, are reported to be able to ſtay a whole hour under water: and *Cardan* tells us of one *Colanus*, a diver in *Sicily*, who was able to continue there three or four times as long. We have alſo often ſeen in *England*, a corpulent man, who deſcends to the bottom of the *Thames*, and thence brings large fiſh, alive in his hands, out of deep holes; as *Acoſta* tells us, he ſaw in *Peru*, the like manner of fiſhing practiſed by the *Indians*.

However, there are but few men, who, even by uſe, can ſupport, for many minutes, the want of air: a famous diver, of my acquaintance, tells me, that at the depth of 50 or 60 feet under water, he cannot continue above two minutes, without reſorting to the air which he carries down with him in an engine. He alſo told me, that by the help of ſpunges dipp'd in oil, and held in his mouth, he could much longer ſupport the want of reſpiration, under water, than without them: the true cauſe of which, would, perhaps, if diſcover'd, hint the nature of reſpiration in fiſh. But the neceſſity of air to the greateſt part of animals, unaccuſtomed to the want of it, may be beſt judg'd of by the following experiment.

We convey'd a bee, a fleſh-fly, and a palmer-worm, into one of our ſmall receivers, and upon exhauſting thereof, obſerv'd, that the bee and the fly fell down, and lay with their bellies upwards, and that the worm ſeemed to be ſuddenly ſtruck dead; all of them lying without motion, or any other diſcernible ſign of life, in leſs than one minute; notwithſtanding the ſmallneſs of the animals, in proportion to the receiver, which too, was not free from leaks: but we had no ſooner re-admitted the air, than all the three inſects gave ſigns of life, and, by degrees, recover'd. When we had again drawn out the air, their motions preſently ceaſed, and they fell down, ſeemingly dead, as before; continuing moveleſs, as long as, by pumping, the veſſel was kept exhauſted. Herein appears the wiſe conduct and goodneſs of the creator, who, by giving the air a ſpring, hath made it very difficult to exclude a thing ſo neceſſary to animals. And here we may ſuſpect, that if inſects have no lungs, nor any part anſwering thereto, the ambient air affects, and relieves them, at the pores of their ſkin; for, as *Hippocrates* well ſaid, "a living body is every where perſpirable." Thus the moiſter parts of the air readily inſinuate themſelves into, and recede from the pores of the beards of wild oats, and of other wild plants, which almoſt continually wreath and untwiſt themſelves, according to the lighteſt variations in the temperature of the air.

We, particulary, took notice in this experiment, that, when, at any time, upon the re-admiſſion of the air, the bee began to recover, the firſt ſign of life ſhe gave was a vehement panting, which appeared near the tail; the like we have obſerv'd in bees drown'd in water, when they firſt come to be revived by a convenient heat; as if the air were, in one caſe, as proper to ſet the ſpirits, and alimental juice in motion, as heat, in the other.

This experiment, alſo, ſeems to manifeſt, that, even living creatures, man always excepted, are a kind of very curious machines. For, here we ſee animals lively, and perfectly ſound, immediately deprived of motion, and all diſcernible ſigns of life, and reduced to a condition that differs from death, only, in being not abſolutely irrecoverable: and this is perform'd without the leaſt external violence, more than is offer'd to a wind-mill, when, the wind ceaſing to blow on the ſails, all the ſeveral parts remain moveleſs, and uſeleſs, till a new breeze puts them again into motion.

'Tis known, that bees, and ſome other inſects, will walk, and fly, for a great while after their heads are off, and ſometimes one half of the body will, for ſeveral hours, walk up and down, when it is ſever'd from the other; yet, upon the exſuction of the air in this experiment, not only the progreſſive motion of the whole body, but the very motions of the limbs immediately ceaſe; as if the air were more neceſſary to theſe animals, than their own heads.

But, in theſe inſects, that fluid body, in which life chiefly reſides, ſeems nothing near ſo diſſipable, as in perfect animals. For, the birds convey'd into our ſmall receiver, were within two minutes, brought paſt recovery; but we were unable to kill our inſects by the exſuction of the air: for, tho' as long as the pump was kept working, they continued immoveable, yet when that reſted, the air, which preſs'd in at the unperceiv'd leaks, ſlowly reſtored them to the free exerciſe and functions of life. Without denying, then, that the air may be, ſometimes, very uſeful, by condenſing, and cooling the blood that paſſeth thro' the lungs; I am of opinion, that the depuration of that animal fluid, is one of the ordinary, and principal uſes of reſpiration.

Whether the action of menſtruums depends upon the preſſure of the air.

42. Having entertain'd a ſuſpicion, that the action of corroſive liquors in diſſolving bodies, may be conſiderably varied by the gravitation, or preſſure of the incumbent air, and the removal of it; I examined my conjecture by the following experiment.

I caſt ten whole pieces or ſprigs of red coral, into as much ſpirit of vinegar as reach'd an inch above them; then putting theſe together, with the menſtruum, into a long neck'd vial, whereof they ſcarce fill'd a third part, we convey'd that vial into one of our ſmall receivers, and having faſten'd on the cover, we let the liquor remain unmov'd a while. But finding, there only aroſe, as before, a number of ſmall bubbles, that cauſed no ſenſible froth upon the ſurface of the vinegar; we made two or three exſuctions of the air, upon which there roſe from the coral, ſuch a multitude of bubbles, as made the whole body of the menſtruum appear white; and ſoon after, yielded a froth, equal in magnitude to the reſt of the liquor; the menſtruum plainly appearing to boil: tho', if we deſiſted but one minute from pumping, the decreaſe of the froth, and ebullition, upon the getting in of a little air, at ſome leak or other, ſeem'd to argue, that the removal of the preſſure of the external air gave occaſion to this efferveſcence. But, for farther ſatisfaction, we let in the external air at the ſtop-cock, when, immediately, the froth vaniſhed; and ſo many of the bubbles, within the body of the liquor, diſappear'd, that it loſt its whiteneſs, and became tranſparent again; the menſtruum, alſo, working as languidly upon the coral, as before they were put into the receiver: but, when we had again drawn out the air, firſt the whiteneſs re-appear'd, and then the ebullition was renew'd; which, at length, grew ſo great, that, for three or four times ſucceſſively, when the air was let out of the receiver into the emptied cylinder, the froth overflowed the glaſs, and ran down the ſides of it: and yet, upon re-admitting the excluded air, it grew, immediately, calm and tranſparent; as if its operation upon the coral, had been facilitated by the exſuction of the incumbent air; which, on its receſs, left it eaſier for the more active parts of the liquor to ſhew themſelves, than whilſt the preſſure of the air continued. It may indeed, be ſuſpected, that thoſe vaſt and numerous bubbles proceeded not from the action of the menſtruum upon the coral, but from the ſudden emerſion of thoſe many little parcels of air, which are diſperſed in liquors; but, having had this ſuſpicion before we made the experiment, we convey'd our diſtill'd vinegar, alone, into the receiver, and kept it a while there, to free it from its bubbles, before ever we put the coral into it. It may be ſuſpected likewiſe, that the agitation of the liquor, conſequent upon ſhaking the glaſs, by pumping, might occaſion the ebullition; but, upon trial, there appear'd no conſiderable change in the liquor, or its operation, tho' the containing veſſel was ſhaken, if no air were drawn out. The experiment was again made in a ſmall receiver, upon coral groſly powdered, with a ſucceſs very like the former; only the coral, being now reduced to ſmaller parts, ſo many little lumps of it would, upon the ebullition of the liquor, be carried, and buoy'd up, by the emerging bubbles, as ſometimes to darken the vial; tho' they would fall again, upon letting in the air. We muſt not omit, that, when the ſpirit of vinegar was boiling upon the coral, we took out the vial, but could not find that the liquor was ſenſibly hot.

The ebullition warm of liquors in vacuo.

43. We cauſed water to be long boil'd, that it might be freed from its air; then, almoſt filling a four-ounce glaſs-vial with it, we convey'd that whilſt the water was yet hot, into a ſmall receiver; and having luted on the cover, the air was drawn out: upon the two firſt exſuctions, there ſcarce appear'd any change in the liquor; nor was there any great alteration made by the third; but at the fourth, and afterwards, the water appear'd to boil in the vial, as if it had ſtood over a very ſtrong fire; for the bubbles were much greater than are uſually found upon the ebullition of large quantities of water. And this efferveſcence was ſo great, that, the liquor, boiling over the top of the neck, much of it ran down into the receiver, and, ſometimes, continued to boil there. In proſecuting the experiment, we obſerv'd, that, ſometimes, after the firſt ebullition, we were obliged to make ſeveral exſuctions, before the liquor could be brought to boil again: but, at other times, as often as the air was ſuffered to paſs from the receiver into the pump, the efferveſcence would begin afreſh tho' the pump were ply'd for a pretty while together: which ſeem'd to argue, that the boiling of the water proceeded from hence, that, upon withdrawing the preſſure of the incumbent air, either the fiery corpuſcles, or rather the vapours agitated by the heat in the water, were permitted

greatly to expand themſelves in the evacuated receiver; and, in their tumultuous dilatation, lifting up the higher part of the water, and turning it into bubbles, made it appear to boil, for the effervеſcence was confined to the upper part of the water; the lower remaining quiet, unleſs the liquor were but ſhallow. And tho', ſometimes, as we ſaid, the ebullition began again, after it had ceaſed a pretty while; whence it ſeem'd that ſome concurrent cauſe did a little modify the operation of heat; yet, when the water, in the vial, could, by pumping, be brought to boil no more, the ſame water being, in the very ſame vial, convey'd back to the receiver, was quickly brought to boil afreſh, with vehemence, and for a conſiderable time; whilſt a new parcel, taken out of the ſame boil'd water with the former, and put in cold, could not, by pumping, be brought to ſhew the leaſt effervеſcence. And hot ſallet-oil ſhew'd no effervеſcence in our receiver; but the chymical oil of turpentine was preſently made to boil up, till it reached four or five times its former height in the vial; and continued boiling, till it was almoſt but luke-warm. Wine, alſo, being convey'd in hot, did, at the very firſt exſuction, begin to boil ſo vehemently, that, in a ſhort time, while the pump was kept moving, four parts of five boiled over the vial, tho' it had a long neck. And even of the water itſelf, near one half would, ſometimes, boil over into the receiver, before it became luke-warm. It was alſo, remarkable, that once, when the air had been drawn out, the liquor did, upon a ſingle exſuction, boil ſo long, with prodigiouſly vaſt bubbles, that the effervеſcence laſted almoſt a quarter of a minute. Hence it appears, that the air by its ſtronger, or weaker preſſure, may very much modify ſeveral operations of that vehement and tumultuous agitation of the ſmall parts of bodies, wherein the nature of heat ſeems to conſiſt: ſo that if a heated body were conveyed above the atmoſphere, it is probable, that the heat would have a different operation, as to the power of diſſipating the parts of it, from what it hath here below.

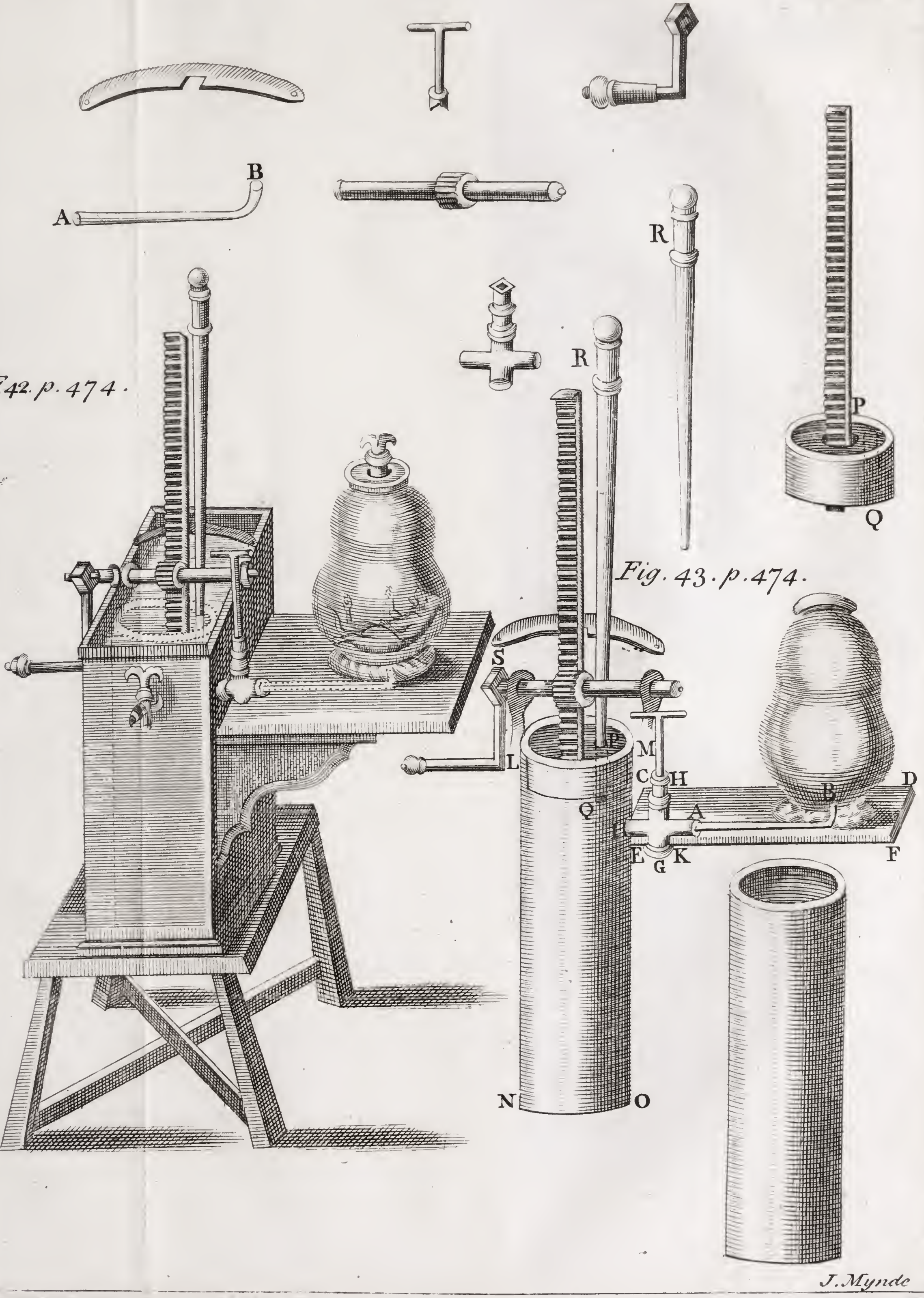
SECT. II.

The air-pump farther improved.

HAving now preſented my great engine to the royal ſociety, I was obliged to procure another; wherein, tho' the conſtruction, in general, be the ſame in both, there were ſome alterations, and improvements made.

Fig. 42 & 43.

The figures repreſent this engine, as ready for work; and, becauſe the ſucker is to be always under water, and the perforation P Q, that is continu'd perpendicular quite thro' it, and ſerves together with the ſtick R S, for a valve, is to be ſtopp'd at the bottom of the cylinder, as at N O, when 'tis full of water; 'twas requiſite to make the ſtick R P, two or three feet long. But the chief thing is, that, in the ſecond figure, the pipe A B, whoſe end B, bends upwards, lies in a groove, purpoſely made, in the flat wooden board C D E F, on which the receivers are to reſt. This ſquare board, I caus'd to be overlaid with very good cement, on which was applied a ſtrong plate of iron, of the bigneſs and ſhape of the board, leaving only a ſmall



F.42. p. 474.

Fig. 43. p. 474.

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ſmall hole, for the erect part of the pipe to come out at; which I added, not only to keep the board from warping, but becauſe the preſſure of the atmoſphere on the ſide of it, when there is none, or very little, on the other, will enable many aerial particles to ſtrain thro' the wood, though thick, and moiſten'd with oil. To this iron-plate, we can fit a lip, turning up about it, to prevent the water, that on ſome occaſions, comes from the receiver, from falling on the floor. And, by the way, tho' the ſtop-cock G H I K, that belongs to the pipe, may be inſerted at I, into the cylinder L M N O, by the help of ſolder; yet we choſe to have the branch I, of the ſtop-cock, made like a ſcrew, which, being once firmly fitted to the barrel, is not apt to be broken off, and may be more eaſily mended, if any thing happen to be out of order; which the engine is moſt liable to, in, or about the pipe.

The ſquare, and hollow wooden part of this engine diſcernible in the firſt figure, is ſo made, not only to contain the cylinder, but as much water as will always keep it quite cover'd, by which means, the ſucker lying and playing always under water, is continually kept plump, and turgid; and the water being ready to fill up any little interval, that may happen, between the ſucker, and the inſide of the barrel, farther conduces to keep out the air. But, if great care be not taken in turning the ſtop-cock, the water will be impell'd into the receiver, and prejudice ſeveral experiments, when the included bodies may be ſpoiled, or impaired by that liquor.

The flat-plate, lately mention'd, has this great conveniency in many experiments, that the receiver needs no ſtop-cock of its own; for ſuch a veſſel being made of an entire piece of glaſs, and laid upon the plate, well cover'd with cement, can better keep out the air, than if there were a ſtop-cock, at which the air too frequently gets in.

A good cement, wherewith to faſten the receivers to the iron plate, is a thing of great moment in making the following experiments, and we employ different compoſitions for different purpoſes; but, in general, only a mixture of bees-wax and turpentine, made with equal parts for the winter, and three parts of the former to two of the latter, for the ſummer.

Mercury rais'd by the ſpring of a little included air.
Fig. 44.

1. We took a vial with a ſmall neck, and having fill'd about a fourth part of it with quick-ſilver, we ſo erected and faſten'd a long and ſlender pipe of glaſs, open at both ends, in the neck thereof, with hard ſealing wax, that the lower end reach'd almoſt to the bottom of the quick-ſilver, and the upper more than a yard above the vial; then, having blown in a little air, we convey'd the whole into a long ſlender receiver: upon evacuating whereof, we found that the ſpring of the air included in the vial, impell'd the quick-ſilver into the erected pipe, to the height of 27 inches; and ſuffering the external air to return into the receiver, the quick-ſilver ſubſided in the tube, ſometimes almoſt, and ſometimes quite as low as the ſtagnant mercury in the vial.

This experiment we made ſeveral times; and having once blown in ſo much air, that what was in the cavity of the vial rais'd and kept the quick-ſilver three inches high in the pipe, found, by emptying the receiver, that the quick-ſilver roſe 30 inches, or more, above that in the vial.

Sometimes it may happen, that the mercury, when taken very ſoon out of the receiver, will not appear to have ſubſided to its firſt ſtation; which is not to be wonder'd at; ſince, in a receiver, containing but little air, the heat of the cement, and of the iron employed to melt it quite round the glaſs, may impart a little warmth to the air in the vial, which will afterwards return to its former temper.

'Tis very remarkable, if the receiver be properly ſtopp'd and ſlender enough, that upon the turning of the ſtop-cock to let out the air at the firſt exſuction, the mercury will be impell'd up by the ſpring of that in the vial, ſo as to riſe ſeveral inches above the height it will afterwards reſt at and make ſeveral vibrations up and down before it comes to ſettle; juſt as the mercury does in the *Torricellian* experiment: and ſuch motions of the mercury will be made for four or five ſubſequent exſuctions; but they grow gradually leſs, as the ſpring of the included air is weaken'd.

At the firſt exſuction, when the ſpring of the included air was yet ſtrong, we found the mercury would be rais'd above half, if not $\frac{2}{3}$ of the whole height, whereto 'twill, at length, aſcend: but the ſubſequent ſtrokes add a leſs proportion of height to the mercurial cylinder, ſucceſſively; becauſe the more mercury is impell'd into the tube, the greater weight preſſes upon the included air; and becauſe the air hereby gains the more room, in the vial, to expand itſelf: whence the ſpring muſt be, proportionably, weakned.

Laſtly, in making of theſe trials, I obſerved the mercury in a good barometer, and found its greateſt height twenty-nine inches, and $\frac{3}{4}$; and ſoon after we had finiſhed, but twenty nine.

To eſtimate the quantity of air, that had raiſed the quick-ſilver to twenty-ſeven inches; we counterpoiſed the vial, employ'd about this experiment, whilſt it was empty; afterwards fill'd it with water, and found the liquor to weigh five ounces, two drams, and twenty grains; then having pour'd out the water, till it was ſunk to a mark, we made on the outſide of the glaſs, we weigh'd the remaining water, equal in bulk to the quick-ſilver, and found it one ounce, two drams, fourteen grains: ſo that the air, which had rais'd up the mercury, poſſeſs'd, before its expanſion, the ſpace but of four ounces, and a few odd grains in the vial. The bore of the pipe uſed in this experiment, was about $\frac{1}{8}$ inch in diameter.

Much included air raiſes mercury, but to the uſual ſtandard of the barometer.

2. Into a ſtrong glaſs bottle, capable of holding a quart, we put a convenient quantity of quick-ſilver, and erected in it a very long ſlender pipe of glaſs, open at both ends, and reaching with the lower beneath the ſurface of the ſtagnant mercury; and having well cemented this pipe in the neck of the bottle, we convey'd the whole into a receiver, much larger than the former; and then the engine being work'd, the quick-ſilver was preſently rais'd to a greater height than before; and when it ſtood ſtill, we, by the help of ſome marks made before-hand on the pipe, and a very long and well-divided ruler, carefully meaſured the height of the mer-

curial cylinder, which we found to be 29 inches and about ⅞; but deducting half an inch, which was rais'd, before we employ'd the pump, by ſome air that had been blown into the bottle, to try whether it were ſtanch, there remain'd 29 inches and near ⅜ for the height of the mercury rais'd by the ſpring of the air ſhut up in the bottle; and then conſulting the barometer, which ſtood in another part of the houſe, I found the weight of the atmoſphere ſuſtain'd a mercurial cylinder, of about twenty-nine inches, and a half.

We cauſed the pump to be well ply'd, to try whether the quick-ſilver would not riſe higher; but were confirm'd, that the ſpring of the air was inſufficient for that purpoſe.

3. Taking the glaſs-bottle, uſed in the former experiment, and erecting in it, after the manner above-deſcribed, a cylindrical pipe of glaſs, much larger than the other; we proſecuted the experiment, as with the ſlender tube before-mention'd, and found, that by the ſpring of the air in the bottle, the quick-ſilver was rais'd twenty-eight inches, and one eighth; that is, above an inch ſhort of the mercurial cylinder in the barometer, at the ſame time; a difference no greater than I expected; conſidering the weight of the atmoſphere, remains the ſame, when the mercury is at its full height, in a ſeal'd tube, whether great or ſmall; whilſt the ſpring of our included air muſt needs be weaken'd, the larger the tube, and the higher the mercury is impell'd in it. Whence, 'tis conſiderable, that the ſpring of ſo little air ſhould raiſe the mercury within an inch as high in a wide, as in a ſlender tube; for the diameter of the bore of the former, was double to that of the latter: and the greater mercurial cylinder may be ſuppoſed to have weigh'd near four times as much as the leſs; allowance being made for an inch difference in their heights. But in caſe theſe had been equal, then the ſolidity of the cylinders would have been as their baſes; that is, as the ſquares of their diameters, or as 1 to 4.

The ſpring of included air raiſes mercury nearly to an equal height, in unequal tubes.

We thought it worth trying, whether, when the included air had raiſed this great cylinder of mercury to the utmoſt height it could, by the ſpring it then had, heat would not force it ſtill higher. And, having cauſed a hot iron, and a ſhovel of kindled coals, to be held near the oppoſite parts of the receiver; we perceived, after a while, that the mercury aſcended one eighth of an inch, or more, above the greateſt height it had reached before; and, cauſing the pump to be ply'd again, to withdraw the air I ſuſpected to have ſtole in; the mercury was quickly raiſed five eighths of an inch, by virtue of the additional force which the included air acquired by the heat.

4. We took a glaſs-bottle, furniſhed with a convenient quantity of water, and fitted it with a ſlender glaſs-pipe, about three feet long, open at both ends; which was ſo placed, that the lower orifice reached far beneath the ſurface of the water, and the pipe itſelf paſſed, perpendicularly, upwards, thro' the neck; which, by the pipe, and hard cement, was ſo firmly cloſed, that no water, or air, could get out of the bottle, or external air get into it, but by paſſing thro' the pipe. This inſtrument

A fountain made by the ſpring of uncompreſſed air. Fig. 45.

we convey'd into a large receiver, ſhaped like a pear; of which a great part of the obtuſe end, and a ſmall portion of the ſharp one, were cut off by ſections parallel to the horizon. And, becauſe this receiver was not long enough to receive the whole pipe, there was cemented on, to the upper part of it, a ſmaller, of ſuch a length and bigneſs, that the higher end of the pipe might reach to the middle of its cavity; and that the motions of the ſpringing water might have a convenient ſcope, and be the better obſerved.

This double receiver, being cemented on to the engine, a little of the air was, by one ſtroke of the pump, drawn from it; by which, the preſſure of the remaining air being weaken'd, that included in the bottle, having not its ſpring, likewiſe, weaken'd, expanded itſelf; and, conſequently, impell'd up the water, in the ſame bottle, thro' the pipe, ſo as to make it ſtrike briſkly, at firſt, againſt that part of the top of the ſmall receiver, which was juſt over the orifice of the tube. But, after the water was, for a while, thus forced up, in a perpendicular line, it would be impell'd up leſs ſtrongly, and leſs directly, till the air, in the bottle, being as much expanded, as that in the receiver, it quite ceaſed to aſcend, unleſs by pumping a little more air out of the receiver, we renew'd it again. The other figure is deſign'd to repreſent the difference that would happen, if, inſtead of making this experiment with water, it were made with quick-ſilver. *Fig* 46.

In making this experiment, 'tis convenient that the upper part of the pipe be very ſlender; whence the water, having but a very ſmall orifice to iſſue out at, may be ſpent but ſlowly, and thereby make the experiment laſt ſo much the longer: or, inſtead of making the upper part of the pipe ſlender; a top, conſiſting of three, or more, very ſlender pipes, with a ſmall hole at the end of each, may be cemented on to it; that one of theſe, pointing directly upwards, and the others to the right hand and to the left, the water may ſpin out ſeveral ways at once; by which kind of branched pipes, we have ſometimes imitated a *Jet d'eau*, and the artificial fountains of gardens and grotto's.

Hence we infer, that had we not wanted convenient veſſels, we might, by the preſſure of the air included in the bottle, have raiſed water fourteen times as high as we did quick-ſilver in the former experiment; ſince, upon weakening the preſſure of the air, but a little, in the double receiver, that within the bottle was able to impel the water, forcibly, and for a conſiderable time, to the top of a pipe a yard long, and higher.

Hence too, it appears, that in thoſe hydraulo-pneumatical engines, where water is placed between two parcels of air, the water may be put in motion, as well by the mere dilatation of one of the parcels, as by giving a new force by heat or compreſſion, to the other. And whether this mechanical principle of motion may not prove uſeful in engines, we leave to be conſider'd.

But if, when ſome of the air had been pumped out of the receiver, we removed that double veſſel from the bottle, the external air would, by its weight, ſuddenly depreſs the water in the pipe, till having driven it to the very bottom, it aſcended in numerous bubbles thro' the water, and joined itſelf with the air incumbent on that liquor. 'Twas here obſervable, that all the external air which got into the bottle, did not come in ſuddenly; but, after the firſt irruption, we could perceive, from time to time, new portions of air, leiſurely inſinuate themſelves thro' the pipe into the bottle, and emerge thro' the ſtagnant water in bubbles, that ſucceeded one another very ſlowly; as if the ſpring of the included air, having been once depriv'd of its natural conſtitution, by its late expanſion, could be but gradually reduced to it, by the weight of the atmoſphere, which was ſtill the ſame; or rather, as if between the ſpring of the included, and the preſſure of the external air, balancing each other, there happen'd ſome ſuch thing as is obſervable in ſcales, of which one is too much depreſs'd; whilſt the motion becomes ſlower, as the weights are nearer to an equilibrium.

But our principal deſign in this experiment, was to obſerve, whether the lines made by the water, in its efflux, would retain the ſame figure, notwithſtanding the rarifaction of the air in the upper part of the receiver; and for this purpoſe it is beſt to make the obſervation towards the latter-end of the experiment; becauſe, then, the receiver being moſt exhauſted, the difference made by the change of the denſity of the medium in which the ſtreams of water move, is likely to be beſt diſcern'd. And this convenience we had by our way of making the experiment, that we could obſerve the lines, deſcribed by the flowing water, as the projection thereof grew fainter. But, for want of a large upper receiver, we could not be ſatisfied in the nature of the curve; tho' both Dr. *Wallis*, and my ſelf, found it to be, ſometimes, part of a parabola.

Flat glaſſes broke by the weight of the atmoſphere.

5. We provided a braſs ring of a conſiderable thickneſs, in height three inches; and the diameter of its cavity, as well at the upper as the lower orifice, was ſomething more than three inches. To this ring we ſucceſſively faſten'd with cement, ſeveral round pieces of window-glaſs, and thereby made the ring a kind of receiver, whoſe open orifice we carefully cemented on to the engine; and found, that uſually, at the firſt exſuction, the glaſs plate would be broken inwards, with ſuch violence, as to be ſhatter'd into a great multitude of ſmall fragments; and the irruption of the external air, driving in the glaſs conſtantly, made a loud report, like that of a piſtol.

Without the aſſiſtance of a Fuga vacui.

6. If, inſtead of the braſs ring, above-mention'd, both orifices whereof are equal in breadth, you employ a taller hollow piece of braſs, or latton, ſhaped like a truncate cone; and the two orifices be made very unequal; as if the larger be as wide as that of our braſs ring, and the ſtraiter were leſs than an inch in diameter; and this piece of metal be made uſe of, as that in the preceding experiment, the flat glaſs will be eaſily broken when cemented to the wider orifice: but if the narrower orifice be turned upward, the glaſs thereon, if it be of a due ſtrength, tho' no thicker than the former, notwithſtanding the air is withdrawn from beneath it, will

remain entire: which ſufficiently argues, that nature's abhorrence of a vacuum, is not the cauſe why glaſſes are uſually broken in ſuch experiments, ſince, whether the wider or narrower orifice be uppermoſt, and cover'd, the capacity of the exhauſted veſſel, will be equal; and therefore nature ought to break the glaſs in one caſe as well as the other.

This phenomenon, therefore, is more properly explain'd, by ſaying, that when the wider orifice lies uppermoſt, the glaſs that covers it muſt ſerve for the baſis of a large column of the atmoſphere, which by its great weight may eaſily force thro' the glaſs; whereas, when the ſmaller orifice is uppermoſt, there reſts upon its cover ſo ſlender a pillar of air, as cannot, by its weight, ſurmount the natural coheſion of the parts of the glaſs.

Blown bladders burſt by the ſpring of the air included in them.

7. We ſeldom fail'd of burſting blown bladders in our exhauſted receiver, by tying their necks very cloſely, and keeping them for a pretty while in the glaſs, whilſt the air was exhauſting, and then taking them out again; that the fibres being ſtretch'd and relax'd, and the capacity diminiſh'd by a new ligature, tho' the air were the ſame, and the membrane being not ſo able to yield as before; upon the ſecond exhauſtion of the receiver, they would break far more eaſily than otherwiſe; and ſometimes be oddly lacerated.

A conſiderable weight lifted by the bare ſpring of a little air, included in a bladder.

8. We took a middle-ſized bladder, and having preſs'd out the air, till there remain'd but about a fourth or fifth part, we caus'd the neck to be very ſtrongly ty'd again; and, about the oppoſite part of the bladder, within an inch of the bottom, we ſo ſtrongly tied another ſtring, that it would not be ſlip'd off, by a conſiderable weight hung at it. Then faſtening the neck to the turn-key, we convey'd the bladder, and the weight hanging at it, into a large receiver; when, by plying the pump, the air within the bladder being freed from the preſſure of the air without it, manifeſtly ſwell'd by its own ſpring, and thereby greatly ſhortned the bladder that contain'd it, and lifted up the weight, which exceeded 15 pounds.

After this, we took a large bladder, and having let out ſo much air, that it was left lank, we faſten'd the two ends of it to the upper part of the receiver, and hung a weight from the middle of the bladder; then exhauſting the receiver, as before, tho' the bladder, and this new weight, which ſtretch'd it, reach'd ſo low, that, for a while, we could ſcarce ſee whether it hung in the air or no; yet, at length, we perceiv'd the bladder to ſwell, and concluded it had lifted up its clog about an inch; as was confirm'd by the return of the air into the receiver; upon which, the bladder became more wrinkled than before; and the weight amounting to about 28 pounds, deſcended.

Perhaps this experiment may conduce to explain muſcular motion *.

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* Something has, from this hint, been offer'd, with a very ſpecious and plauſible ſhew of reaſon, to account for muſcular motion; but when thoroughly conſider'd, it fails in ſolving the phenomenon. And the laſt beſt writer on this ſubject, the learned Dr. *Pemberton*, after ſhewing the inſufficiency of all other methods, accounts for it, from that ſubtile medium whereby the great Sir *Iſ. Newton* ſolves various other phenomena of nature.

Glaſs bubbles broke by the ſpring of their own air.

9. A large glaſs bubble, hermetically ſeal'd, being put into the receiver, and the air drawn out ſomewhat more than uſual; tho' I had, ſeveral times, obſerv'd, that ſuch bubbles would not break immediately, upon evacuating the receiver; yet this continu'd ſo long entire, after we had left off pumping, that preſuming it had been blown too ſtrong, I began to deſpair of ſucceſs in the experiment; when, about four minutes after the pump had been let alone, the bubble ſurpriz'd us with breaking ſo violently, by the ſpring of the included air, that the fragments of it were daſh'd every way againſt the ſides of the receiver, and broke to powder.

The external force of the ſpring of uncompreſs'd air upon ſolid bodies.

10. We took the braſs-ring, lately mention'd, whereto were fitted ſome plates of window-glaſs, as covers; and having carefully faſten'd one of them, with cement, to the upper orifice of the ring; and cementing the lower orifice to the engine, ſo that the veſſel, compoſed of metal and glaſs, ſerv'd for a ſmall receiver, we whelm'd another over it that was large and ſtrong; which was alſo faſten'd to the engine with cement, after the uſual manner. By this contrivance, when the pump was ſet on work, the ſmall included receiver muſt have its air withdrawn, while that in the larger could not get out, but by breaking thro' the glaſs; ſo that the internal air of the ſmall receiver being evacuated, the glaſs plate, that made part of it, muſt lie expoſed to the preſſure of the ambient air, ſhut up in the other receiver, without having the former aſſiſtance of the air, now withdrawn, to reſiſt the preſſure; wherefore, at the firſt or ſecond exſuction of the air, included in the ſmall receiver, the glaſs plate was, by the preſſure of the incumbent air, contain'd in the larger one, broken into a hundred pieces, which were beaten inwards into the cavity of the ring.

But to ſhew that there needed not the ſpring of ſo great a quantity of included air to break ſuch glaſſes, we took another roundiſh one, which, tho' wide enough at the orifice, to cover the braſs ring, and the new glaſs plate, that we had cemented on it, was yet ſo low, that it held but a ſixth part of what the large receiver, formerly employ'd, would contain; and having whelm'd this veſſel, which was ſhaped like a tumbler, over the little receiver, and well faſten'd it to the engine with cement, we found, that tho' the external receiver had a great part of its cavity fill'd by that included; yet when this internal one was evacuated, by an exſuction or two, the ſpring of the little air that remain'd, broke the plate into a multitude of fragments.

And becauſe the glaſs plates, hitherto mention'd, ſeem'd not ſo thick, but that the preſſure of the included air might give greater inſtances of its force; inſtead of the ſmall metalline receivers, before employ'd, we took a ſtrong, ſquare bottle of glaſs, able to contain a pint, inverted it, and applied it to the engine, as a receiver; over which we whelm'd, and cemented the large one, formerly mention'd; and ſetting the pump on work, to empty the ſquare bottle, the figure of the veſſel allow'd the preſſure of the air, included in the external receiver, to cruſh it into a great number of pieces.

We alſo took another glaſs, of the ſhape, and about the bigneſs of the former; and having applied it to the engine, as before, and cover'd it with a receiver, that was a little higher than itſelf; upon exhauſting the air, this was likewiſe broken into many fragments, ſome of them very thick; tho' probably the cracks that reach'd thereto, were begun in much weaker parts of the glaſs.

The bottoms, and the necks of both theſe ſquare bottles, were entire; by which it ſeem'd probable, that the veſſels had been broken, by the preſſure of the air againſt the ſides, which were not only thinner than the other parts, but expos'd a larger ſuperficies to the lateral preſſure of the air, than to the perpendicular. We obſerv'd, in one of theſe experiments, that the veſſel did not break preſently, upon the laſt exſuction of the included air, but a conſiderable time after.

To confirm that it is the ſpring of the air, in the external receivers, that breaks the glaſſes, and to prevent ſome ſcruples, we apply'd a plate of glaſs, like thoſe formerly mention'd, to the braſs ring; but, in the cementing of it on, we placed, in the thickneſs of the cement, a ſmall pipe of glaſs, about an inch long, whoſe cavity was not ſo big as that of a ſtraw, and which, being left open at both ends, might ſerve for a little channel, for the air to paſs thro', from the external receiver to the internal; over this we whelm'd a ſmall receiver, and then, tho' we work'd the pump much longer than would have been neceſſary, if the little pipe had not been made uſe of, we found the internal receiver continue entire; becauſe the air, whoſe ſpring ſhould have broken it, having liberty to paſs thro' the pipe, and conſequently to expand itſelf, into the place deſerted by the air pump'd out, thereby weaken'd its ſpring too much for that purpoſe.

But, either the pipe muſt be made bigger, than that lately mention'd, or the exſuction of the air muſt not be ſudden, by the pump; otherwiſe the plate of glaſs may be broken, notwithſtanding the pipe; becauſe the air contain'd in the external receiver, having a force much greater than is neceſſary to break ſuch a plate, it may well happen, as I have ſometimes found it, that if the air be haſtily drawn out of the internal receiver, that which ſhould ſucceed in its room, cannot get faſt enough out of the external receiver, thro' ſo ſmall a pipe; whilſt the air remaining in the ſame, will yet retain a ſpring ſtrong enough to break the glaſs. Thus, ſometimes, when at the flame of a lamp, glaſs bubbles are blown with ſlender ſtems; if they be ſuddenly remov'd out of the flame, they either break, if cool'd too faſt; or are compreſs'd inwards, if they long retain the ſoftneſs given them by fuſion. For the air in the bubble, being exceedingly rarified and expanded, whilſt the glaſs is kept in the flame, and coming to cool haſtily, when remov'd from thence, loſes, upon refrigeration, the ſpring which the heat had given it; and ſo, if the external air cannot preſs in faſt enough thro' the too ſlender pipe, a ſufficient quantity of air will not get in to reſiſt the preſſure of the atmoſphere; and therefore, if this preſſure find the bubble yet ſoft, it will preſs it a little inwards,

wards, and either flatten it, or make a dent, tho' the orifice of the pipe be left open.

Mercury riſes no higher by ſuction, than the weight of the atmoſphere impels it.

Fig. 47.

11. We took a braſs pipe, bent like a ſiphon, and fitted at the bigger end with a ſtop-cock, *&c.* and to the ſlender end of this we faſten'd with cement, the upper end of a cylindrical glaſs pipe, about 50 inches long, open at both ends, and having the lower plung'd into a veſſel of ſtagnant quick-ſilver, whoſe upper ſuperficies reach'd conſiderably higher than the immers'd orifice of the glaſs tube; then cauſing the pump to be work'd, the air was, by degrees, drawn out of the ſiphon, and conſequently out of the glaſs tube that open'd into it; and the ſtagnant mercury proportionably impell'd up into the glaſs tube, till it had attain'd to its due height, which exceeded not 30 inches. And then, tho' there remain'd in the upper part of the pipe, above 20 inches unfill'd with quick-ſilver, we could not, by further pumping, raiſe it higher.

Hence it appears, that the fancied power of nature, to prevent a vacuum, has its bounds; and thoſe depending upon the ſpecific gravity of the liquor, to be rais'd by ſuction. For ſubſtituting, inſtead of the ſtagnant mercury, a baſon of water; and inſtead of the many ſtrokes, in vain employ'd, to raiſe the quick-ſilver above the height juſt mention'd, making ſcarce one exſuction, which only, in part, emptied the ſiphon; yet the water, upon opening the ſtop-cock, was not only impell'd to the very top of the glaſs tube, but continu'd running, for a conſiderable time, thro' the ſiphon, and thence fell upon the plate of the engine; ſo that it appear'd ſtrange to thoſe, who knew not the reaſon of it, that the water ſhould run very briſkly, of its own accord, out of the leg of a ſiphon; which, perhaps, was not above a quarter ſo long as the other. I muſt not here omit, that tho' ſometimes, in the *Torricellian* experiment, I have obſerv'd the mercury to ſtand at 30 inches, and now and then above it; yet the height of the mercury in our glaſs tube, appear'd not to reach full 29 inches and a quarter. But, conſulting the barometer, I found the quick-ſilver at 29 inches, and one eighth; which, probably, would have been the very height of that rais'd by the engine, had it been freed from bubbles.

Hence we may conclude, that ſuction will elevate liquors in pumps, no higher than the weight of the atmoſphere is able to raiſe them; ſince the cloſeneſs requiſite in the pump of our engine, makes it very unlikely, that a more accurate ſuction can be effected by an ordinary pump.

Tho' the exhauſting ſiphon, uſed in this experiment, may be eaſily conceiv'd by an attentive inſpection of the figure; yet, becauſe I frequently employ it in pneumatical experiments, it is proper to intimate, once for all, that tho' the bended pipe itſelf, may be, on ſome occaſions, more conveniently made of glaſs, for the ſake of tranſparency; yet, for the moſt part, we choſe to employ pipes of braſs, becauſe the others are ſo very ſubject to break; that it is convenient to make the longer leg of the ſiphon a little larger at the bottom, than the reſt of the pipe uſually needs to be, that it may the more commodiouſly admit the ſhank of a ſtop-cock, which is to be very carefully inſerted with cement; by ſeaſonably turning and returning whereof, the paſſage between the engine and the veſſel to be exhauſted, is to be open'd and ſhut; and laſtly, that tho' we ſometimes immediately apply the braſs ſiphon to the engine, by cementing the external ſhank of the ſtop-cock to the orifice of the little pipe, thro' which the exſuction of the air is made; yet the bended pipe alone, is ſo apt to be looſen'd by the motion of the engine, and the turning of the ſtop-cock, that, for the moſt part, we uſe a ſiphon conſiſting of a braſs pipe, a ſtop-cock, and a glaſs 8 or 10 inches high, and of ſome ſuch ſhape as is expreſs'd in the figure; for by this means, tho' the exhauſtion is longer in making, yet it is more ſecurely and uninterruptedly carried on; becauſe of the ſtability which the breadth of the lower orifice of the glaſs gives to the whole inſtrument. Beſides, not only the ſiphon is thus much lengthen'd, but we may commodiouſly place a gage in the glaſs part of this compounded ſiphon, to ſhew, from time to time, how far the air is drawn out of the veſſel to be exhauſted.

Liquors aſcend to different heights by ſuction, according to their ſpecific gravities.

12. I caus'd to be made, and inſerted to the ſhorter leg of the abovemention'd ſiphon, a ſhort pipe, which branch'd itſelf equally to the right hand and to the left; ſo that I might exhauſt two glaſs tubes at the ſame time, and prevent any ſuſpicion, that the engine was not equally applied to both. This additional braſs pipe, being carefully cemented into the ſiphon, to each of its two branches were well faſten'd, with the ſame cement, a cylindrical glaſs of about 42 inches in length; the lower orifice of one of theſe glaſſes being immers'd in a veſſel of ſtagnant mercury, and that of the other in a veſſel of water; when care was taken, that as the tubes were choſen near of a ſize, ſo the ſurfaces of the two different liquors ſhould be near of a height. This being done, we began to pump warily and ſlowly, till the water in one of the pipes was elevated about 42 inches; and then meaſuring the height of the quick-ſilver in the other pipe above the ſurface of the ſtagnant mercury, we found it to be almoſt three inches; ſo that the water was about 14 times as high as the quick-ſilver. And, to proſecute the experiment further, we very warily let in a little air to the exhauſting ſiphon, and ſaw the two fluids proportionably deſcend; till turning the ſtop-cock, when the water was about 14 inches high, we thereby kept them from ſinking any lower, before we had meaſur'd the height of the quick-ſilver, which we found to be about one inch.

But we obſerv'd, that the quick-ſilver, for the moſt part, ſeem'd to be a very little higher, than the proportion of 1 to 14 required; and accordingly, I had long before, by particular trials, found, that, tho' 14 and 1 be the neareſt of ſmall integer numbers, that expreſs the proportion between the ſpecific gravities of mercury and water; yet the former is not quite ſo heavy as this proportion ſuppoſes.

This experiment evidently ſhews, that the fluids roſe by the weight of the air, and leaves no pretence of a *Fuga vacui*. It may alſo be made uſeful to eſtimate the different gravities of liquors; for which purpoſe,

poſe, I cauſed the afore-mention'd glaſs-pipes, to have their ends plung'd, the one in freſh water, and the other in ſome impregnated with a large proportion of ſea ſalt; and found, that when the freſh water was rais'd to about forty-two inches, the ſaline ſolution had not fully reach'd to forty.

But, to make the diſparity more evident, I prepar'd an unuſual brine, by ſuffering ſea-ſalt to diſſolve in the moiſt air: and, having apply'd this liquor, and freſh water to the two pipes, and proceeded after the former manner; we found, that when the pure water was elevated to near forty-two inches, the liquor of ſea-ſalt wanted about ſeven inches and one fourth of that height; and when the water was made to ſubſide to the middle of its pipe, the ſaline liquor in the other pipe was between three and four inches lower than that. I alſo took fair water, and a liquor made of the ſalt of pot-aſhes ſuffer'd to run *per deliquium*, and proceeding as before, found, that when the common water was about forty-two inches high, the ſolution wanted of thirty inches; and when the water was made to ſubſide to the middle of its tube, the other liquor was between ſix and ſeven inches lower.

The heights whereto water and mercury may be rais'd by the ſpring of the air.

13. We took a ſtrong glaſs bottle, that would contain above a pint, and having in the bottom of it lodg'd a convenient quantity of mercury, we pour'd on it a greater quantity of water; and providing two ſlender glaſs pipes, open at both ends, we ſo plac'd and faſten'd them cloſe by cement, that the ſhorter of the pipes had its lower orifice immers'd beneath the ſurface of the quick-ſilver, and the longer reach'd not quite ſo low as that ſurface, and ſo was immers'd but in the water. This done, we convey'd the bottle into a proper receiver, and having begun to pump out the air; we took notice to what heights the quick-ſilver and water were impell'd up in their reſpective tubes, on which we had before made marks; and found, that when the quick-ſilver was impell'd up to two inches, the water was rais'd to about twenty-eight; and when the quick-ſilver ſtood at about one inch, the water ſtood at about fourteen.

And the heights whereto they will ſubſide upon withdrawing it.

14. We convey'd into a fitly ſhaped receiver two glaſs pipes very unequal in length; but each of them ſealed at one end: the ſhorter tube was fill'd with mercury, and inverted into a ſmall glaſs jar, wherein a ſufficient quantity of that fluid had been before lodg'd, the longer pipe was fill'd with common water; and inverted into a larger glaſs, which likewiſe contain'd a fit proportion of the ſame liquor. Then the receiver being cloſely cemented to the engine, the air was pump'd out for a pretty while before the mercury began to ſubſide; but when it was ſo far withdrawn, that its preſſure could no longer keep up a mercurial cylinder of that height, the quick-ſilver began to ſink; the water in the other tube, tho' three times as long, ſtill retaining its full height. But when the quick-ſilver was fallen to between three and four inches above the ſurface of that in the veſſel, the water alſo began to ſubſide; but ſooner than according to the laws of ſtatics it ought to have done: becauſe many aerial particles emerging from

the body of the water to the upper part of the glaſs, by their ſpring concurr'd with the gravity of the water to depreſs this liquor. And ſo when the quick-ſilver was three inches above the ſtagnant mercury, the water in the pipe was fallen ſeveral inches beneath forty-two; and ſeveral beneath twenty-eight, when the mercury had ſubſided an inch lower. But after the pump had been ply'd, to free the water from the latent aerial bubbles, we let in the external air; and having thereby impell'd both the fluids up again into their pipes, and remov'd the receiver; we took them both out, to free them from the air, and fill'd each of them with a little of their reſpective ſtagnant liquors; then inverting them again into their proper veſſels, we repeated the experiment, and found it to require more pumping than before, to make the liquors begin to ſubſide: ſo that when the mercury was fallen to three inches, or two, or one, the water ſubſided ſo near to the heights of forty-two, twenty-eight, or fourteen inches, that we ſuppos'd the little differences which appear'd between the ſeveral heights of the quick-ſilver, and fourteen times as great heights of the water, proceeded from ſome aerial corpuſcles yet remaining in the water, and by their ſpring, when once they had emerg'd, promoting the depreſſion of it.

The greateſt height to which water can be rais'd by attraction, or ſucking-pumps. *Fig.* 48.

15. Having procured ſeveral tin pipes above an inch in bore, very carefully ſolder'd together, to make one whole tube, about thirty-two feet long; and cas'd it over firſt with cement, and then with plaiſter of *Paris*; we very carefully cemented a ſtrong pipe of glaſs, between two and three feet in length to the upper part of it; and to the upper end of this pipe, by means of cement and a ſhort elbow of tin, we very cloſely faſten'd another pipe of the ſame metal, conſiſting of two pieces making a right angle; whereof the upper part was parallel to the horizon, and the other, which lay parallel to the glaſs pipe, reach'd down to the engine that was placed on the flat roof of a houſe thirty feet high from the ground, and was to be cemented to the lower end of this deſcending part of the pipe, whoſe horizontal leg reſted upon a piece of wood nail'd to the rails on the top of the building: the tube, alſo, was kept from ſhaking by a board faſten'd to the ſame rails, with a deep notch for it to be inſerted in.

This apparatus being made, and the whole tube, with a pole to ſuſtain it, erected along the wall, faſten'd there, and the deſcending pipe carefully cemented on to the engine; there was placed under the bottom of the long tube a convenient veſſel, whereinto ſo much water was pour'd, as reach'd far above the orifice of the pipe; and providing, that the veſſel might ſtill be kept competently full, we, at length, rais'd the water to the middle of the glaſs pipe; but not without numerous bubbles, made by the air conceal'd in the pores of the water, which, for a time, kept a kind of foam upon the ſurface of it. And finding the engine, and tube as ſtaunch as could be expected; I thought fit to try what was the utmoſt height, to which, water could be elevated by ſuction: and therefore, tho' the pump ſeem'd to have been ſufficiently ply'd already; yet, for further ſatisfaction, when the water was within a few inches of the top of the glaſs, I caus'd twenty exſuctions more to be ſuddenly made. And, having taken notice where the ſurface reſted, we meaſured the height of the cylinder of water, and found it thirty-three feet, and about ſix inches; the barometer then ſtanding at twenty-nine inches, and between two and three eighths of an inch. Now, ſuppoſing the ſpecific gravity of water, to that of quick ſilver, as 1 to 14; the height of the water ought to have been thirty-four feet, and about two inches; that is, about eight inches more than we found it. But then, I formerly noted, that the proportion betwixt mercury and water, is not altogether ſo great; and therefore, in ſo tall a cylinder as ours was, the difference muſt be conſiderable. If therefore, inſtead of making an inch of quick-ſilver equivalent to fourteen inches of water, we abate a quarter of an inch; which is but a fifty-ſixth part of the height of the water; this abatement being repeated twenty-nine times and one quarter, will amount to ſeven inches, and above a quarter; which added to the former height of the water, thirty-three feet, ſix inches, will make thirty-four feet, and above an inch: ſo that the difference between the height of the mercury, ſuſtain'd by the weight of the atmoſphere in the barometer, and that of the water, rais'd, and ſuſtain'd, by the preſſure of the ſame in the long tube, did not appear to differ more than an inch or two, from the proportion they ought to have, according to their ſpecific gravity: nor could we, by obſtinately plying the pump, raiſe the water higher.

This experiment, being ſoon repeated, in my abſence, by Dr. *Wallis*, Dr. *Wren*, and Dr. *Millington*; they preſently after aſſured me, that the greateſt height, whereto they could raiſe the water, was thirty-three feet and a half; and, as it happen'd, within leſs than an hour before, I had obſerved the barometer, to ſtand ſomewhat below twenty-nine inches, and three eighths; when, now, conſulting the ſame inſtrument again, the mercury appear'd to be riſen a little higher. Hence appears the impoſſibility of making water paſs over the higheſt mountains by the help of inflected pipes, and ſuction. For, if the water be to riſe above thirty-five, or thirty-ſix feet, a ſucking-pump will not, ordinarily, here in *England*, ſuffice for that purpoſe.

An elaſtic body bent in the exhauſted receiver.

16. To try whether the air contributes to the elaſticity of bodies, we took a piece of whale-bone, of a convenient length, and having faſten'd one end of it into a thick heavy trencher, to be placed on the plate of the engine; to the other end we tied a weight, whereby the whale-bone was moderately bent, which reach'd down to a flat body placed under it, ſo that if the ſpring were but a little weaken'd, the weight muſt either reſt upon, or touch the horizontal plane; or if, on the other ſide, the ſpring ſhould grow ſenſibly ſtronger, it might be eaſily perceived, by the diſtance of the weight, which was ſo near the plane, that a little increaſe of it muſt be viſible. Theſe things we conveyed into the receiver, and took care to ſhake the engine as little as poſſible, that the weight might not hit againſt the body which lay under it; or, we be hinder'd from diſcerning, whether it were depreſſed by the bare extraction of the air. And, when the air had been well pumped out, I watched attentively, whether any notable change, in the diſtance of the weight from the plane, would happen upon

its being let in again; for the weight was then at reſt: and the returning air flowing in much faſter than it could before be drawn out, this ſeem'd the likelieſt time to diſcover, whether the abſence of the air had, ſenſibly, alter'd the ſpring of the whale-bone. But, tho' the experiments were made more than once, I could only ſatisfy myſelf, that the depreſſion, or elevation, of the weight, owing to the mere change of the ſpring, was not very conſiderable; for I do not think myſelf ſure, that I perceived any at all: tho', ſometimes, when the receiver was well exhauſted, the weight ſeem'd to be a little depreſs'd; yet this, I thought, might well be aſcribed to the abſence of the air, not conſider'd as a body that had any thing to do directly with the ſpring, but as a body that had ſome gravity; whereby it made the medium, wherein the experiment was try'd, contribute to ſupport the weight that bent the ſpring; which weight, when the air was abſent, muſt have its gravity increaſed, by as much weight, as a quantity of the exhauſted air, equal to it in bulk, amounts to.

To make gages for eſtimating how far the receiver is exhauſted.

17. The air, being inviſible, it is not always eaſy to know, whether it be ſufficiently pumped out of the receiver, to be exhauſted; we, therefore, thought it very convenient to have ſome inſtrument within the receiver, that might ſerve for a gage, or ſtandard, whereby to judge when it was ſufficiently evacuated. The firſt attempt, made to this purpoſe, was by means of a bladder, very ſtrongly tied at the neck; after having had only ſo much air left in its folds, as might fully diſtend it, when the receiver was very well exhauſted. And this way, in ſome caſes, is uſeful; but, in others, a bladder takes up too much of the receiver, and hinders the objects from being obſerv'd on all ſides.

Another ſort of gage we made with quick-ſilver, pour'd into a very ſhort pipe, which was, afterwards, inverted into a little glaſs of ſtagnant quick-ſilver, as in the *Torricellian* experiment. For this pipe being but a very few inches long, the mercury in it would not begin to deſcend, till a very great proportion of air was pumped out of the receiver; becauſe, till then, the ſpring of the remaining air would be ſtrong enough to ſuſtain ſo ſhort a cylinder of mercury. And this kind of gage is no bad one. But, becauſe it cannot eaſily be ſuſpended, and the mercury in it is apt to ſhake by the motion of the engine, another was ſubſtituted in its place, conſiſting of a kind of ſiphon, to the ſhorter leg whereof belonged a large glaſs-bubble.

But, none of theſe gages having the conveniences, that ſome of our experiments require; I deviſed another, after the following manner.

Fig. 47.

Take a cylindrical pipe of glaſs, ſix, eight, ten, or more inches in length, and not ſo thick as a gooſe-quill; and, by the flame of a lamp, melt it, but not too near the middle, and make it into a ſiphon; the legs whereof are to be parallel, and as near to each other, as poſſible. In one of theſe legs, uſually the longer, leave at the top, either half an inch, or a whole inch, more or leſs, according to the length of the gage, or the deſign of the experimenter, of air in its natural ſtate; and fill the reſt of the longer leg, and as great a part of the ſhorter as ſhall be thought proper, with quick-ſilver. This done, there may be marks placed on the outſide of the longer, or ſeal'd leg, whereby to meaſure the expanſion of the air included therein.

This inſtrument, being convey'd into a receiver, and the air very diligently pump'd out, notice muſt be taken, to what part of the gage the mercury is depreſs'd, that we may know, when the mercury ſhall, afterwards, be driven ſo far, that the receiver, wherein the gage is placed, is well exhauſted. And if it be deſired to know, more accurately, what ſtations of the mercury, in the gage, are anſwerable to the degrees of the rarifaction of the air in the receiver; this may be gain'd by letting in water, as often as is neceſſary, into a receiver, whoſe entire capacity is firſt meaſured; and in which there might be marks made to ſhew when the water to be let in, ſhall have fill'd a fourth, a half, *&c.* of the cavity. For if, when the quick-ſilver in the gage, is depreſſed to a certain mark, you let in water, which appears to fill a fourth part of the receiver; you may conclude, that about one fourth part of the air was pump'd out; or that a fourth of the ſpring of the whole included air was loſt. And if the water either falls conſiderably ſhort of, or exceeds the quantity expected; you may the next time, let in the water, either after the mercury has a little paſs'd the former mark, or a little before it is arrived at it. And when once you have, this way, obtain'd one long, and accurate gage, you may divide others by the help of this, placed with them in a ſmall receiver: when, the mercury in the former, being depreſs'd to any determinate diviſion, obtain'd by obſervation; you may, thence, conclude, how much the air, in the receiver, is rarify'd; and conſequently, by taking notice of the place where the mercury reſts in the other gages, determine what degree of exhauſtion, in a receiver, is denoted by that ſtation of the mercury.

That leg of the gage which includes the air, may be ſeal'd up, either before the pipe is bent into a ſiphon; or, which is much better, by firſt drawing out that end of it you deſign ſhall be ſeal'd, to a ſhort, and very ſlender thread: then, having made the tube into a ſiphon, pour into the leg, which is to remain open, as much quick-ſilver as you judge convenient, which will riſe to an equal height in the other leg; and, by gently inclining the ſiphon, you may pour the ſuperfluous mercury out of it, if there be any; and when there is an inch, or the proper ſpace, unfill'd with mercury next the end that is to be cloſed; and the reſt of that leg, and as much of the other as is neceſſary, fill'd with quick-ſilver; you may, by keeping the ſiphon in the ſame poſture, and warily applying the ſlender apex above-mention'd, to the upper part of the flame of a lamp, blown horizontal, conveniently ſeal it up.

But there are ſome experiments, wherein it is not neceſſary that the receiver ſhould be fully exhauſted; but, rather, that the degrees of the air's rarifaction ſhould be well meaſured. And, in many caſes, we may uſe gages, ſhaped like thoſe hitherto deſcribed, made as long as the receiver will admit, and furniſh'd, inſtead of quick-ſilver, either with tinged Spirit of wine; or elſe the tincture of red roſe-leaves, drawn with common water, and heightned with a little ſpirit of vitriol. For the lightneſs of theſe liquors, in compariſon of quick-ſilver, will allow the expanſion of the air, included in the gage, to be very manifeſt; tho', perhaps, a quarter of the air be not pump'd out of the receiver.

We may alſo, in ſuch caſes, and where the receiver is ſufficiently large, and not to be quite exhauſted, make uſe of a mercurial gage, differing from the former in this, that the ſhorter leg need not be above an inch, or half an inch long, before it widens into a bubble, about half an inch, or an inch in diameter; and having, at the upper part, a very ſhort and ſlender open pipe, whereat the air may get in and out: and here we need not include ſo much air, as, otherwiſe, would be requiſite, at the top of the longer leg; becauſe the mercury, in the ſhorter, cannot, by reaſon of the breadth of the bubble, into which the expanſion of the air drives it, be conſiderably raiſed; whereby the degrees of the included air's rarifaction become very viſible.

An eaſy way to make the preſſure of the air ſenſible to the touch.

18. I cauſed a hollow ſtrong piece of braſs to be made, two or three inches high, opening, at both ends, in orifices circular and parallel, but not equal; which, being cemented, as a ſmall receiver, to the engine; whoever doubted the preſſure of the air to be conſiderable, needed only lay the palm of his hand upon the upper orifice, and preſs it cloſe thereto: for, upon withdrawing, by a ſingle ſtroke, the greateſt part of the preſſure of the internal air, that, before counter-balanced the external; the hand, being left alone, to ſupport the weight of the atmoſphere, would be preſs'd inwards very forcibly; eſpecially, if, by a ſecond ſtroke of the pump, the little receiver were farther exhauſted: and this preſſure continues, till the air be re-admitted into the receiver. If a more ſenſible conviction be deſir'd, it is eaſy to give it, by turning the larger orifice uppermoſt, and proceeding, as before; but this ought not much to exceed two inches and a half in diameter, leſt the great weight of the air ſhould break, or conſiderably hurt the hand: as I once much endanger'd my own, thro' miſtake of the pumper, who fell to work, while I held it upon the orifice of a veſſel too large in diameter.

Mercury ſubſiding in the Torricellian *tube to a level with the ſtagnant by extracting the air.*

19 A barometer being included in a receiver, made of a long bolthead, with the lower part of the ball cut circularly off; upon the firſt exſuction of the air, the quick-ſilver, that before ſtood at twenty-nine inches, would fall, and reſt, at nine or ten inches; and, in about three ſtrokes more, it would be brought quite down to the level of the ſtagnant quickſilver, and ſomewhat below: but the air, being let into the receiver, the mercury would be impell'd up ſlow, or faſt, as we pleas'd, to the former height of twenty-nine inches.

If the air were ſuffer'd to go haſtily out of the receiver, the mercury would, at the very firſt ſtroke, deſcend, till it reached within an inch or two of that in the veſſel; tho' it would, preſently after a few riſings and fallings, ſettle at the height of nine or ten inches, till the next ſtroke brought it down lower.

And if, when the mercury was re-impell'd up to its due height, inſtead of rarifying the air, it were a little compreſs'd; the quick-ſilver would be eaſily made to riſe an inch, or more, above the former ſtandard of twenty-nine inches.

We, alſo, took a glaſs-tube, ſeal'd at one end, much ſhorter than the due length, and having fill'd it with mercury, and inverted it into a veſſel of ſtagnant mercury, we placed all in the former receiver; where the mercurial cylinder, for want of the requiſite height, remain'd totally ſuſpended; but upon the firſt, or ſecond ſtroke, ſubſided, and, after two or three more, fell to a level with the ſtagnant mercury, or a little below it: and upon the letting in the air, it would be again impell'd to the very top of the tube, bating an aerial bubble, which ſeem'd to come from the mercury itſelf; and was ſo little, as not to be at all diſcernible, but to a very attentive eye.

In ſmall and large open tubes, when no Fuga vacui *can be pretended, the weight of water raiſes quick-ſilver to an equal height.*

20. Into a very large glaſs-tube, hermetically ſeal'd at one end, and about two feet and a half in length, we pour'd quick-ſilver, to the height of three or four fingers; then we took two cylindrical pipes, of very unequal bores, and open at both ends, and plunged the lower ends of both into the quick-ſilver; faſtening them to the former tube, that they might not be moved out of their poſture; in which the convex ſurface of the mercury, in both, ſeem'd almoſt to lie in a level; the tube, alſo, being placed, perpendicularly, in a frame: then, by the help of a funnel, we pour'd water, by degrees, in at the top of the tube; and obſerv'd that, as the water gravitated, more and more, upon the ſtagnant mercury; ſo the included mercury roſe equally, in both the pipes; till the tube, being almoſt fill'd with water, the mercury appear'd to be impell'd, and ſuſtain'd in both, at the height of about two inches above the ſurface of the ſtagnant quick-ſilver. And, having caus'd about half the water, in the large tube, to be ſuck'd out at the top; we obſerv'd the quick-ſilver, in both the others, to ſubſide uniformly, and to re-aſcend alike upon the re-affuſion of the water.

We, alſo, took a very wide tube of glaſs, a foot long, and pour'd into it a convenient quantity of quick-ſilver; then we took two pipes, of an equal length, but unequal bores, as before; and theſe, being fill'd with quick-ſilver, as in the *Torricellian* experiment, were let down into the tube, and unſtopp'd, under the ſurface of the ſtagnant mercury: when, that in the pipes, falling to its wonted ſtation, and reſting there, we pour'd into the tube about a foot height of water, whereby the quick-ſilver appear'd equally impell'd above its ſtation, and ſuſtain'd there, in both the pipes; and, upon withdrawing ſome of the water, it began to ſubſide alike, as to the ſenſe, in both: and water, being a ſecond time pour'd down into the tube, the mercury, in both pipes, roſe uniformly, as before. By which, and the former experiment, it appears, that a gravitating liquor, as air, or water, may impel, or ſuſtain mercury, at the ſame height, in tubes of very different capacities; and that liquors balance each other, according to their altitude, and not barely according to their weight. For, in the laſt experiment, the additional cylinder of one inch of mercury, was manifeſtly rais'd, and kept up by the water incumbent on the ſtagnant mercury. And the ſame parcel of water counterpois'd, in the different pipes, two mercurial cylinders, which, tho' of the ſame altitude, were very unequal in weight.

The height whereat pure mercury, and mercury amalgamated with tin, will ſtand in barometers.

21. Amalgamating mercury, with a convenient proportion of pure tin, that the mixture might not be too thick, we therewith fill'd a cylindrical pipe, ſeal'd at one end, and of a fit length; and then inverted it into a little glaſs, furniſh'd with the like mixture. The event was, that the amalgam did not fall down to 29, but ſtopt at 31 inches, above the ſurface of the ſtagnant parcel. Hence it appears, that the height of the liquor, ſuſpended in the *Torricellian* tube, depends ſo much upon its equilibrium, with the external air, that it may be varied as well by a change of gravity in the ſuſpending liquor, as we formerly ſaw it might by an alteration in the atmoſphere.

It might be worth while to try, by comparing the height of the amalgam to what it ought to be by the ſpecific gravities of the mercury, and the tin mix'd in a known proportion, whether theſe metals penetrate each other, in the ſame manner as copper and tin have been obſerv'd to do; when being melted down together, they make a more cloſe and ponderous body than their reſpective weights ſeem'd to require.

To make portable barometers.

Fig. 49.

22. We took a hollow cylinder of glaſs, ſeal'd at one end, and 4 or 5 feet in length; and by the flame of a lamp, bent it after the manner of a ſiphon, one of the legs whereof is 3 or 4 times longer than the other; whence the ſhorter leg may ſerve, inſtead of the veſſel, uſually employ'd to contain the ſtagnant mercury. To fill this, take a ſmall glaſs funnel, with a long and ſlender ſhank, ſo that it may reach 3 or 4 inches, or farther, into the ſhorter leg of the barometer; and by the funnel, pour into the ſhorter leg, as much mercury as may reach about 2 or 3 inches, in both legs; then ſtopping the orifice with your finger, and ſlowly inclining the tube, the mercury, in the longer leg, will fall to the ſeal'd end, and the air that was there before, paſs by, and give it room. The mercury, in the ſhorter leg, which ought to be held uppermoſt, will, by the ſame inclination of the tube, fall towards the orifice; but being, by the finger, kept from falling out, if you ſlowly erect the glaſs again, and then ſtop it, as before, the mercury will paſs out of the ſhorter leg into the longer, and join with that which was there before; and if all the mercury do not ſo paſs, the orifice is to be ſtopt again with the finger, and the tube inclined as formerly. This done, the tube is to be erected, and by the help of the funnel, more mercury is to be pour'd in; and the ſame proceſs of ſtopping the orifice, inclining the tube, *&c.* is to be repeated, till all the mercury, pour'd into the ſhorter leg, be brought to join with that in the longer; and then the open leg is to be furniſh'd with freſh mercury; obſerving, that the nearer the longer leg comes to being fill'd, the leſs you muſt raiſe it, from time to time, when you pour mercury into the ſhorter; as alſo, that when the longer leg is

quite full of mercury, you need not pour in any more, if the longer much exceed a yard; becauſe, upon erecting the tube, there will ſubſide from the taller leg into the other, a conſiderable quantity of mercury. And to free it from bubbles, you muſt, once more, ſtop the orifice with the finger, and incline, and re-erect the tube ſeveral times, till you have thereby brought moſt of the ſmaller bubbles into a ſingle large one; then making this paſs leiſurely, two or three times, from one end of the tube to the other, it will unite all the ſmall bubbles to itſelf: and this may, afterwards, by one inclination more of the tube, be made to paſs into the ſhorter leg, and thence into the free air.

But there is another ſort of funnels, with which, if ſkilfully uſed, the bended tubes of our portable barometers, may be very expeditiouſly fill'd. For, if the ſlender part of the funnel be bent in an obtuſe angle, and ſo long, that the part which is to go into the ſhorter leg of the ſiphon, may reach to its flexure; you may, by holding the tube ſo, that the ſealed end be ſomewhat ~~lower~~ than the other, and by pouring in mercury at this obtuſe end of the angular funnel, eaſily make it run over the flexure, into the longer leg of the ſiphon; provided you now and then, as occaſion requires, erect and ſhake the tube, to help the mercury to get by the air, and expel it.

We accompliſh'd another part of our deſign by means of a piece of wood, ſomewhat longer than the tube, and conſiderably broader in the lower part, than in the upper, to receive the ſhorter leg of the ſiphon. In ſuch a piece of wood, which was about an inch thick, we caus'd ſuch a channel to be made, that our ſiphon might be placed in it ſo deep, that a flat piece of wood might be laid on it, without touching the glaſs; ſo that this piece of wood may ſerve for a cover to defend the glaſs, to be put on when the inſtrument is to be tranſported; and taken off again, when 'tis to be hung up for obſervation; the channel'd piece of wood ſerving both for part of a caſe, and for an entire frame; which may, for ſome uſes, be a little more commodious, if the cover be join'd to the reſt of the frame, by two or three little hinges and a haſp, whereby the caſe may be readily open'd and ſhut, at pleaſure.

The third thing we propos'd, is not ſo eaſy as the ſecond; nor have we yet had opportunity to try whether the way we made uſe of, will hold, if the barometer be tranſported into very remote parts; tho', by ſmaller removes, we found reaſon to hope 'twill ſucceed in greater.

The grand difficulty was, to prevent the ſpilling of the mercury; for, the upper part of the tube being deſtitute of air, if the quick-ſilver, by the motion of the inſtrument, be made to vibrate, it will hit ſo violently againſt the top of the glaſs, as to break it. To obviate this inconvenience, we incline the tube, till the mercury be impell'd to the very top of it; when yet there will remain a competent quantity in the ſhorter leg of the glaſs, if that be not too ſhort; then the remaining part of the ſhorter leg, is to be fill'd up either with water or mercury, and the orifice of it very carefully ſtopp'd with cement: by this means, the mercury in the longer leg, having no room to play, cannot ſtrike with violence againſt the top of the glaſs.

When the inſtrument is to be tranſported, the height of the mercurial cylinder being taken for that place, day, and hour, and compared with that of another good barometer, which is to continue in the ſame place; as much of the channel, as is unpoſſeſs'd by the glaſs, may be ſtuffed with cotton, or the like; and ſome of the ſame matter may be put between the reſt of the frame, and the cover, which ought to be well bound together. And when the inſtrument is arrived at the place deſigned, the water, that is added, may be taken off again, by pieces of ſpunge, linen, *&c.* but, if inſtead of water, mercury be employ'd, it ought to be taken out, till you have juſt the weight that was put in. The chief uſe of this barometer is, by keeping a diary of the heights of the mercury herein, and comparing them with thoſe in the barometer, that was not remov'd, to diſcover the agreement or difference of the weight of the atmoſphere, in diſtant places. The ſtructure of this inſtrument, alſo, fits it to be ſecurely let down into wells, or mines; to be drawn up to the top of towers, and other elevated places; and, perhaps, by a convenient addition, ſuch barometers may ſhew very minute alterations of the atmoſphere's preſſure.

Whether this barometer, furniſh'd, at its upper end, with a ball and ſocket, and at the lower, with a great weight, may be ſerviceable at ſea, notwithſtanding the rolling of a ſhip, I have not try'd; but it may, at leaſt, be apply'd in flat calms, to ſhew the weight of the atmoſphere in different climates upon the ſea; which may, perhaps, prove uſeful to navigators, by enabling them to foretel the end of the calm. Beſides, having one of theſe inſtruments ready, whenever they come on ſhore, they can preſently take notice of the gravity of the atmoſphere, in that place; and this, perhaps, compared with other obſervations, may, in time, help them to gueſs where they are, and to foreſee ſome approaching changes of weather.

Mercury in a barometer will be kept ſuſpended higher at the bottom than at the top of a hill.

23. Two perſons, whom I employ'd, found the mercury, in a portable barometer, fall a little, as they aſcended a hill; at the top whereof they let the fluid ſettle, and carefully noted the place whereat it reſted, which was one quarter of an inch beneath its former ſtation; tho' the hill was not high, and the air and wind ſeem'd, to them, much colder at the top, than below. And as they deſcended, they obſerved that the mercury roſe gradually.

The weight of the air will ſuſtain the mercury in the barometer, tho' it preſs thereon but at a very ſmall orifice.

24. Take the bent tube, mention'd in the twenty-ſecond experiment, and inclining it, till the greateſt part of the mercury paſs from the ſhorter leg into the longer, the upper-end of the ſhorter leg, may, by the flame of a lamp, be drawn out ſo ſlender, that its orifice ſhall not be above an eighth or tenth part as big as 'twas before. This being done, and the tube erected again, if the tall cylinder of mercury be of the uſual, or former height, as we found it, 'twill appear that the weight of the external air may preſs as much upon the ſtagnant mercury, thro' a little hole, as when all the upper ſuperficies of that mercury, was directly expoſed to it.

Both an oblique preſſure of the atmoſphere, and the ſpring of a little included air, will ſuſtain the mercury in the barometer.

25. If, inſtead of drawing the ſhorter leg of our ſiphon directly upwards, or parallel to the longer, as in the foregoing experiment, you bend off the ſlen-

der part, ſo that, were it continued, it would make a right angle with the longer leg of the ſiphon, or an acute one, tending downwards; and when the tube is erected, the mercury reſts at its uſual ſtation; 'twill appear, that the preſſure of the atmoſphere may be exercis'd upon it as well obliquely, when the pipe that conveys it, is either horizontal, or opens downwards. *Fig.* 50.

And, if inſtead of bending this ſlender pipe, you ſeal it up hermetically, the continuance of the mercurial cylinder, at the ſame height, will ſhew, the ſpring of a very little air, ſhut up with the preſſure of the atmoſphere upon it, is able to ſupport as tall a cylinder of mercury, as the weight of that part of the atmoſphere, which can come to exerciſe its preſſure againſt the mercury. *Fig.* 51.

If, when the ſhorter leg of the barometer is ſeal'd, you move the inſtrument up and down, the mercury will vibrate, by reaſon of the yielding ſpring of the impriſon'd air; but becauſe of the reſiſtance of the ſpring, the motion will be diverſified after an odd manner; which may be eaſily perceiv'd by the impreſſion it makes upon the hand, but not ſo eaſily deſcribed. And as, when the ſhorter leg is drawn out ſlender enough, after the inſtrument is furniſh'd with quick-ſilver, 'tis eaſy to ſeal it up with the flame of a candle, without the help of any inſtrument at all; I might here obſerve, that it may, on ſome occaſions, be convenient to ſeal up the barometer, before it be tranſported; and, in ſome caſes, to incline the tube before-hand, till the quick-ſilver have quite fill'd the longer leg: for by this means, the vibrations of the quick-ſilver will be leſs; and 'tis eaſy, when the inſtrument is brought to the deſign'd place, to break off the ſlender apex of the ſhorter leg, and ſo expoſe, again, the mercury to the preſſure of the atmoſphere.

Having caus'd a portable barometer to be made, with the ſhorter leg of a more than ordinary length; I afterwards cauſed the upper part of this leg to be drawn out very ſlender; and laſtly, the ſame to be, about the middle, bent downwards, ſo that the ſmall orifice of the ſlender apex, pointed towards the ground; when neither I, nor ſome others, took notice that the mercury ſtood lower than in ordinary barometers: whence we concluded, that the atmoſphere could preſs, not only at a very ſmall orifice, but, when the air muſt, at this little orifice, tend upwards, to preſs upon the ſurface of the ſtagnant mercury.

To make a barometer uſeful but at certain times.

26. When it appear'd, by a good barometer, that the atmoſphere was conſiderably heavy, I cauſed a glaſs pipe, hermetically ſealed at one end, and in length about two feet and a half, to be fill'd with quick-ſilver; except a very little part, wherein ſome drops of water were put, that we might the better diſcern the bubbles, if any ſhould be left, after the inverſion of the tube into an open glaſs, containing ſtagnant mercury. Having, by this means, freed the tube from bubbles, we ſo order'd the matter, that the quick-ſilver, and the little water about it, exactly fill'd the tube, without leaving any viſible interval at the top; and yet the mercurial cylinder was but very little higher than that of our barometer at

that time. Then the pipe was left erected in a quiet place, where the liquors retain'd the former height for ſeveral days. A ſchool-philoſopher would confidently have attributed this ſuſtentation of ſo heavy a body, to nature's dread of a vacuum; but either ſhe is not always equally ſubject to that fear, or ſome other cauſe of the phenomenon muſt be aſſign'd: for, when, long after, I had obſerv'd by the barometer, that the atmoſphere was grown much lighter than before, I found the quick-ſilver, in the ſhort tube, conſiderably ſubſided; leaving a cavity at the top, which afterwards grew leſs, as the atmoſphere became heavier.

The aſcent of liquors in very ſlender tubes in vacuo.

27. Some ſpirit of wine, ting'd with cochineal, being put into the receiver, and the air withdrawn, it bubbled exceedingly for a conſiderable time. Then, little hollow pipes, of different ſizes, were put into it, when the red liquor aſcended higher in the more ſlender, than in the others; but upon extracting the air, there ſcarce appeared any ſenſible difference in the heights of the liquor, nor upon the letting it in again.

Afterwards, two ſuch tubes, of different ſizes, being faſten'd together with cement, we let down into the ſame ſpirit of wine, when the receiver was well exhauſted: notwithſtanding which, the liquor aſcended in them, for ought we could plainly ſee, after the ordinary manner: only when the air was let in again, there ſeem'd to be ſome little riſing, at leaſt in one of the tubes.

In this experiment, tho' there appear'd no bubbles at all in the ſpirit of wine in the veſſel, yet, for a conſiderable time, there aroſe bubbles in that part of the liquor which was got into the ſlender pipes.

A ſpontaneous aſcent of water in a tube filled with a compact body.

28. I took a ſtrait pipe of glaſs, open at both ends, and of a moderate bore; and having tied a linen rag to one end of it, that the water might have free paſſage in, and the powder not be able to fall out, we carefully fill'd the cavity with minium; and then having erected the tube, ſo that the bottom of it reſted upon that of a ſhallow, open-mouth'd glaſs, containing water enough to riſe an inch or two above the bottom of the tube, it inſinuated itſelf, by degrees, into the cavity thereof, as appear'd by a little change of colour in that part of the minium which it reach'd; till the open glaſs being, from time to time, ſupplied with freſh liquor, it attain'd to the height of about thirty inches.

Taking, afterwards, another tube, and ſome minium, carefully prepared, I proſecuted the experiment, ſo as to make the water riſe in the pipe about forty inches above the ſurface of the ſtagnant water.

Making the experiment with beaten glaſs, pieces of ſpunge, putty, *&c.* I did not find any of them ſucceed ſo well as the minium. Ting'd liquors, as ink, tincture of ſaffron, *&c.* ſeem'd not to riſe near ſo high as water; as if the diſſolved ingredients gradually choaked the pores of the minium.

To have the grains of our powder more minute, and the intervals between them ſmaller, I choſe the beſt ſort of minium, ſifted it very fine, and ſo put it, by little and little, into the tube; that by ramming it from time to time, it might be made to lie the cloſer: and this method ſucceeded

ceeded well. It ſeem'd, by a trial or two, that if the tube were very ſlender, the experiment would not ſucceed.

It may be worth while to obſerve, in what times the water aſcends to certain heights; for, at the beginning, it will aſcend much faſter than afterwards, and ſometimes continue riſing for 30 hours, or longer.

One end propoſed in this experiment is, to diſcover a miſtake in the modern explication of filtration; which ſuppoſes, that the parts of the filtre which touch the water, being ſwell'd, by the ingreſs of it into their pores, are thereby made to lift up the water, till it touch the higher parts of the filtre; by which means, theſe being alſo wetted and ſwell'd, raiſe the water to the other neighbouring parts of the filtre, till it have reach'd to the top of it, whence its own gravity makes it deſcend; but, in our caſe, we have a filtre made of ſolid metalline corpuſcles; where it will be very hard to ſhew, that any ſuch intumeſcence is produc'd, as this explanation requires.

Water aſcends ſo few inches, even in very ſlender pipes, that the riſe of the ſap in trees, ſeems hardly accountable for, from the ſame cauſe. In the laſt trial, above-mention'd, I made water to aſcend above 3 feet and a half; and if by ſo ſlight an expedient, water may be rais'd as high as is neceſſary for the nutrition of ſome thouſands of plants; for ſuch a number there is that exceed not 3 feet and a half in height; I aſk why nature may not have uſed other contrivances, to make liquors aſcend to the tops of the talleſt trees; eſpecially, ſince beſides heat, and ſomething equivalent to valves, *&c.* many other things, perhaps not yet dream'd of, may probably concur to the effect?

As formerly, by bending theſe ſlender pipes, we made ſhort ſiphons, thro' which the water would run, without being at firſt aſſiſted by ſuction; ſo I try'd whether I could, in larger pipes, make much longer ſiphons, by the help of minium. But tho, when the orifices pointed upwards, fine minium were ramm'd into both the legs, and both the orifices cloſed, yet when they came to be again turn'd downwards, the weight of the minium would make ſome ſuch diſcontinuation, as to hinder the farther progreſs of the water. This impediment, however, I judg'd ſuperable, but had no opportunity to proſecute the experiment.

The ſpontaneous aſcent of ſalts along the ſides of glaſſes.

29. Having in ſhallow, wide-mouth glaſſes, expoſed a ſtrong ſolution of common ſea-ſalt, or of vitriol, to the air, which reach'd not, by ſome inches, to the tops of the veſſels; and having ſuffer'd much of the aqueous part to exhale very ſlowly; the coagulated ſalt, at length, appear'd to have lined the inſide of the glaſſes, and to have aſcended much higher than where the ſurface of the remaining water then reſted; or the part whereto the liquor reach'd, when it was firſt pour'd in. And if the experiment were continu'd long enough, I ſometimes obſerv'd this aſcent of the ſalt to be of ſome inches; and that the ſalt did not only line the inſide of the glaſs, but getting over the brim of it, cover'd the outſide alſo with a ſaline cruſt; ſo that, conſidering what a little liquor remain'd in the glaſs, it was ſurprizing how it could poſſibly get thither. Other ſalts alſo, beſides

theſe mention'd, will exhibit the ſame phenomenon. The cauſe of this odd effect may be referr'd to that of the aſcent of liquors in pipes.

I obſerv'd in water, and aqueous liquors, that part of the ſurface next the ſides of the glaſs, to be ſenſibly more elevated than the reſt of the ſuperficies; and if very minute clippings of ſtraw, or other ſmall and light bodies, floating upon the water, approach near enough to the ſides of the glaſs, they will be apt to run up, as it were, this aſcent of water, and reſt againſt the ſides of the glaſs.

We may alſo obſerve, that ſea-ſalt uſually coagulates at the top of the water, in ſmall and oblong corpuſcles; ſo that, as to theſe, it is eaſy to conceive, how numbers of them may faſten themſelves around the inſide of the glaſs. And beſides ſea-ſalt, I have found ſeveral others, which, if their ſolutions be ſlowly evaporated, will, whilſt yet there remains a large proportion of liquor, afford ſaline concretions at the top of the water. And the faſtning of ſaline particles to the ſides of the glaſs, may, perhaps, be promoted by a coldneſs, communicated by corpuſcles contiguous to the glaſs; becauſe the glaſs may be ſuppoſed more cold, upon account of its denſity, than water; but by the evaporation of the aqueous parts of the ſolution, the ſurface of the remaining liquor muſt neceſſarily ſubſide; and thoſe ſaline particles that were contiguous to the inſide of the glaſs, and the more elevated part of the water, having no longer liquor enough to keep them diſſolv'd, will be apt to adhere to the ſides of the glaſs; and upon the leaſt farther evaporation of the water, become a little higher than the greater part of the ſuperficies of that liquor; whence, by reaſon of the little inequalities that will be on the internal ſurface of the adhering corpuſcles of the ſalt; and perhaps alſo, on the internal ſuperficies of the glaſs; there will be intercepted between the ſalt and the glaſs, little cavities, into which the water, contiguous to the bottom, will aſcend, or be impell'd by the ſame power that raiſes it in ſlender pipes. And when the liquor is thus got to the top of the ſalt, and lies expoſed to the air; the ſaline part may, by the evaporation of the aqueous, be brought to coagulate there; and conſequently, to increaſe the height of the ſaline film, which, by the like means, may, at length, reach to the very top of the glaſs; and thence it may eaſily be brought over to the outſide of the veſſel, where the natural weight of the ſolution will facilitate its progreſs downwards; whence the pellicle of ſalt, together with the contiguous ſurface of the glaſs, may, at length, conſtitute a kind of ſiphon.

Thus I have uſually obſerv'd the ſaline film to be very eaſily ſeparable from the glaſs in large flakes; which argues, that they did not ſtick cloſe to one another, except in a few places; but had a thin cavity interpoſed between them, thro' which the water might aſcend.

Nor is it repugnant to this explanation, that in caſe the water aſcended, it ſhould diſſolve the ſalt; for the liquor being already upon the point of concretion, it is ſo ſaturated with ſalt, that it can diſſolve no more. Whence we may alſo ſee, why, when the ſaline film reaches to the outſide of the glaſs, the liquor does not run down to the bottom, but coagulates by the way. And I have ſuſpected, that when the concretion is once began, the film may be raiſed and propagated, not only by the motion of the liquor between the inſide of that and the glaſs; but by the ſame liquor inſinuating itſelf on the outſide of the film, into the ſmall interſtices of the ſaline corpuſcles; as ink riſes into the ſlit, and along the ſides of the nib of a pen, tho' nothing but the very point touch the ſurface of the liquor. And by this means the impregnated ſolution may, as it were, climb up to the top of the ſaline concretion, and by coagulating there, add to its height.

To eſtimate the gravity of cylinders of the atmoſphere in known weights.

30. Having caus'd a cylindrical piece of braſs to be very carefully turn'd, of an inch in diameter, 3 inches in length, and open at both ends; to one of theſe ends we exactly fitted a flat bottom of the ſame metal, and faſten'd it very cloſe with little ſcrews on the outſide.

This inſtrument, being balanc'd in an exact pair of ſcales, was carefully fill'd with pure mercury, which we found to weigh 137 drams, and 45 grains; and multiplying that by 10, there will ariſe, for the weight of a mercurial cylinder of one inch in diameter, and 30 inches in height, about 14 pound, 2 ounces, and 3 drams, troy.

The weight of a mercurial cylinder in an equilibrium with the atmoſphere, and of an inch in diameter, being thus ſettled, we may eaſily compute the weight of a cylinder of quick-ſilver of another diameter, and conſequently the force of the preſſure of an atmoſpherical column of the ſame diameter; for ſince cylinders of equal heights are to one another, as their baſes; and the baſes of cylinders to each other as the ſquares of their diameters; and laſtly, ſince we here ſuppoſe mercury a homogeneous body; the mercurial cylinders will be to each other in weight, as they are in bulk; if then, for inſtance, we would know the weight of a cylinder 30 inches high, whoſe diameter is 2 inches, the rule is this: as the ſquare of the diameter of the ſtandard cylinder, whoſe weight is known, to the ſquare of the diameter of the cylinder propos'd; ſo is the bulk of the former to the bulk of the latter, and the weight of that to the weight of this. Thus the ſquare of one inch, the diameter of the ſtandard cylinder, being one, and the ſquare of two, the diameter of the cylinder given, being four; the bulk or ſolid content of this latter cylinder, and conſequently its weight, will be four times as great as thoſe of the ſtandard cylinder.

The attractive virtue of the load-ſtone in an exhauſted receiver.

31. We took a ſmall vigorous load-ſtone, cap'd and fitted with a looſe plate of ſteel, ſo ſhaped, that when ſuſtain'd by the ſtone, we could hang, at a little crook that came out of the midſt of it, and pointed downwards, a ſcale; into which we put weights; and then, by ſhaking the load-ſtone, as much as we gueſs'd it would be by the motion of the engine, we found the greateſt weight, that we preſum'd it would ſupport, notwithſtanding the agitation whereto it would be expos'd, was, beſides the iron plate and the ſcale, 6 ounces troy: and if we added half an ounce more, the whole weight appear'd too eaſy to be ſhaken off. This done, we hung the load-ſtone with all the weight it ſuſtain'd, at a button of glaſs faſten'd to the top of the inſide of the receiver, when it was firſt blown; and tho'

tho' in about 12 exſuctions we uſually emptied ſuch receivers, as much as was requiſite for moſt experiments; yet this time we made above twice that number; when violently ſhaking the engine, without thereby ſhaking off the weight that hung at the load-ſtone, the iron ſeem'd to be very nearly as firmly ſuſtain'd by it, as before the air began to be pump'd out; for the extraction of the air, tho' it be not ſuppos'd to weaken the preciſe power of the load-ſtone; yet it muſt leſſen its power to ſuſtain the ſteel, becauſe this in ſo thin a medium muſt weigh heavier than in the air.

The preſſure of the external air being taken off, the ſucker of a ſyringe is eaſily drawn up, tho' the lower orifice be ſtopt.

Fig. 52.

32. We took a braſs ſyringe, the barrel about 6 inches in length, and the diameter about an inch and 3 eighths; and having, by placing a thin bladder about the ſucker, and pouring oil into the barrel, made the inſtrument ſtanch, whilſt the ſucker mov'd without much difficulty; we thruſt this to the bottom of the barrel to exclude the air; and having laid aſide the ſlender pipe of the ſyringe, we carefully ſtopt the orifice to which the pipe, in theſe inſtruments, is uſually ſcrew'd; then drawing up the ſucker, we let it go, to judge, by the violence with which it would be driven back again, whether the ſyringe were fit for our purpoſe; and finding it to be ſo, we faſten'd a ponderous piece of iron to keep it down; and then fixing to the handle of the rammer one end of a ſtring, whoſe other end was ty'd to the turn-key, we convey'd this ſyringe, and the weight belonging to it, into a receiver; and having pump'd out the air, we began to turn the key, thereby to ſhorten the ſtring that ty'd the handle of the ſyringe to it, and found no reſiſtance in drawing up the ſucker from the bottom of the cylinder.

And repeating the experiment with the like ſucceſs, when the receiver being exhauſted, we had drawn up the ſucker, almoſt to the top of the barrel by a weak ſtring, we kept the parts of the ſyringe in that poſture, till a paſſage was open'd to the outward air; upon which the ſucker was ſo forcibly depreſs'd, that it broke the ſtring, and was violently driven back to the lower part of the barrel; tho' the ſtring had ſuſtain'd between 4 and 5 pound weight, and broke long before all the air, that flow'd in to fill the receiver, had found entrance.

Again, we took the ſame ſyringe, and having found it tight enough for our purpoſe, we carefully clos'd the vent with a cork and cement, and having ty'd a weight of 2 pound 2 ounces to the barrel, we ſuſpended the rammer of the ſyringe, by a ſtring, in a large receiver; and cauſing the pump to be ply'd, we made 11 or 12 exſuctions, without finding any appearance of change in the ſyringe; but cauſing the pumping to be continu'd, I perceiv'd, within 2 or 3 exſuctions more, the cylinder began to be drawn very ſlowly down, by the weight hanging at it; and likewiſe try'd, that, juſt upon a freſh exſuction, the deſcent would be manifeſtly accelerated. And when we had ſuffer'd the barrel and weight to ſlide down as far as we thought fit, we let in the external air, which rais'd them both again much faſter than they had ſubſided.

And, ſubſtituting a far heavier weight for the former, the depreſſion of the barrel of the ſyringe ſucceeded for two or three times, ſucceſſively, much ſooner than before.

A ſyringe cauſing the preſſure of the air to raiſe a conſiderable weight.

Fig. 52.

33. Having cemented up the hole at the bottom of the ſyringe, we ty'd to the barrel a hollow piece of iron, that ſerved for a ſcale; into which we put weights, ſucceſſively, to try if, when the ſucker was forcibly drawn up, and held ſteddily, in its higheſt ſtation, the weight, faſten'd to the barrel, which was held down whilſt the ſucker was drawn up, and, afterwards, let go, would be conſiderably rais'd. And, when we perceiv'd, that the addition of half a pound, or a pound, more, would make the weight too great to be ſo rais'd; we forbore to put in that increaſe of weight: and, having ty'd the handle of the rammer to the key, we convey'd the ſyringe, together with its clog, into a receiver; out of which, a convenient quantity of air being pump'd, we were, thereby, eaſily enabled to draw up the ſucker, without the cylinder: after which, having let in the air, ſo that the weight was rais'd a little, I cauſed two pound to be taken out; and then the receiver, being ſomewhat exhauſted, and the air admitted; the clog, which amounted to about ſixteen pound, was ſwiftly rais'd, and, as it were, ſnatch'd up from the middle, to the upper part of the rammer.

The aſcent of liquors in ſyringes owing to the preſſure of the air.

Fig. 53.

34. We took a ſmall receiver, ſhaped like a pear, cut off, horizontally, at both ends; we, alſo, took the ſyringe, formerly mention'd, and having cemented thereto, inſtead of its own braſs-pipe, a ſmall pipe of glaſs, about half a foot in length, we put this ſyringe in at the narrow end of the receiver; to whoſe orifice was, afterwards, carefully cemented the braſs cap, with the key, whereto we ty'd the handle of the rammer: then, having conveniently placed, upon the engine, a very ſhort thick conical glaſs, containing a ſufficient quantity of quick-ſilver; we ſet the receiver over it, ſo that the lower end of the pipe of the ſyringe reach'd almoſt to the bottom of this glaſs; and, conſequently, was immers'd far beneath the ſurface of the quick-ſilver: when, all things being prepared, the air was pump'd out of the receiver, and, conſequently, out of the little glaſs that held the mercury; the ſucker being warily drawn up; we could not ſee the quickſilver aſcend to follow it: but the air being let ſlowly into the receiver, the mercury was quickly impell'd up to the top of the glaſs-pipe.

And, for farther ſatisfaction, when the experiment was repeated, we plainly obſerv'd, that tho', when the receiver being not yet exhauſted, the ſucker was drawn up but one inch, the mercury would be rais'd to the upper part of the glaſs-pipe of the ſyringe; yet, after the exhauſting of the receiver, tho' the ſucker was drawn up twice as high, there appeared no aſcent of the mercury in the pipe.

To confirm this experiment, we caus'd the ſyringe to be ty'd faſt to a ponderous body, that might keep the cylinder unmov'd, when the ſucker ſhould be drawn up; we alſo cemented to the vent, or ſcrew, at the bottom of the ſyringe, a pipe of glaſs, about two inches long; and, having placed the heavy body upon a pedeſtal of a convenient height, that the glaſs-pipe might be all ſeen beneath it; and a very low vial, almoſt fill'd with quick-ſilver, might be ſo placed underneath the pipe, that the ſtagnant mercury reach'd far above the immers'd orifice of the ſaid pipe: when things being thus provided, and the handle of the rammer ty'd to the key, belonging to the braſs-cover of the receiver, this veſſel was cemented to the engine, and exhauſted.

We then look'd upon the glaſs-pipe above-mention'd, and, being able to ſee thro' it, we, by the ſtring, drew up the ſucker to a conſiderable height, but could not perceive the pipe to be fill'd with any ſucceeding mercury; but, warily letting in ſome air, we quickly ſaw the mercury impell'd to the very top of the pipe; and concluded, from the quantity rais'd, that ſome was alſo driven into the cavity of the cylinder. This experiment alſo we ſucceſsfully try'd with tinged ſpirit of wine. Hence it appears, that if a ſyringe were made uſe of above the atmoſphere, neither the ſtopping of the pipe would hinder the eaſy drawing up of the ſucker; nor the drawing up the ſucker, tho' the pipe were not ſtopp'd, raiſe, by ſuction, the liquor wherein the pipe was immers'd.

The adheſion of cupping-glaſſes depends upon the preſſure of the air.
Fig. 54.

35. We took a glaſs, about an inch and half in diameter, but much longer than an ordinary cupping-glaſs of that breadth; we alſo provided a receiver, ſhaped like a pear, and open at both ends, at the ſharper whereof, there was a ſmall orifice; but, at the obtuſer, a ſhort neck, wide enongh to admit the cupping-glaſs, without touching the ſides of it. The ſmaller orifice of the receiver being cemented to the engine, I caus'd the cupping-glaſs to be well faſten'd, with the mouth upwards, to the palm of a perſon's hand; then caus'd him to put it into the receiver, and lay his hand ſo upon the orifice, that it might ſerve for a cover to it, and hinder any air from getting in between them: but, upon the firſt ſuck, the cupping-glaſs fell off; the weight of the atmoſphere preſſing ſo ſtrongly upon the perſon's hand, that he complain'd, he could very hardly take it from the glaſs, into which it was almoſt thruſt. We repeated the experiment, faſtening the cupping-glaſs more ſtrongly than before; the tumour occaſion'd whereby, was very viſible: but now alſo, as before, at the very firſt turning of the ſtop-cock, to let the air out of the receiver, the cupping-glaſs fell off.

A great weight rais'd by a cupping-glaſs without heat.
Fig. 55.

36. We took the braſs-ring, formerly mention'd, and cover'd it with a wet bladder, which was ſo ty'd on, that the bottom of the bladder cover'd the upper orifice of the ring, and lay ſtretch'd upon it whilſt the neck of the bladder was ty'd with a ſtring, near the middle of the lower orifice of the ring; and, in this lower part of the bladder, we made two or three ſmall holes, for the air to paſs in and out at: then having placed, at the bottom of our capp'd receiver, a thick piece of wood, perforated to receive the neck of the bladder; we placed the cover'd ring upon this piece of wood, ſo that the upper part of the bladder lay parallel to the horizon; then we ſuſpended, at the key belonging to the cap of our receiver, a blind glaſs-head, inſtead of a cupping-glaſs, which name it may bear;

and to the upper part of this glaſs, we faſten'd a large ring of metal to preſs it againſt the bladder. The receiver being now cemented on to the engine, we, by the help of the key, let down the cupping-glaſs till it almoſt touch'd the level ſuperficies of the bladder; and when the receiver was moderately exhauſted, we let down the cupping-glaſs a little lower, ſo that it reſted upon the bladder, and touch'd it with all the parts of its orifice; whence the cupping-glaſs with its ſubjacent bladder was become an internal receiver wherein the air was conſiderably expanded. Then we warily let the air into the receiver, and thereby the air that ſurrounded the cupping-glaſs or internal receiver, having now a ſtronger preſſure than that in the cupping-glaſs could reſiſt, the bladder on which the cupping-glaſs reſted, was conſiderably thruſt into the cavity of the glaſs, and made to ſtick very cloſe to the orifice of it.

Fig. 56.

Repeating the experiment, and exhauſting the receiver further than before, we took out the cupping-glaſs and the bladder, which, together with the included braſs-ring was hanging at it; and having ty'd the glaſs to the hook of a ſtatera, and a large ſcale to the neck of the bladder, we put weights, by degrees, into the ſcale, till we thus forced off the bladder from the glaſs; which hapned not till the weight amounted to thirty-five pound.

Bellows, with the noſe ſtopp'd, open of themſelves, when the preſſure of the air is taken off.

Fig. 5.

37. We caus'd a pair of bellows to be made different from ordinary ones, their boards being circular, without handles, and without clack or valve, the noſe but an inch long, to be lengthned, if occaſion required, with a pipe, and about ſix inches in diameter, the leather being limber; ſo that when the bellows were open'd to their full extent, by drawing up the upper baſis at a button purpoſely made in the midſt of it, they reſembled a cylinder 16 or 18 inches high; but there was ſome little and unperceiv'd leak in them, whereat air had paſſage, when the noſe was accurately ſtopp'd; however, if we drew up the upper baſis from the lower, the external air would, on all ſides, preſs the leather inward, and render the ſhape of the inſtrument very far from cylindrical. Then carefully ſtopping the noſe, after we had brought the baſes to touch each other, and conveying the inſtrument into a large receiver, it quickly appear'd, when the pump was ſet on work, that, at every exſuction, the air in the folds of the leather, and the reſt of the little cavity left between the baſes, made the upper of them manifeſtly riſe; tho' its own weight would ſoon after depreſs it again, either by driving out ſome of the air, where the inſtrument was not ſufficiently right, or by making it, as it were, ſtrain thro' the leather itſelf: and if the pump were ply'd faſter than ordinary, the upper part of the bellows would be ſoon rais'd to a conſiderable height; as appear'd more evidently, if we haſtily let in the external air, whereby the baſes would be clapt together, and the upper of them conſiderably depreſs'd; ſo that the imperfection of the bellows render'd the experiment rather more than leſs concluſive: for ſince there was no external force apply'd to open them, if, notwithſtanding ſome of the included air could get out, the ſpring of the internal air was ſtrong enough to open

the bellows, when the ambient air was withdrawn, much more would the effect have been produced, if the bellows had been perfectly ſtanch.

An attempt to examine the hypotheſis of æther, as to its exiſtence.

38. Since, if there be ſuch a thing as a celeſtial matter, or æther, it muſt compoſe far the greateſt part of the univerſe known to us; it deſerves to be enquir'd, whether we can, by ſenſible experiments, diſcover its exiſtence, or qualities. To this end I thought our pneumatical engine might contribute, if I could manage therein ſuch a pair of bellows as I deſign'd; for I propoſed to faſten a convenient weight to the upper baſis, and clog the lower with another, able to keep it horizontal, and immoveable, ſo that when, by the help of the turn-key, the upper baſis ſhould be rais'd to its full height, the cavity of the bellows might be brought to its full dimenſions. This done, I intended to exhauſt the receiver, and, conſequently, the bellows thus open'd; ſo that both the receiver, and they, might be carefully freed from air: after which, I purpos'd to let go the upper baſe of the bellows, that being haſtily depreſs'd by the incumbent weight, it might ſuddenly fall down to the lower; and by thus greatly leſſening the cavity, expel thence the matter, if any there were, before contain'd in it; and that, if it could, by this way be done, at the hole of a ſlender pipe, faſten'd either near the bottom of the bellows, or in the upper baſis, againſt, or over the orifice of which pipe, there might be placed, at a convenient diſtance, either a feather, or the ſail of a little windmill, made of ſome other light body, fit to be put into motion by the impulſe of any matter which ſhould be forced out of the pipe.

Now, if by this means, notwithſtanding the abſence of the air, it ſhould appear, that a ſtream of other matter, able to ſet viſible bodies in motion, ſhould iſſue out at the pipe of the compreſs'd bellows, it would alſo appear, that there may be, plentifully, found a much ſubtiler body than common air, in places deſerted by ſuch air: and that it is not ſafe to conclude, from the abſence of the air, in our receivers, and the upper part of the *Torricellian* tube, that there is no body, but an abſolute vacuity. But if, on the other ſide, there ſhould appear no motion at all to be produc'd, ſo much as in the feather, it ſhould ſeem, that either the cavity of the bellows was abſolutely empty; or that it would be very difficult to prove, by any ſenſible experiment, that it was full. And if, by any other means, it be demonſtrable, that it was repleniſh'd with æther, we might ſuppoſe, from our experiment, that 'tis not eaſy to make it ſenſible by mechanical experiments; and that 'tis really ſo ſubtile, and yielding a matter, as does not either eaſily impel light bodies, or ſenſibly reſiſt, like air, the motions of other bodies thro' it; but is able, freely, to paſs the pores of wood, leather and cloſer ſubſtances, which the air, in its natural ſtate, doth not.

Fig. 58.

And, to make the trial more accurate, I caus'd a ſmall pair of bellows to be made with a bladder; and that this might remain entire, we glued the two baſes, the one to the bottom, and the other to the oppoſite part thereof; ſo that the neck came out at a hole, purpoſely made for it, into the upper baſis; whence, into the neck, it was eaſy to fix what pipe we judg'd fit. We had, alſo, thoughts of procuring another pair of tight bellows,

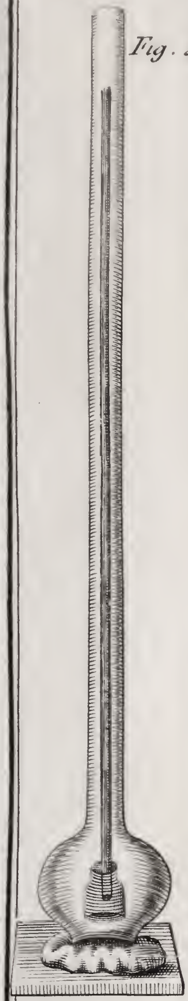

Fig. 44. p. 475.

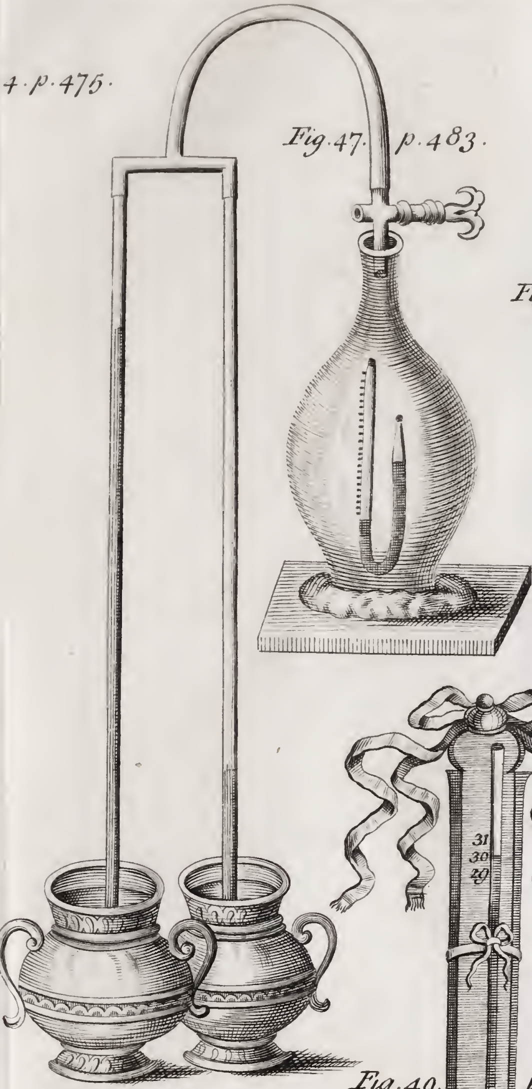

Fig. 47. p. 483.

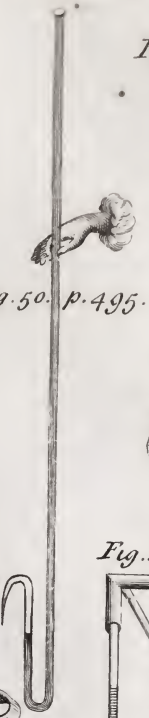

Fig. 50. p. 495.

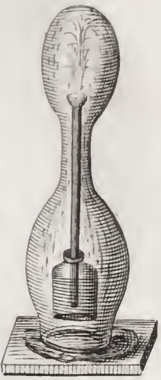

Fig. 45. p. 477.

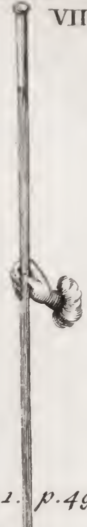

Fig. 51. p. 495.

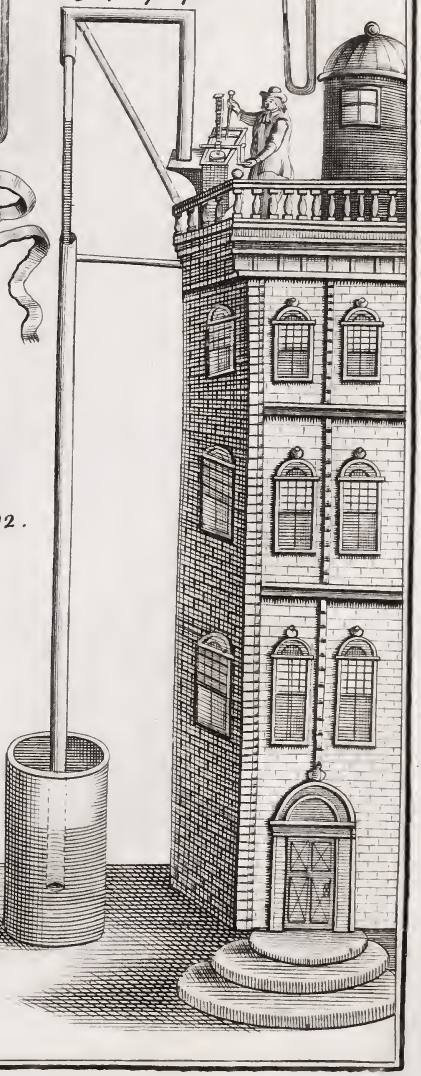

Fig. 48. p. 486.

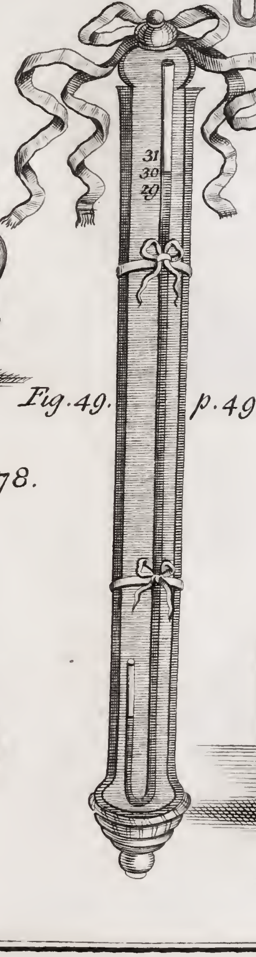

Fig. 49. p. 492.

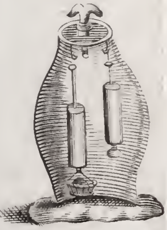

Fig. 52. p. 500.

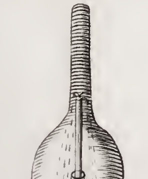

Fig. 46. p. 478.

made with a very little clack in the lower baſis; that, by haſtily drawing up the other baſis, when the receiver and bellows were very carefully exhauſted, we might ſee whether the ſubtile matter that was expell'd by the upper baſis, in its aſcent, would, according to the modern doctrine of the circle, made by moving bodies, be impell'd up, or not.

We likewiſe, thought of placing the little pipe of the bladder-bellows, beneath the ſurface of water, exquiſitely freed from air, to ſee whether, upon the depreſſion of them, by the incumbent weight, when the receiver was carefully exhauſted, there would be any thing expell'd at the pipe, productive of bubbles in the liquor, wherein its orifice was immers'd.

To bring our conjectures to a trial, we put into a capp'd receiver, the bladder, accommodated as already mentioned, containing between half a pint, and a pint; and to depreſs the upper baſis of theſe little bellows the more eaſily, and uniformly, we cover'd the round piece of paſt-board, that made the upper baſis, with a pewter plate; a hole being made in it for the neck of the bladder: which upon trial, prov'd not ponderous enough without weight of lead. And to ſecure the feather above-mentioned, from being blown aſide, we made it to move in a perpendicular ſlit in a piece of paſt-board, faſten'd to one part of the upper baſis; as that whereto we glued the feather, was to another part. Things being thus provided, the pump was work'd; and as the ambient air was, from time to time, withdrawn, that in the bladder expanded itſelf ſo as to lift up the metalline weight, and yet, in part, to ſally out at the little glaſs pipe of our bellows; as appear'd by its blowing up the feather, and keeping it ſuſpended, till the ſpring of the air in the bladder was too far weakned. In the mean time, we did, now and then, by the help of a ſtring faſtned to the turn-key, and the upper baſis of the bellows, let down the baſis a little, to obſerve how, upon its ſinking, the blaſt, againſt the feather, would decreaſe, as the receiver was further exhauſted. And when we judg'd it to be ſufficiently freed from air, we let down the weight, but could not perceive that, by ſhutting the bellows, the feather was at all blown up as before; tho' the upper baſis were more than uſually depreſs'd. And yet it's ſomewhat odd, that when, in order to a further trial, the weight was drawn up again; as the upper baſis roſe from the lower, the ſides of the bladder were ſenſibly preſs'd, or drawn inwards. The bellows being thus open'd, we let down the upper baſis again, but could not perceive that any blaſt was produc'd; for though the feather, which lay juſt over, and near the orifice of the little glaſs pipe, had ſome motion, yet this ſeem'd plainly to be but a ſhaking, and almoſt vibrating motion, whereinto it was put by the upper baſis, which the ſtring kept from a ſmooth and uniform deſcent: but not to proceed from any blaſt, iſſuing out of the cavity of the bladder. And, for further ſatisfaction, we cauſed ſome air to be let into the receiver; becauſe there was a poſſibility that the ſlender pipe might, by ſome accident, be choaked: but tho', upon the return of the air into the receiver, the baſes of the bellows, were preſs'd cloſer together, yet it ſeem'd that ſome little air got thro' the pipe, into the cavity of the bladder; for when we began again to withdraw the air that was let into the receiver, the bladder began to ſwell again, and, upon letting down the weight, to blow up, and ſuſtain the feather, as happen'd before the receiver had been ſo well exhauſted.

Continued. *Fig.* 59.

39. I caus'd a crooked pipe to be made for the ſyringe, formerly mention'd, inſtead of its ſtrait one, whoſe ſhorter leg was parallel to the longer. And this pipe, after being ſcrew'd on carefully, was cemented to the barrel; and becauſe the braſs-pipe could ſcarce be made ſmall enough, we cauſed a ſhort and ſlender pipe of glaſs, to be put into the orifice of the ſhorter leg, and carefully faſten'd to it with cement. Then the ſucker being made to go ſmooth, without leſſening the ſtaunchneſs of the ſyringe, there was faſten'd to the handle of the rammer, a weight made in the form of a ring, or hoop; which, by reaſon of its figure, might be ſuſpended from the handle of the rammer, and hang looſe on the outſide of the cylinder, and which, both by its figure and weight, might eaſily, and ſwiftly depreſs the ſucker, when drawn up. The ſyringe, thus furniſh'd, was faſten'd to a broad, heavy pedeſtal, to keep it in its vertical poſture, and to hinder it from tottering, notwithſtanding the weight that clogg'd it. Beſides all this, we took a feather, about two inches long, of which there was left, at the end, a part about the breadth of a man's thumb-nail, to cover the orifice of the ſlender glaſs pipe of the ſyringe; for which purpoſe, the other extremity of it was ſo faſten'd, with cement, to the lower part of the ſyringe, that the broad end of the feather ſtood juſt over the little orifice of the glaſs, at ſuch a convenient diſtance, that when the ſucker was a little drawn up, and let go again, the weight would depreſs it faſt enough to blow up the broad part of the feather. The handle of the rammer, being now ty'd to the turn-key of a capp'd receiver, the ſyringe, and its pedeſtal, were incloſed in a capacious receiver; and the pump, being ſet on work, we, after ſome quantity of air was drawn out, rais'd the ſucker a little, by the help of the turn-key: and, then, turning the ſame key the contrary way, we ſuffer'd the weight to depreſs the ſucker, to ſee how the feather would be blown up; and, finding that it was impell'd, forcibly, we continued to pump, by pauſes; during each of which, we rais'd and depreſs'd the ſucker, as before; and obſerv'd, that as the receiver was gradually exhauſted of air, the feather was leſs briſkly driven up, till, at length, when the receiver was well empty'd, the uſual elevations and depreſſions of the ſucker would not blow it up at all, tho' they were far more frequently repeated than before.

After we had long tried, in vain, to raiſe the feather, ſome air was let into the receiver; and tho', when but very little air was admitted, the motions of the ſucker had ſcarce any ſenſible effect upon the feather; yet when the quantity began to be conſiderable, the feather began to move a little upwards; and ſo letting in air, not all at once, but ſucceſſively, and moving the ſucker up and down, in the intervals of thoſe times of admiſſion; we obſerv'd, that as the receiver contain'd more air, the feather was more briſkly blown up.

But, not content with a ſingle trial, we caus'd the receiver to be again exhauſted, and proſecuted the experiment with the like ſucceſs; only having, after the receiver was exhauſted, drawn up, and let fall the ſucker, ſeveral times, ineffectually; having, hitherto, not, uſually, rais'd it by more than one turn of the hand; we now uſed an inſtrument that was tolerably long, and fit to take hold of the turn-key, ſo that we could eaſily raiſe the ſucker between two and three inches at a time, and ſuddenly depreſs it again: yet for all this, which would much have increas'd the blaſt, if there had been a matter fit for it in the cavity of the ſyringe, we could not, ſenſibly, blow up the feather, till we had let a little air into the receiver. But, now, inſtead of the braſs-pipe, hitherto employ'd, we cemented one of glaſs to the ſyringe; its ſhorter leg, after it had, for a while, run parallel to the other, being bent off ſo, that above an inch and a half of it tended downwards; whereby the orifice of it might be immers'd in the water contain'd in a ſmall open jar. The deſign of this contrivance was, that when the receiver ſhould be well exhauſted, we might try whether, by raiſing and depreſſing of the ſucker, any ſuch matter would be driven out at the noſe of the pipe, as would produce bubbles in the incumbent water; which, air, tho' highly rarify'd, is capable of doing. *Fig.* 60.

The only particulars, wherein this experiment differ'd from the former, were theſe. As the air was here pump'd out of the receiver; that in the glaſs-pipe made its way thro' the water, in bubbles. And a little air having once, by a ſmall leak, got in, and forc'd ſome of the water out of the jar into the pipe; when the receiver was again well empty'd, both that water, and the little ſtagnant quantity contain'd in the immers'd part of the pipe, produced ſo many bubbles, of ſeveral ſizes, as quite diſturb'd our obſervations. Wherefore, we let alone the receiver, exhauſted as it was, for ſix or ſeven hours, that the water might free itſelf from air; and then cauſing what air might have ſtolen in, to be again pump'd out, till we perceiv'd, by the gage, that the receiver was well exhauſted, we caus'd the ſucker of the ſyringe to be rais'd and depreſs'd ſeveral times; and tho' even then, a bubble would, now and then, diſturb our obſervations, yet, when we were not thus confounded, we ſometimes obſerv'd, that the elevation and fall of the ſucker, tho' repeated, did not drive out at the pipe, any thing that made diſcernible bubbles in the incumbent water: for tho' ſome ſmall bubbles would rarely appear on the ſurface of the water, yet I could not perceive, that the matter which made them, iſſued out of the pipe; and ſome of them manifeſtly proceeded from aerial particles, ſtill lurking in the water, as I concluded from the place and time of their riſing. But, at length, we obſerv'd, the water, in the immers'd part of the pipe, which was very ſlender, to be about an inch higher than the reſt of the ſtagnant water, and to continue at that height in the pipe, tho' the ſucker were, ſeveral times together, rais'd and depreſs'd, between two and three inches at once; which ſeem'd to argue, that there was a vacuum in the cavity of the ſyringe: or if it were full of æther, this was ſo ſubtile, that the impulſe it receiv'd from the falling ſucker, would not make it diſplace that very ſlender thread of water in the ſmall pipe; though it appear'd by the bubbles, which ſometimes diſclos'd themſelves in the water, after the receiver had been exhauſted, that far more water would be diſplaced, and carried up by a ſmall bubble, conſiſting of air, ſo rarified, that, according to my eſtimate, the particles of it did not, before the pump was firſt ſet on work, poſſeſs, in the water, a five hundredth part of the ſpace of a pin's head.

A light body falling in the exhauſted receiver.

Fig. 61.

40. We took a receiver twenty-two inches high; and, that we might let a body fall therein, we ſo faſten'd a ſmall pair of tongs to the inſide of its braſs-cover, that, by moving the turn-key, we might by a ſtring, open them; which their own ſpring would, otherwiſe, keep ſhut: we then join'd, croſs-wiſe, four broad light feathers, each about an inch long, at their quills, with a little cement; into which we, alſo, ſtuck, perpendicularly, a ſmall label of paper, about the eighth of an inch in breadth, and ſomewhat more in height; by which the tongs might take hold of our light inſtrument, without touching the cement, which, elſe, might ſtick to them. By the help of this ſmall piece of paper, the little inſtrument, of which it made a part, was ſo held by the tongs, that it hung horizontal; and then the receiver, being cemented to the engine, the pump was diligently ply'd, till it appear'd, by a gage, that the receiver had been thoroughly exhauſted. Laſtly, our eyes, being attentively fixed upon the connected feathers, the tongs were, by the help of the turn-key, open'd, and the little inſtrument let fall; which, tho', in the air, it had made ſome turns in its deſcent from the ſame heighth, whence it now fell; yet it here deſcended like a dead weight, without being perceived to make a ſingle turn, or a part of one. However, I cauſed the receiver to be taken off, and put on again, after the feathers were taken hold of by the tongs; whence, being let fall in the glaſs, unexhauſted, they made ſome turns in their deſcent; as they, alſo, did, being let fall a ſecond time, after the ſame manner.

But when, after this, the feathers being placed, as before, we repeated the experiment, carefully pumping out the air, we could not perceive any turning in the deſcent; yet for farther ſecurity, we let them fall twice more, in the unexhauſted receiver; and found them to turn in falling: but when we did, a third time, ſet them looſe in the receiver, well exhauſted; they fell, after the ſame manner they had, in the ſame caſe, done before.

The propagation of ſounds in an exhauſted receiver.

41. We caus'd a cylinder of box to be turn'd of a length ſuitable to that of the receiver, wherein it was to be employ'd. Out of the lower baſis of this cylinder, which was about an inch and a half in diameter, there came a ſmaller cylinder or axle-tree, not a quarter ſo thick as the other, and leſs than an inch long: this was turn'd very true, that it might move ſmoothly in a little ring of braſs made for it in the midſt of a fix'd trencher, or piece of ſolid wood, ſhap'd like a mill-ſtone; being four or five inches in breadth, and between one and two in thickneſs: and the large round groove, purpoſely made, in the lower part of this trencher, I cauſed to be fill'd up with lead, to keep the trencher ſteady: and in the uppermoſt part of this trencher we intended to have holes made, to place bodies in at ſeveral diſtances, as occaſion ſhould require. The upper baſis of the cylinder had, alſo, another axle-tree coming out of the midſt of it, but wider than the former,

that into its cavity it might receive the lower end of the turn-key, to which 'twas to be faſten'd by a ſlender peg of braſs, thruſt thro' two correſpondent holes, the one made in the turn-key, and the other in the ſocket of the axle-tree. There were alſo ſeveral horizontal perforations made in the pillar itſelf to which this axis belong'd ; which pillar we call the vertical cylinder. The general uſe of this contrivance, is, that the end of the turn-key being put into the ſocket, and the lower axis of the vertical cylinder into the trencher ; by the motion of the key, a body faſten'd at one of the holes to the cylinder may be brought to, or remov'd from, or made to ſtrike againſt another body, faſten'd, in a convenient poſture, to the upper part of the trencher.

We caus'd then a hand-bell without its handle and clapper, to be ſo faſten'd to a ſtrong wire, that one end of the wire being fixed in the trencher, the other, which was bent downwards, took hold of the bell. In another hole, made in the circumference of the ſame trencher, was wedg'd a ſteel ſpring, to the upper part whereof was wedg'd a gad of ſteel leſs than an inch long, but conſiderably thick ; the length of this ſpring made the upper part of the hammer, or piece of ſteel, of the ſame height with the bell ; and the diſtance of the ſpring from the bell was ſuch, that when forc'd back the other way, it might, at its return make the hammer ſtrike briſkly upon the outſide of the bell. The trencher being thus furniſh'd and plac'd in a capp'd receiver, the air was diligently pump'd out, and then, by the help of the turn-key, the vertical cylinder was made to go round, by which means, as often as one of the two ſtiff wires, or ſmall pegs, that were faſten'd at right angles into holes made near the bottom of the cylinder, paſs'd by the ſpring, they forcibly bent it in their paſſage from the bell, ſo that as ſoon as the wire was gone by, and the ſpring ceas'd to be preſs'd, it would fly back with violence enough to make the hammer give a ſmart ſtroke upon the bell. And, by this means, we could both continue the experiment at diſcretion, and make the percuſſions more equally ſtrong than it would otherwiſe have been eaſy to do.

Now, when the receiver was well emptied, it ſometimes appear'd doubtful whether any ſound was produc'd or no ; but to me, for the moſt part, it ſeem'd, that, after great attention, I heard a very faint and languid ſound, and yet methought it had ſome ſhrilneſs in it, and ſeem'd to come from afar. But letting in the air, at competent intervals, it was eaſy to obſerve, that the vertical cylinder being ſtill made to go round, when a little air was let in, the ſtroke of the hammer upon the bell became very audible : when more air was admitted, the ſound grew greater, and ſo increaſed till the receiver was again repleniſh'd with air ; tho', even then, the ſound was obſerv'd to be much leſs than when the receiver did not interpoſe between the bell and the ear.

We now, alſo, ſuſpended in the receiver a watch with a good alarum ; and to make this experiment the more accurate, we employ'd a receiver that conſiſted of but one piece of glaſs furniſh'd on the inſide with a glaſs knob or button, to which a ſtring might be ty'd : we alſo hung the watch, not by its chain, but by a very ſlender thread, whoſe upper end was faſten'd to the glaſs button. Then the air being carefully pump'd out, we ſilently expected ed the ringing of the alarum; but hearing no noiſe ſo ſoon as we expected, it might have been doubted, whether the watch continued going, if we had not contriv'd a way to diſcern its motion: wherefore I deſired a gentleman to hold his ear exactly over the button, at which the watch was ſuſpended, and very near to the receiver; who told us, that he could juſt perceive ſomething of a ſound, which ſeem'd to come from far; tho', neither we, who liſten'd very attentively near other parts of the receiver, nor he, if his ears were no more advantageouſly plac'd, were ſatisfied, that we heard the watch at all. Then letting in ſome air, we did, with attention, begin to hear the alarum, whoſe ſound was odd; and by returning the ſtop-cock, to keep any more air from entering, we kept the ſound thus low for a conſiderable time; after which, a little more air, that was permitted to enter, made it become more audible; and when the air was yet more freely admitted, we could plainly hear the alarum at a conſiderable diſtance from the receiver *.

A glaſs drop broke in an exhauſted receiver.

42. The blunter part of a glaſs-drop being faſten'd to a ſtable body, and convey'd into the receiver, and the crooked ſtem being ty'd to one end of a ſtring, whoſe other end was faſten'd to the turn-key, we carefully pump'd out the air; when the ſtem, by ſhortning the ſtring, being broken off, the glaſs-drop was ſhatter'd into a thouſand pieces.

This experiment was, afterwards, repeated with the like ſucceſs; and having, at that time, no gage to try how far the air had been drawn out, we let the external air impel up the water out of the pump inro the receiver, and thereby found, that the veſſel had been well exhauſted.

Light produced in the exhauſted receiver. Fig. 62.

43. Knowing, that hard ſugar, being briſkly ſcraped with a knife, affords a ſparkling light; ſo that one would ſometimes think ſparks of fire flew from it; we caus'd a lump of hard loaf-ſugar to be conveniently, and firmly plac'd in the cavity of our capp'd receiver; and to the vertical cylinder, formerly mentioned, we faſten'd ſome pieces of a ſteel-ſpring, which, being but thin, might, in their paſſage along the ſugar, grate or rub forcibly againſt it; and, then the receiver being well exhauſted, in the night-time, and in a dark room, the vertical cylinder was made, for a pretty while, to move round, by help of the turn-key. Thus the irons that came out of the vertical cylinder, making, in their paſſage, vigorous impreſſions upon the ſugar, that ſtood in their way, there were manifeſtly produced many little flaſhes; and ſometimes too, tho' not frequently, there ſeem'd to be ſtruck off ſmall ſparks of fire †.

44. We

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* That ſound cannot be propagated thro' a vacuum, appears more fully from an experiment of the late Mr. *Hauksbee*, who included a large bell in a receiver full of common air, and cover'd them both with another glaſs, out of which, the air being extracted, tho' ſound was actually produced in the innermoſt, it could not be heard by the by-ſtanders. *Philoſ. Tranſ.* N°. 321. p. 367.

From ſome other experiments of the ſame perſon, 'tis alſo evident, that ſounds are as well augmented in condenſed air, as diminiſh'd in that, which is rarified. See his *Phyſico-mechan. Experiments.* p. 129, 134.

† From a variety of experiments, relating to the attrition of bodies *in vacuo*, made by the late Mr. *Hauksbee*, it appears, that

A kind of halo, and colours produced in the exhauſted receiver.

44. We took a large inverted cucurbit for a receiver, made very clear by wiping, and obſerv'd, that when the pump began to be work'd, if a large candle were held on the other ſide of the glaſs, upon turning the ſtop-cock to let the air out of the receiver into the cylinder, the glaſs would ſeem to be full of fumes, and a kind of halo appear about the flame of the candle; and this, at firſt, was commonly between a blue and a green, but after ſome ſucks, turn'd of a reddiſh or orange colour, both very vivid. The phenomenon, in my opinion, proceeded from hence, that the cement being ſomewhat ſoft, and abounding with turpentine, and having a hot iron apply'd to it, whereby it was both ſoften'd and heated, it ſeems rational to expect, that, upon withdrawing the air in the receiver, the aerial particles in the cement freed from their former preſſure, would extricate themſelves, and with the looſer ſteams of the turpentine, and perhaps of the bees-wax, expand themſelves, with a kind of exploſion, in the receiver; and by their interpoſition between the light and the eye, exhibit thoſe delightful colours we have ſeen. And, I afterwards found, that I could plainly perceive the colouring ſteams, juſt upon turning the ſtop-cock, to fly up from the cement towards the top of the glaſs; and, if we continued pumping, the receiver would grow clearer, and the colours more dilute, poſſibly becauſe the aerial and volatile particles of the upper part of the cement did, in that time, ſpend themſelves; and alſo, becauſe the agitation they receiv'd, from the heat communicated by the iron, continually decay'd. Beſides, when the receiver is more exhauſted, the want of air makes it more difficult for ſteams to float, and be ſupported in it.

But, for a farther confirmation, I caus'd ſome cement to be put into a ſmall crucible, warm enough to melt it; and conveying this into a clear receiver, I caus'd the pump to be work'd: upon which, it manifeſtly appear'd, that opening the ſtop-cock, to let out the air, the ſteams would copiouſly be thrown about from the crucible into the capacity of the receiver; and, after having play'd there a little, fall down again. But, in theſe phenomena, the vividneſs, and ſometimes the kind of the exhibited colours ſeem'd much to depend on circumſtances, ſuch as the degrees of heat, the magnitude and ſhape of the receiver, the quantity of air that remain'd therein, and the nature of the cement itſelf.

Heat produced by attrition in the exhauſted receiver.

45. Croſs the ſtable trencher, formerly mention'd, we faſten'd a ſtrong ſpring of ſteel, ſhap'd almoſt like the lathe of a croſs-bow; and to the midſt of this ſpring was ſtrongly fix'd on the outſide a round piece of braſs, hollow'd almoſt like a concave burning-glaſs. To this piece of braſs, which was thin, and about two inches in diameter, we fitted a convex piece of the ſame metal, almoſt like a gage for a tool to grind glaſſes in, which had be-

Fig. 63.

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that different ſorts of bodies afford lights greatly differing in colour, force, and vigor; that the effects of attrition vary with the different preparation and management of the bodies which ſuſtain it: that bodies, which have yielded a particular light, may, by attrition, be brought to yield no more thereof; and that a conſiderable light is producible, by the attrition of glaſs on glaſs, both *in vacuo*, in common air, and even in water. *Hauksbee*'s *Phyſico-mechan. Exper.* p. 40—44.

longing to it a ſquare handle, whereinto, as into a ſocket, was inſerted a ſquare piece of wood, proceeding from the baſis of a ſquare wooden pillar, which we made uſe of, on this occaſion, inſtead of our vertical cylinder. By the help of another piece of wood, coming from the other baſis of the ſame pillar, the turn-key was join'd to this pillar, and made of ſuch a length, that when the turnkey was forcibly kept down as low as the braſs-cover, it was a part of, would permit, the convex piece of metal juſt deſcribed, depreſs'd the concave piece a pretty way, notwithſtanding a vigorous reſiſtance of the ſubjacent ſpring. A little fine powder of emery was alſo put between the convex and concave pieces of braſs, to make them fit the better, and to facilitate the motion that was to be made; and, to the upper part of the turn-key was faſten'd a good wimble, without which, we preſum'd, that the turning of the key would not produce a ſufficient motion. Things being thus in readineſs, and a mercurial gage convey'd into the receiver, we caus'd the air to be diligently pump'd out, and then order'd a ſtrong man to turn the wimble, and to continue to lean a little on it, that he might be ſure to keep the turn-key from being lifted up by the ſpring. Whilſt the man, with much agility and ſtrength was moving the wimble, I watch'd the gage, to obſerve, whether the agitation of the ſtop-cock, and conſequently the engine, did not prejudice the experiment; and for greater caution, I caus'd the pump to be almoſt all the while kept working. When the man was almoſt out of breath, we let in the air at the cover of the receiver, by lifting up the turn-key; and nimbly removing the receiver, we felt both the pieces of braſs, betwixt which the attrition had been made, and found them very ſenſibly warm.

We afterwards caus'd the man to lay hold of the wimble again, when, by the gage, it appear'd, that the receiver was exhauſted; ſo that by further pumping the quick-ſilver ſeem'd not to be further depreſs'd. And, in this ſecond trial, when we did, as before, haſtily let in the air, and take out the bodies that had been rubb'd againſt one another, they were both of them, eſpecially the uppermoſt, ſo hot, that I could not endure to hold my hand on either; and they did, for ſome time, retain a conſiderable degree of warmth. I alſo caus'd two bodies of wood to be turn'd, for ſize and ſhape like thoſe of braſs, which we had juſt before employ'd; the upper of theſe was of hard oak, the other of beech: but, tho' the wimble was ſwiftly turned, as before, by the ſame perſon, the wood ſeem'd not to me to have manifeſtly acquir'd any warmth; yet, that there had been a conſiderable attrition, appear'd by the great poliſh, which part of the wood had evidently acquired: however, upon repeating the experiment, with more obſtinacy than before, the wood, eſpecially the upper piece of it, was brought to a warmth unqueſtionably ſenſible.

Quicklime ſlaked in the exhauſted receiver.

46. Into an evaporating glaſs, we put a convenient quantity of water; and having convey'd it into a receiver, and well drawn out the air, we let down into it, by the turn-key, a large lump of ſtrong lime; and obſerv'd not, that, at the firſt emerſion, nor for ſome time after, there appear'd any conſiderable number of bubbles; but within about a quarter of an hour the lime

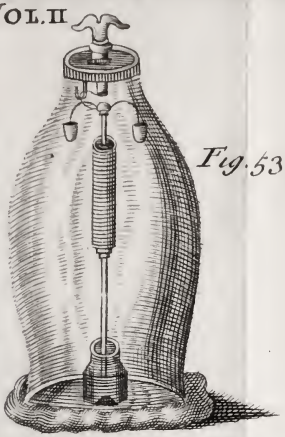

Fig.53.p.501

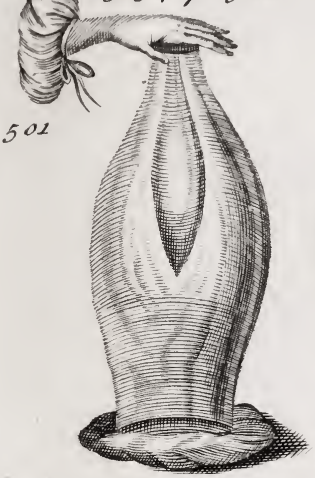

Fig.54.p.502.

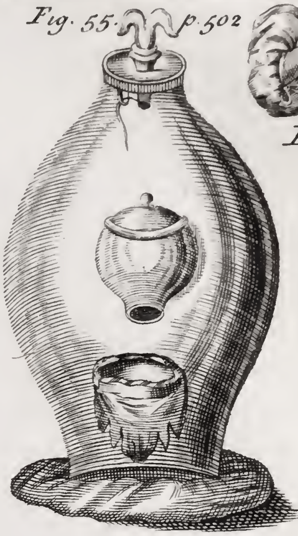

Fig.55.p.502

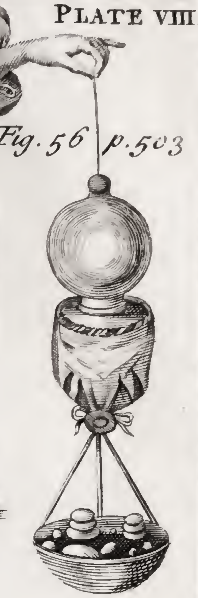

Fig.56 p.503

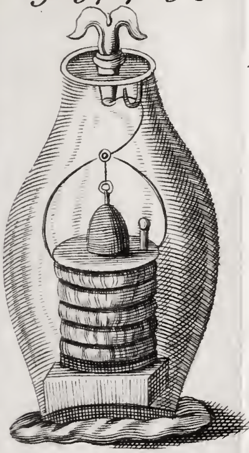

Fig.57.p.503.

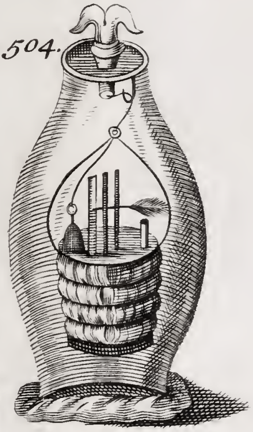

Fig.58.p.504.

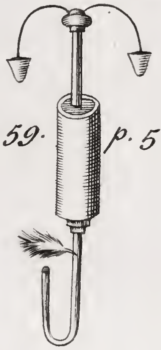

Fig.59. p.506

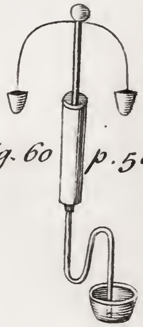

Fig.60 p.507

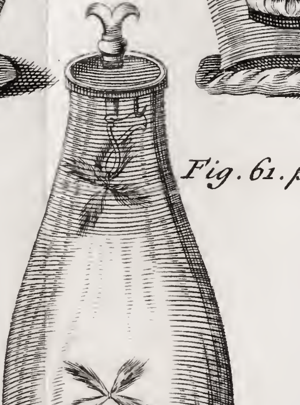

Fig.61.p.508.

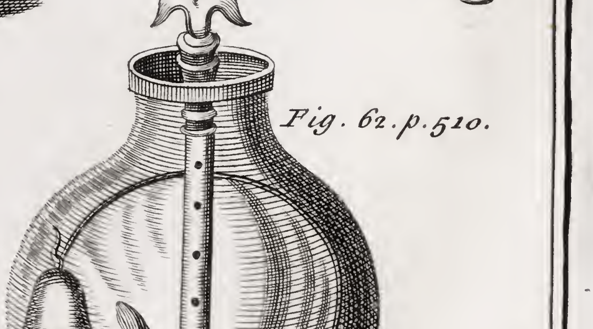

Fig.62.p.510.

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began (the pump continuing to be ply'd from time to time) to ſlake with much violence, and with bubbles wonderfully great, appearing at each new exſuction; ſo that the inſide of the receiver, tho' large, was at length lined with lime-water; and much of the mixture did, from time to time, overflow the veſſel, a great part whereof was purpoſely left unfill'd; nor did any thing, but our wearineſs, put a period to the bubbling of the mixture, whoſe heat was ſenſible even on the outſide of the receiver, and continu'd conſiderably hot, in the evaporating glaſs, for a quarter of an hour after the receiver was remov'd. The lime, employ'd in this experiment, was of a very good and ſtrong kind, made of hard ſtones, and not of chalk, as is that commonly uſed at *London*, which probably would not have been ſtrong enough to have afforded us the ſame phenomenon.

An attempt to meaſure the force of the ſpring of included air.

47. To try, by means of our ſyringe, formerly mention'd, what weight a cylinder of uncompreſs'd air included in it, and conſequently of the ſame diameter with the cavity of the barrel, would be able to ſuſtain; we provided a ſtable frame, wherein the ſyringe might be kept firm and erect; we alſo provided a weight of lead, ſhaped like our braſs ring, formerly deſcrib'd, that, by the advantage of its figure, it might be made to hang down, by ſtrings, from the top of the handle of the rammer, and ſo preſs evenly on all ſides, without rendring the upper part of the inſtrument top-heavy. We took care to leave between the bottom of the ſyringe, which was firmly clos'd with ſtrong cement, and that part of it, where the ſucker was, a convenient quantity of air to expand itſelf, and lift up the weight, when the air external to that included, ſhould be pump'd out of the receiver. And laſtly, the handle of the rammer, from which the annular weight depended, was ſo faſten'd to the turn-key of the cover of the receiver, that the weight might not compreſs the air included in the ſyringe, but leave it in its natural ſtate, till the air was withdrawn from the receiver.

By this method the included air would lift up a weight of 7 or 8 pound; yet when the rammer came to be clogg'd with a greater, the inſtrument prov'd not ſo ſtanch, but that it was eaſier for ſome particles of air to get away between the ſucker and the inſide of the barrel, than to raiſe ſo great a weight. But, if an exact ſyringe can be procur'd, this ſeems to be one of the likelieſt, and leaſt exceptionable ways of meaſuring the force of the air's ſpring.

Fig. 64.

But being unable to procure ſuch a ſyringe as I deſir'd, I got two hollow cylinders to be turn'd, whoſe ſides were of a ſufficient thickneſs to reſiſt the preſſure of the air to be impriſon'd in them; one an inch in diameter, and the other two; their depths were alſo unequal, that the one might receive a much larger bladder than the other. With the leſſer of theſe I made a diligent trial, but found it very difficult to procure a bladder ſmall, and fine enough for the cylinder; and that which we at length procur'd, would not continue ſtanch for many trials; but, after a while, parted with a little air in the well exhauſted receiver, when it was clogg'd with the utmoſt weight it could ſuſtain; but whilſt it continu'd ſtanch, we made one fair trial with it; from whence we concluded, that a cylinder of air of an inch in diameter, and leſs than 2 inches in length, was able viſibly to raiſe a weight of above 10 pound, averdupoize.

At another time, into a hollow cylinder of wood 4 inches deep, and 2 in diameter, furniſh'd with a broad and ſolid body or pedeſtal, we put a lamb's-bladder, very ſtrongly ty'd at the neck; on which we ſet a wooden plug, mark'd with ink, where the edge of the cylinder was contiguous to it; this plug being loaded with weights, amounting to 35 pound, the receiver was exhauſted, till the mark appear'd very manifeſtly above the brim of the cylinder; and then, tho' the ſtring was, by turning the key, quite ſlacken'd, yet the mark on the plug continu'd very viſible. And when ſo much air was let into the receiver, as made the weight depreſs the plug quite beneath the mark, upon pumping out the air again, the weight was, without the help of the turn-key, lifted up; and by degrees all the mark of the plug was rais'd above 3 eighths higher than the edge of the cylinder. Wherefore we ſubſtituted for the 7 pound weight, one of 14; and uſing the ſame bladder, we repeated the experiment; only a little ſupporting the uppermoſt weight by the turn-key, till the bladder had attain'd its expanſion; and then the weight, being gently let go, depreſs'd not the plug ſo low, but that we could yet ſee the mark on it; tho' that part of the plug where the mark was, appear'd manifeſtly more depreſs'd than the other.

An eaſy way of making a ſmall quantity of included air raiſe a great weight.
Fig. 65.

48. We took a braſs veſſel, made like a cylinder, and having one of its orifices exactly cover'd with a flat plate firmly faſten'd to it, the other orifice being wide open; the depth of this veſſel was 4 inches, and the diameter 3 and 3 quarters. To this hollow cylinder we fitted a wooden plug, like one of thoſe deſcrib'd in the foregoing experiment; only it was not quite ſo long, and was furniſh'd with a lip, which we purpoſely made of a conſiderable breadth, that it might afford a ſtable baſis to the weight that ſhould reſt upon it; then taking a middle-ſiz'd limber bladder, ſtrongly ty'd at the neck, but not near full blown; we preſs'd it, by the help of the plug, into the cylinder, that it might the better fit itſelf to the figure thereof; then taking notice by a mark, how much of the plug was extant above the orifice of the veſſel, we laid the weights upon the plug, whoſe lip hinder'd it from being depreſs'd too deep into the cavity of the veſſel; and having convey'd them into the receiver, we found, that a common half-hundred-weight would very ſoon be manifeſtly rais'd by the ſpring of the included air.

Fig. 66.

In another experiment, the bladder in a cylinder 4 inches broad, rais'd 75 pound weight, till the wooden plug diſclos'd the mark deſign'd to ſhew the height at which the air kept the plug, before it was compreſs'd; and this viſibly at the fifth exſuction; and at the ſeventh, that mark was rais'd $\frac{3}{16}$ above the edge of the cylinder. In the gage, where the mercury, in the open air, uſually ſtood, about an eighth above the uppermoſt glaſs mark, it was depreſs'd an eighth below the ſecond mark; and after we let in the air, it was a pretty while before the weight manifeſtly began to ſubſide. The bladder being taken out, and the place it had poſſeſſed in the cylinder being ſupply'd with a ſleeve, or ſome ſuch thing, and the weight laid again upon the plug; we found, that at 24 exſuctions the mercury was depreſs'd to the loweſt mark of the gage; and the 34th or 35th exſuction was made, before the receiver appear'd to be ſo exhauſted, as to ſtop the ſinking of the mercury, which was then above one eighth beneath the loweſt mark. But having cauſed leaden weights to be purpoſely caſt flat, and as broad as we could conveniently put into the receiver, that, by the advantage of this ſhape, we might be able to pile up the more of them, without much danger of their being ſhaken down; we laid ſeveral of them one upon another; and then, the upper part of the receiver growing too narrow to admit any more; we added a weight or two, leſs broad; when exhauſting the receiver, till we perceiv'd by the gage, that the air was manifeſtly withdrawn; we found, by the help of a mark, and a pair of compaſſes, the plug to be ſo far rais'd, that it was concluded, the elevation would have been much greater, if the included air had not found it eaſier to produce ſome leak at the neck of the bladder, than to lift up ſo great a weight; which was about 100 pounds averdupoize.

To ſhew the weight of air to that of water.

49. We weigh'd a ſeal'd bubble in the receiver, and found it above half a grain heavier when much of the air was exhauſted, than when it was full; afterwards we took out this bubble, and found it to weigh 68 grains and a half; then breaking off the ſmall tip of it under water, we found, that the heat, by which it was ſeal'd up, had rarify'd its included air, ſo that it admitted 125 grains of water; for the admitted water and glaſs weigh'd 193 grains and a half. Then filling it full of water, we found it to contain in all 739 grains; for it weigh'd 807 grains and a half; whence it is evident, that the difference between the weight of water and air, was leſs than 1228 and 1. We alſo weigh'd in the receiver a bubble, the glaſs of which amounted to 60 grains; the air that fill'd it, weigh'd, *in vacuo*, $\frac{27}{32}$ of a grain; the water that fill'd it, weigh'd 720 grains and a quarter; ſo that by this experiment, the proportion of the weight of water to air, is as 853 $\frac{17}{27}$ to 1.

But it is ſo deſirable a thing, and may prove of ſuch importance, to know the proportion in weight betwixt air and water, that I ſhall here mention an attempt I made to diſcover it by another way.

A ſmall receiver, being exhauſted of air by the engine, and counterpois'd; whilſt it continu'd ſo, the ſtop-cock was turn'd, and the air re-admitted; which made it weigh 36 grains more than before; and this happen'd alſo upon repeating the experiment.

We next took a ſmall glaſs receiver, fitted with a ſtop-cock; and having exhauſted it of the air, counterpoiz'd it, and let in the outward air; we found the weight of the veſſel to be increas'd, by that admiſſion, 36 grains. This done, we took the receiver, after having well counterpoiz'd it, out of the ſcale; and having apply'd it a ſecond time to the engine, we once more withdrew the air; and then turning the ſtop-cock, to keep out the external air, we took care that none of the cement employ'd to join it to the engine, ſhould ſtick to it; when weighing it again, we found it 35 grains heavier, than when it was laſt counterpoiz'd in the balance. Then we immers'd the ſtop-cock into a baſon of fair water, and let in the liquor, that we might find how much of it would ſucceed in the place of the air drawn out. When no more water was impell'd in, we turn'd the ſtop-cock once more, to keep it from falling out; and then weighing it in the ſame ſcales, we found the water to be 47 ounces, 3 drams, 6 grains; which divided by 35 grains, the weight of the air, equal in bulk to this water, the quotient is nearly 650 grains, for the proportion of weight between air and water, of the ſame bulk, at the time when the experiment was made; the atmoſphere then appearing, by the barometer, wherein the mercury ſtood, at 29 inches 3 quarters, to be very heavy.

Two marbles ſtrongly join'd together, ſeparated by withdrawing the air from them.

50. We took a pair of flat round marbles, each of them two inches, and about 3 quarters in diameter; and having put a little oil between them, to keep out the air; we hung, at a hook faſten'd to the lowermoſt, a pound weight, to ſurmount the coheſion which the tenacity of the oil, and the imperfect exhauſtion of the receiver might give them; then having ſuſpended them in the cavity of a receiver, by a ſtick that lay croſs it, and the engine being made ready to work, we ſhook it more ſtrongly than we concluded it would be by the operation; and beginning to pump out the air, we obſerv'd the marbles to continue join'd, till it was ſo far drawn out, that we ſuſpected they would not ſeparate. But at the 16th ſtroke, upon turning the ſtop-cock, which let the air paſs out of the receiver into the pump, the ſhaking of the engine being over, the marbles ſpontaneouſly fell aſunder; tho' they hung parallel to the horizon, and adhered very firmly together when they were put in; and tho' a weight of above 80 pounds, faſten'd to the lowermoſt marble, might be drawn up, together with the uppermoſt, by virtue of their firm coheſion.

Fig. 67.

But faſtening to the lowermoſt of the two marbles, a weight of a very few ounces, and having cemented a capp'd receiver, with the marbles in it, as before, to the pump; we, by means of a ſtring (whereof one end was ty'd to the bottom of the turn-key, and the other to the uppermoſt marble, and paſſing thro' the hook belonging to the braſs cover) and by turning round the key, drew up the upper marble; and by reaſon of their coherence, the lowermoſt alſo, together with the weight that hung at it. Being thus ſure that the two marbles ſtuck cloſe together, we began to pump out the air; and after a while the marbles fell aſunder.

But having ſo order'd the matter, that the lower could fall but a little way from the other; we were able, by inclining and ſhaking the engine, to place them together again; and then letting in the air haſtily, that by its ſpring it might preſs them hard together; we could not only, by turning the key, make the uppermoſt marble take up the other, and the annex'd weight; but were oblig'd to make a much more laborious exhauſtion of the air, to procure the disjunction of the marbles, this ſecond time, than was neceſſary to do it at the firſt.

And, when the marbles were thus aſunder, and the receiver exhauſted, we did not let in the air, till we made them fall upon one another, as before; but the little highly expanded air, that remain'd in the receiver, having not a ſpring near ſtrong enough to preſs them together, we very eaſily, by turning the key, rais'd the uppermoſt marble alone, without finding it to ſtick to the other. We, therefore, once more join'd the marbles together, and then letting in the external air, found them afterwards to ſtick ſo cloſe, that a very ſtrong man could not ſeparate them.

That 'tis difficult to produce flame without air, ſhewn by an attempt to kindle ſulphur in vacuo.

51. Into a ſmall earthen melting-pot of a cylindrical figure, and well glaz'd, we convey'd a ſmall cylinder of iron, about an inch long, and an inch and half in diameter, made red-hot in the fire; and having ſuddenly exhauſted the receiver, wherein we plac'd them, we let down a piece of paper, containing a convenient quantity of flowers of ſulphur, upon the heated metal; whereby, the paper being immediately deſtroy'd, the included ſulphur would lie upon the iron, whoſe upper part would lie a little concave, to contain the flowers, when melted. But all the heat of the iron, tho' it made the paper and ſulphur ſmoke, would not actually kindle either.

Into a glaſs-bubble of a convenient ſize, furniſh'd with a neck for our purpoſe, we put a little flower of brimſtone, and having exhauſted the glaſs, and ſecur'd it againſt the return of the air; we laid it upon burning coals, where the ſulphur did not take fire, but roſe to the oppoſite part of the glaſs in the form of a fine powder; and that part being turn'd downward, and laid on coals, the brimſtone without kindling roſe again in the form of an expanded ſubſtance, which, when remov'd from the fire, was, for the moſt part, tranſparent like a yellow varniſh.

The efficacy of air in the production of flame.

52. To examine, whether, when a heated iron would not keep the melted brimſtone ſo hot, as was requiſite to make it burn, without air, or with very little, it would yet ſuffice to kindle the ſulphur, if the air had acceſs to it, we made two or three ſeveral trials; and found, that, if ſoon after the flame was extinguiſh'd, the receiver were remov'd, the ſulphur would preſently take fire again, and flame as vigorouſly as before. But, we ſuſpected, that the agency of the air, in the production of the flame, might be ſomewhat leſs, than theſe trials would perſuade; becauſe, by taking off the receiver, the ſulphur was not only expoſed to freſh air, but alſo advantaged, by a free liberty for the avolation of thoſe fumes, which in a cloſe veſſel might be unfavourable to the flame.

And, to try at how great a degree of rarifaction of the air, it was poſſible to make ſulphur flame, by the aſſiſtance of an adventitious heat, we repeated the ſame experiment; the pumping being continued for ſome time after the flame of the melted brimſtone ſeem'd quite extinguiſh'd, till the receiver was judg'd to be very well exhauſted: then, without ſtirring the glaſs, we very warily let in a little air; upon which we could perceive, tho' not a conſtant flame, yet ſeveral little flaſhes, as it were, diſcloſe themſelves, by their blue colour, to be ſulphureous; yet the air that had ſufficed to re-kindle the ſulphur, was ſo little, that two exſuctions drew it out again, and put an entire ſtop to the phenomenon. And, when a little air was cautiouſly let in again, the like flaſhes began again to appear; which, upon two exſuctions more, quite vaniſhed: tho', upon letting in a little freſh air the third time, they, once more, re-appear'd.

An attempt to fire gun-powder in vacuo, *by the ſun's rays.*

53. Having conveniently placed three or four grains of gun-powder in our receiver, and carefully drawn out the air, we threw the ſun-beams, united by a good burning glaſs, upon the powder, and kept them there, for a conſiderable time, to little purpoſe; till, at length, the powder, inſtead of taking fire, only melted, like a metal. And this was not the only experiment we then made, which diſcover'd a great indiſpoſition, even in gun-powder, to be fir'd *in vacuo.*

By means of a hot iron.

54. We took a convenient weight of gun-powder, that was extraordinary ſtrong, and well made; and, having placed a red-hot iron in our receiver, that was capable of holding ſixteen pounds of water; when the air appear'd, by the mercurial gage, to have been well exhauſted, we let down a ſmall piece of thin paper, wherein the powder had been put, till it reach'd the plate; by whoſe heat, we hoped, the paper would be deſtroy'd, and the powder made to go off. But, tho' both of them had been previouſly well dry'd by the fire, no exploſion of the powder enſued; yet there appear'd upon the iron-plate, a broad blue flame, ſurprizingly durable, and reſembling that of brimſtone. At length, taking off the receiver, we found, that the paper, contiguous to the iron, was, in part, deſtroy'd by the heat; but moſt of the grains of the powder ſeem'd unaltered, and retain'd their diſpoſition to be fir'd, notwithſtanding the conſumption made of their brimſtone.

Upon repeating this experiment, we found no exploſion to be made for ſo long a time, that thinking it in vain to wait, we let in the air; and, after we had alſo deſpair'd of any effect from hence, the powder ſuddenly went off, with a great flaſh, and a conſiderable ſhake of the receiver, that was yet ſtanding on the engine: which ſhews, that ſuch experiments ſhould be made with caution; for tho' this receiver would contain two gallons of liquor, the powder here employ'd, weigh'd but one grain.

A heated glaſs emptied of air.

55. Into a large ſtrong glaſs-bubble, we put a few ſmall corns of gun-powder; and, having carefully exhauſted the glaſs, and ſecured it againſt the return of the air, we put it upon live-coals, ſuperficially cover'd with aſhes; by the heat whereof, the ſulphureous ingredient of the powder was, in part, kindled, and burn'd blue for a pretty while, and with a flame conſiderably great; upon the ceaſing whereof, the powder, which, after all, did not take fire, appear'd to have ſent up, beſides the flame, a large quantity of ſulphureous ſublimate, that ſtuck to the upper part of the glaſs: and, being held againſt a lighted candle, it exhibited ſeveral vivid colours, like thoſe of the rain-bow.

And by means of ſparks of fire in vacuo.

56. We took a ſmall, and very ſhort piſtol, and having well faſten'd it, with ſtrings, to a great weight, that was placed upon the iron-plate of our engine, we drew up the cock, and prim'd the pan with dry powder; then, over both the weight and piſtol, we whelm'd a receiver, capable of containing two gallons of liquor; and, having carefully cemented it on, we cauſed the air to be diligently pump'd out; having, before, put in a mercurial gage, to help us to diſcern when it was well exhauſted. Laſtly, ordering the pump to be plied, in the mean time, for fear ſome air ſhould ſteal in; we, by ſhortning a ſtring that was tied to the trigger of the piſtol, did all we could towards firing of the powder in the pan: but tho' the pan were made to fly open, the powder did not go off; then, letting in the air, and cocking the piſtol again, we drew out a little air, to be ſure that the receiver was cloſely cemented on; when, letting in the air at the top of the receiver, and ſtopping it in, we pull'd the trigger again: whereupon, tho' there had been no new powder put into the pan, nor any left in it, but the little that remain'd after the late trial; yet that little readily took fire, and flaſh'd in the pan: which made it the more probable, that, in the former trial, ſparks of fire had been ſtruck out, by the colliſion of flint and ſteel. Beſides, in another trial, made the ſame hour, in the ſame exhauſted receiver, a ſpark, or two, were ſeen to fly out, upon the falling of the cock. It appears therefore, that, notwithſtanding the great indiſpoſition of gun-powder to be reduced into flame, *in vacuo*, yet even ſolid matter is not incapable of being fir'd there, if put into a motion ſufficiently vehement.

Two ways of making Aurum fulminans *go off* in vacuo.

57. The rays of the ſun, being thrown upon ſome *Aurum fulminans*, placed in an exhauſted receiver, made it go off, and violently ſcatter about the cavity of the glaſs a yellow duſt, which other trials, in the free air, made us look upon as particles of the gold, that was the principal ingredient of this odd compoſition.

This experiment we repeated, long after, in another place, with other veſſels, and found the like ſucceſs. And once, in the night-time, putting upon a heated iron, $\frac{1}{8}$ of a grain of good *Aurum fulminans*, of our own preparing, looſely tied up in a piece of thin paper, we found, that after the powder had lain long enough upon the iron, to be thoroughly heated, it went off all together, and with a conſiderable flaſh.

Flame difficultly preſerved without air, in ſulphur.

58. Upon a thick, metalline plate, we put a convenient quantity of flowers of ſulphur; and, having kindled them in the air, ſuddenly convey'd them into a receiver, and made haſte to pump out ſome of the included air; as ſoon as the pump began to be ply'd, the flame appear'd to be ſenſibly decay'd; and continued leſs at every exſuction of the air; and, in effect, expired before the air was quite drawn out. And, upon the ſudden removal of the receiver, it only afforded, for a very little time, ſomewhat more ſmoke in the open air, than it appear'd to do before.

59. Upon a larger cylinder of iron, than the former, made red-hot, we let down a moderate lump of brimſtone, in a receiver moderately exhauſted; when, being kindled, it ſent up a great flame, with large fumes,

However, we ſtill ply'd the pump, drawing out, together with the air, much ſulphureous, and offenſive ſmoke; whereby, though the flame ſeemed ſomewhat gradually impair'd, yet it manifeſtly continued burning much longer than, by the ſhort duration of other flames in our receivers, one could expect. And once, particularly, in making this experiment, the flame laſted, till the receiver was judg'd to be thoroughly exhauſted; and ſome thought it ſo ſurviv'd the exhauſtion, that it went not out for want of air-fewel; the brimſtone appearing, when we took off the receiver, either to have been conſumed by the fire that fed on it, or to have caſually ran off from the iron, the heat whereof had kept it conſtantly melted.

A durable flame of a metalline ſubſtance in vacuo.

60. Having obtain'd a ſaline ſpirit, which, by an uncommon way of preparation, was made exceeding ſharp, and piercing, we put into a vial, capable of containing three or four ounces of water, a convenient quantity of new filings of pure ſteel; which, being moiſten'd in the vial, with a little of the ſaline menſtruum, were, afterwards, drench'd with more; whereupon the mixture grew very hot, and yielded large and fetid fumes. And ſo inflammable was this ſmoke, that upon the approach of a lighted candle, it would readily take fire, and burn with a bluiſh, and ſomewhat greeniſh flame, at the mouth of the vial, for a conſiderable time together; and that, tho' with a little light, yet with more ſtrength than one would eaſily ſuſpect.

This flaming vial, therefore, we convey'd into a receiver, which he who uſed to manage the pump affirm'd, would be exhauſted by about ſix exſuctions; and the receiver being well cemented on, upon the firſt ſuck, the flame ſuddenly appear'd four or five times as great as before; becauſe, as we ſuppoſed, upon the withdrawing of the air, and, conſequently, the weakning of its preſſure, numerous bubbles were produced in the menſtruum; which breaking, ſupply'd the neck of the vial with inflammable ſteams; and theſe, we thought, took fire, with ſome noiſe. Upon the ſecond exſuction of the air, the flame blazed out, as before; and ſo it, likewiſe, did upon the third; but, after that, it went out: nor could we rekindle any fire, by ſuddenly removing the receiver; only we found, that there remain'd ſuch a diſpoſition in the ſmoke to inflammability, that holding a lighted candle to it, a flame quickly enſued.

The flame of ſpirit of wine impregnated with a metal in vacuo.

61. Having ſo united highly rectified ſpirit of wine with a prepared metal, that they would afford a viſible ting'd flame; we put this mixture into a ſmall glaſs lamp, furniſhed with a very ſlender wieck, which the mixture would not burn, whilſt there was liquor enough left to moiſten it well; and putting this lighted lamp into a convenient part of a receiver, able to hold two gallons of water, we made haſte to cement on the glaſs to the engine; yet found not, in two or three ſeveral trials, that, after the pump began to be work'd, ſo little a quantity of ting'd flame laſted more than half a minute.

We alſo obſerv'd, in repeating this experiment, that when the flame began to decay, the turn-key, being now and then drawn almoſt out, the ting'd flame once laſted a minute and half, and another time longer; that longer; that the turn-key being, from the firſt, taken out, the flame laſted two minutes; that, in the ſame caſe, a pipe being bedded in the cement, at the bottom of the glaſs, and open at both ends, each almoſt as big as the orifice fill'd by the turn-key, the ting'd ſpirit ſeem'd to burn as if the flame would have laſted very long, had we permitted it; and laſtly, that the orifice at the top being ſtopt with the turn-key, tho' the pipe were left open at the bottom, it plainly, in a ſhort time, ſeem'd greatly to decay, and ready to expire; but cauſing one to blow in gently at the pipe, with a pair of bellows, tho' this did not keep the flame vigorous, yet it continu'd alive for above four minutes; and then obſerving it to be manifeſtly ſtronger than it was, when we began to refreſh it with the bellows, we ceas'd from blowing, and found, that tho' the glaſs pipe was ſtill left open, yet within about one minute the flame entirely vaniſh'd.

Flame preſerved under water.

62. Eminent writers, both ancient and modern, tell us, without ſcruple, that naptha and camphire will burn under water; but I had never the good fortune to ſee them do ſo; and doubt, theſe writers deliver not what they affirm from experience. And tho', in celebrated authors, I have met with many compoſitions, that will not only burn under water, but be kindled by it; yet I found thoſe I have had occaſion to conſider, to be ſo lamely, or ſo darkly, and ſome of them, I fear, ſo falſly ſet down, that by the following compoſition, how ſlight ſoever it may ſeem, I have been able to do more than with things they ſpeak very promiſingly of.

We took of gun-powder, three ounces; of well burn'd charcoal, one dram; of good ſulphur, or flour of brimſtone, half a dram; of choice ſalt-petre, a dram and half; theſe ingredients being reduc'd to powder, and diligently mix'd without any liquor, we fill'd a large gooſe-quill with it; for the kindling whereof, the open orifice of the quill or pipe, was carefully ſtopt with a convenient quantity of the ſame, made up with as little chymical oil, or water, as would bring it to a fit conſiſtence. This wild-fire we kindled in the air; and the quill, together with a weight to which it was ty'd, to keep it from aſcending, we ſlowly let down to a convenient depth under water; where it would continue to burn, as appear'd by the great ſmoke it emitted, and other ſigns, as it did in the air; becauſe the ſhape of the quill kept the dry mixture from being acceſſible to the water, at any other part than the orifice; and there the ſtream of fired matter iſſued out with ſuch violence, as inceſſantly beat off the neighbouring water, and kept it from entring into the cavity that contain'd the mixture, which therefore would continue burning till it was conſumed.

An odd phenomenon of the flame of a metal in vacuo.

63. In trying to kindle a combuſtible ſubſtance in our exhauſted receiver, it happen'd to fall beſide the iron, whereby our intended experiment was defeated; but whilſt we were conſidering what was to be done on this occaſion, and had not yet let in the air, nor brought in the lights that were remov'd out of the room, we were ſurpriz'd to ſee ſomething burn like a pale blueiſh flame, almoſt in the midſt of the cavity of the receiver; and at firſt ſuſpected it to be ſome deception of the

ſight; but all the by-ſtanders perceiving it alike, and obſerving that it grew very broad, we look'd at it with great attention, and found it to laſt much longer, than I remember to have ſeen any flame in an exhauſted receiver. I ſhould have expected that it proceeded from ſome brimſtone ſticking, unobſerv'd, to a part of the iron we had formerly employ'd to kindle ſulphur, had we not, juſt before, kept it red-hot in the fire. But tho' we much wonder'd whence this flame proceeded, we did not haſten its extinction; and at length, when it expired of itſelf, we let in the air; and perceiv'd, upon the concave part of the iron, which we judg'd to be the place where the flame had appear'd, a piece of melted metal, ſuppoſed to have been faſten'd to the ſtring whereto the fewel we deſign'd to kindle, had been ty'd, in order to let it down the more eaſily; and this made us conceive, that the ſtring happening to be burn'd, by the exceſſive heat of the iron, the piece of metal fell into the cavity of it; and that by the ſame heat, the more combuſtible part, which the chymiſts call the ſulphur, was melted, and kept on fire, and continu'd burning, as we have related. This piece of metal was judg'd to be lead, but having not, formerly, obſerv'd ſuch a diſpoſition in lead, to be inflam'd, I conſider'd it attentively, and perceiv'd, that it was ſome fragment of a mixture of lead and tin, that I caus'd to be melted in a certain proportion. Upon this account, it ſeems, the mixture of the ingredients had acquired ſuch a new texture, as fitted the maſs to afford this odd phenomenon; which argues, that there may be flames of metalline ſulphurs produc'd as eaſily, without the concurrence of the air, as that of common ſulphur; and continue to burn longer than that in our vacuum.

Actual flame propagated with difficulty in vacuo.

64. Having plac'd our cylindrical plate of iron, firſt brought to be red hot, in a receiver, capable of containing two gallons of water; and having alſo diligently pump'd out the air, we kindled a little ſulphur upon the heated plate; and then a piece of dry'd ſpunck, ty'd to a ſtring, was let down to the flame. When the experiment was finiſh'd, and the ſpunck taken out, we found it, in ſeveral places, not manifeſtly alter'd ſo much as in colour; and in thoſe parts that had been moſt expos'd to the flame, it was turn'd to a ſubſtance very different from aſhes; being black, and brittle as tinder, and, like that, exceedingly diſpoſed to kindle upon the touch of fire.

An attempt to make flame kindle camphire without the help of air.

65. Into the ſame receiver, we convey'd the ſame cylindrical plate of iron; and when the air had been thoroughly pump'd out, we let a piece of ſuch brimſtone down upon the hot iron, as would there kindle with the heat. A little above this ſulphur, we had ty'd to the ſame ſtring, a piece of camphire, that being a body exceedingly apt to take fire, or, as it were, to draw it at the flame of lighted brimſtone; but our ſulphur melting with the heat of the iron, dropt from the ſtring it was faſten'd to. As ſoon as it came to the bottom, where it was diſtant from the vehement heat of the metal, the flame expired; but a part of it, that happen'd to ſtick to the ſide of the iron, was inflam'd by it, and the flame reach'd the camphire, without being able to make it blaze.

We alſo attempted to kindle one piece of ſulphur *in vacuo*, by the flame of another, ty'd a little lower on the ſame ſtring, that it might firſt touch the heated iron, and be thereby ſet on fire; but tho' we could find nothing amiſs in the kind of ſulphur we then us'd, yet we were not able, even by a repeated trial, to make it take fire upon the iron; where, nevertheleſs, it melted, and ſeem'd a little to boil.

A third trial was not ſo unſucceſsful; for having in the receiver, well exhauſted, let down a card-match upon a very hot iron, the lower extreme of it was kindled thereby. But tho' the ſulphurated part of the match thus flam'd away, yet the remaining part, which was a mere piece of card, was not thereby turn'd into flame, nor, in moſt places, ſo much as ſenſibly ſcorch'd or black'd, tho' it had been purpoſely dry'd beforehand.

Gun-powder, tho' fired itſelf, fires not the contiguous grains in vacuo.

66. Upon a paper, laid on a convenient part of the plate of the engine, we made a train of dry powder, as long as the glaſs would well cover; then carefully faſtening on the receiver, we exactly pump'd out the air; which done, we took a good burning-glaſs, and about noon caſt the ſun-beams thro' it, upon a part of the train; but the indiſpoſition of the powder to fire was ſo great, that it ſmoked, and melted, without going off. We afterwards employ'd a thinner, and more tranſparent receiver, which ſo little weaken'd the ſun's rays, that being kept obſtinately upon the ſame part of the train, they were able to fire ſeveral parts, one after another, tho' they could not cauſe the flame to propagate; only thoſe parts that were melted, did, at length, kindle, and fly away, leaving the reſt unalter'd; ſo that I found ſeveral little maſſes of diſſolv'd matter, in ſeveral parts of the train, with the powder unchang'd in all the others. And ſome of theſe maſſes were contiguous to grains of the powder, which both appear'd unchang'd, and kindled readily, and flaſh'd all away, as ſoon as I caus'd the burning-glaſs to be applied to them in the open air.

67. For farther confirmation of ſo odd an experiment, I ſhall add, that to try whether by the help of one of thoſe little inſtruments, wherewith the ſtrength of powder is commonly examin'd, we could find any difference made by the abſence and preſence of the air, in the reſiſtance of the inſtrument, or the effects of the powder on it; we faſten'd it to a competently heavy, and commodiouſly ſhap'd weight of lead; and when it was carefully fill'd, and primed with powder, we plac'd it in a receiver of a convenient bigneſs; whence we pump'd out the air after the uſual manner, tho' perhaps with more than uſual diligence. But tho' at length, after the powder had long reſiſted the beams of the ſun, thrown on it by a good double convex-glaſs, it took fire at the touch-hole, and fill'd the receiver with ſmoke; yet this kindled powder could not propagate the flame, to that which was in the box, how contiguous ſoever the parcels were to one another; tho' when the inſtrument was taken out into the air, where the touch-hole appear'd to be free; as ſoon as ever new priming, with the ſame ſort of powder, was put in, the whole very readily went off. And when we caus'd the inſtrument to be new charg'd, and upon its firing

only

only at the touch-hole in the exhauſted receiver, order'd new priming to be added, without ſo much as taking the inſtrument out of the glaſs; tho' afterwards this was clos'd again, but without being exhauſted, the powder, cloſely ſhut up in the glaſs, readily went off; as well that which was in the box or cavity of the powder-tryer, as that which lay on the outward part of it. And this experiment was repeated with the like ſucceſs.

Two different trials with different events, to kindle gun-powder in vacuo.

68. A few corns of gun-powder being included in a very ſmall bubble, freed from air, and ſecur'd againſt the return of it, and then apply'd warily to coals cover'd with aſhes, did neither go off nor burn; but afforded a little yellow powder, that ſeem'd to be ſulphur, ſublimed to the upper part of the glaſs. But two larger bubbles, tho' ſtrong, whereof one had the air but in part, and the other totally evacuated, being provided, each of them, with a greater quantity of powder; a while after they were put upon quick-coals, they were both blown to pieces, with a report almoſt like that of a muſquet; but tho' this was done in a dark place, yet we did not perceive any real flame produc'd.

Experiments, ſhewing the relation betwixt air, and the Flamma vitalis *of animals; and firſt, an animal, included with the flame of ſpirit of wine.*

69. We put a ſpoonful of highly rectify'd ſpirit of wine into a ſmall glaſs lamp, conveniently ſhaped, and purpoſely blown, with a very ſmall orifice, at which we thruſt in a ſlender cotton-wieck; we alſo provided a tall glaſs receiver, in length 18 inches, that contain'd above 20 pints of water. This receiver, which was open at both ends, had its upper orifice cover'd with a braſs plate, faſten'd on very cloſe with good cement; and for the lower orifice, which was far the wideſt, we had provided a braſs plate, furniſh'd with a competent quantity of the cement we employ'd to keep the air out of the pneumatical engine; by means whereof, we could ſufficiently cloſe the lower orifice of our receiver, and hinder the air from getting in at it. We then lighted up the ſmall glaſs lamp, and plac'd it, together with a green-finch, upon the braſs-plate, and in a trice faſten'd it to the lower orifice of the receiver, and then watch'd the event; which was, that within two minutes the flame, after having ſeveral times almoſt diſappear'd, was utterly extinguiſh'd; but the bird, tho' for a while he ſeem'd to cloſe his eyes, as tho' he were ſick, appear'd lively enough at the end of the third minute, when I caus'd him to be taken out.

After he had, by being kept in the free air, recover'd and refreſh'd himſelf, the former trial was repeated; and at the end of the ſecond minute the flame of the lamp went out; but the bird ſeem'd not to be endanger'd by being detain'd a while longer.

After this, we put in, with the ſame bird, two lighted lamps at once, whoſe flames laſted not one whole minute, before they went out together; but the bird appear'd unhurt, after having been kept five or ſix times as long, before we took off the receiver.

In the tall receiver, above-mention'd, we included a mouſe, with a lighted lamp, fill'd with the ſpirit of wine; but before the experiment was near finiſh'd, the mouſe, being at liberty within the glaſs, made ſhift to extinguiſh the flame; which, being revived, without taking out either the lamp, or the animal, the ſpirit of wine burned about a minute longer; during which time, the mouſe appear'd not to be grown ſick, no more than when, for ſome minutes after the extinction of the flame, he had been kept in the ſame cloſe and infected air.

We afterwards placed the ſame mouſe in another receiver, which ſeem'd leſs, by a third, than the former; and in it we, alſo, fix'd a piece of ſlender wax-candle, which continu'd burning in this receiver, but for one minute; and, during that, it emitted much ſmoke: the animal, nevertheleſs appear'd lively, even after we had kept him much longer in that infected air. And the ſame candle, without being taken out, was lighted again, but burned not ſo long as before; yet it ſufficed to darken the receiver, and therefore, probably, much clogg'd the included air: in which, nevertheleſs, the mouſe being kept for eight or ten minutes longer, he appear'd, neither when taken out, nor a while before, to have receiv'd any conſiderable harm from his detention.

The duration of a bird's life, compared with the duration of a burning coal and candle, in vacuo.

70. We included a green-finch, and a piece of lighted candle, in a great capp'd receiver, capable of containing two gallons of water, and very carefully cemented on to the pump: in this glaſs, we ſuffer'd the candle to burn, till the flame expired, which it did within leſs than two minutes; whilſt the bird ſeem'd to be in no danger of ſudden death; and, tho' kept a while longer in that clogg'd and ſmoaky air, he appear'd well, when the receiver was remov'd. We, afterwards, put the ſame bird into the receiver, with a piece of ſmall wax-taper; whoſe flame, tho' it laſted longer than the other, yet the bird out-liv'd it: and, 'twas judg'd, he would have done ſo, tho' the flame had been much more durable. After this, we included the ſame bird, with the former candle, in the receiver, which we had caus'd to be often blown into with a pair of bellows, to drive out the ſmoke, and infected air; and then, beginning to pump, we found, that the flame began to decay more ſuddenly, and the bird to be much more diſcompos'd, than in the former experiments: but ſtill he ſurviv'd the flame, tho' not without convulſive motions.

We repeated the experiment with a piece of wax-taper, and the ſame bird, which, tho' caſt into dangerous ſymptoms, upon the gradual evacuation of the air, out-liv'd, not only the flame, but the ſmoke too, that iſſued from the kindled wieck; a circumſtance that was, alſo, obſerv'd in the preceding trial. Laſtly, having freed the receiver from ſmoke, and ſupply'd it with freſh air; we put in, with the ſame bird, a piece of charcoal, of about two inches in length, and half an inch in breadth, which, juſt before, had been well blown with a pair of bellows; immediately pumping out the air, till none of the fire could be diſcern'd, and till it ſeem'd irrecoverable, by the admiſſion of the outward air; which being afterwards admitted, the bird was, indeed, very ſick, yet capable of a very quick recovery. And this experiment we, with the ſame animal, and re-kindled coal, made over again with the ſame ſucceſs.

Glow-worms, and their luminous matter, in vacuo.

71. We took two glow-worms, that ſhone vividly, eſpecially one of them, whoſe light appear'd ſtrong, and ting'd, as if it had been tranſmitted thro' a blue glaſs; theſe we laid upon a little plate, which we included in a ſmall receiver, of finer glaſs than ordinary; and, having remov'd the candles, that no other light might obſcure that of the inſects, we waited in the dark, till it was conſpicuous, and then ordered the air to be pump'd out; and, upon the very firſt exſuction, there began to be a diminution of the light, which grew gradually dimmer, as the air was more withdrawn; till at length, it quite diſappear'd. This darkneſs having been ſuffer'd to continue a long while in the receiver, we let in the air again, whoſe preſence reſtor'd, at leaſt, as much light as its abſence had depriv'd us of. This experiment was repeated, with the addition of one more of thoſe inſects; when they all three gradually loſt their light, by the exhauſtion of the receiver, and regain'd it by the return of the air. And here we let in the air by degrees, and with an interval or two; and obſerv'd, that as the light was gradually diminiſh'd, upon withdrawing the air; ſo the returning ſplendor was gradually increas'd, as we let more in upon the worms.

72. 'Tis known, that if glow-worms be kill'd whilſt they are ſhining, their luminous matter may continue to ſhine long after 'tis taken out of their bodies. And, having put ſome of that, we took out of the foremention'd inſects, upon a little paper, and included it in the receiver, we employ'd; the candles being remov'd; we perceiv'd it to ſhine vividly, before the pump was ſet on work; and afterwards, to grow dimmer, by degrees, as the air was exhauſted, till at length it quite vaniſh'd: but it reappeared immediately upon the air's return. This experiment was repeated twice more, with the ſame ſucceſs. But we took notice, that the luminous matter, after the air was let in, ſeem'd not only to have regain'd its former degree of light, but to have acquired a greater; as it once happen'd alſo in the experiment made on the living worms. It was ſomewhat ſtrange to obſerve, that ſo very ſmall a quantity of air, as we at firſt let in, before the light revived, was ſufficient to make it become plainly viſible, tho' dim: in which ſtate it continu'd, till we thought fit admit more air.

73. Having, at another time, procured two more of thoſe inſects, whereof one was judg'd to be as large as three ordinary ones; when we had brought them out of the country to *London*, the great worm appear'd to be dead; but, finding him to retain a conſiderable degree of light, in the under part of his tail, we put him into the ſmall receiver formerly mention'd, to try whether, after the death of the animal, the ſhining matter would retain its former properties; and, at the firſt ſtroke of the pump, the light was not aboliſh'd, but continu'd vivid: and ſo it did, when the air being let in, and again withdrawn, the trial was made a ſecond time. I afterwards cauſed the receiver to be exhauſted once or twice more; and at length perceived, that the light began to diminiſh, as the air was withdrawn; and at laſt of all, it ſo diſappear'd, that we could not ſee it: but upon the re-admiſſion of the air, the light ſhone vividly, as before, if not more bright.

This experiment was repeated, with the ſame ſucceſs, and both times, the like happen'd to the light of the dead one, and of the living one, that we included with it; tho' there was this diſparity betwixt them, that the luminous part of the dead worm appeared much larger than that of the living one: and the light of the latter was of a very greeniſh blue; but of the former, a white yellow.

Animals weighed before death and after.

74. A mouſe, weighing about three drams and an half, being put in one ſcale of a very nice balance, was counterpoiſed together with a ſtring put about his neck in a nooſe; and ſoon after, by drawing the ends of that, we ſtrangled him: when we judg'd him quite dead, we weighed him again; and, tho' nothing was ſeen to fall from him; yet, contrary to the received tradition, that bodies are much heavier dead than alive, we found his weight diminiſh'd about $\frac{7}{16}$ of a grain; which, probably, proceeded from the avolation of ſeveral ſubtile particles, upon his violent and convulſive ſtrugglings in death.

Afterwards, in a larger balance, made for nice experiments, we took a very young kitten, between ten and eleven ounces in weight, and caus'd him to be ſtrangled on the ſcale, wherein he had been put. But, being not immediately diſpatch'd, as young animals of this kind are not eaſily deſtroy'd for want of reſpiration; we found him, by that time he was quite dead, lighter, by four grains.

Experiments to ſhew the nature of reſpiration made upon ducks.

75. Nature having furniſh'd water-fowl with a peculiar ſtructure of ſome veſſels about the heart, to enable them, when they dive, to ſuſpend, for a while, the act of reſpiration under water, without prejudice; I thought fit to try, whether ſuch animals would, much better than others, ſuſtain the want of the air in our exhauſted receiver.

We put a full-grown duck into a receiver, whereof ſhe fill'd about a third part; but was very unable to ſtand in any poſture therein: then pumping out the air, tho' ſhe ſeem'd, at firſt, to continue well, ſomewhat longer than a hen in her condition would have done; yet, within one minute, ſhe appear'd much diſcompos'd, and, between that and the ſecond minute, her convulſive motions encreaſed ſo much, that her head hanging careleſly down, ſhe ſeemed to be juſt at the point of death; from which, we preſently reſcued her, by letting in the air. And, to manifeſt, that it was not the cloſeneſs and narrowneſs of the veſſel, that produced this great, and ſudden change, we, ſoon after, included the ſame bird in the ſame receiver; and, having cemented it on very cloſe, we ſuffer'd her to ſtay, thus ſhut up with the air, five times as long as before, without perceiving her to be diſcompos'd; and, ſhe might, probably, have continued longer in the ſame condition.

76. Having procured a duckling, that was yet callow, we convey'd her into the ſame receiver, wherein the former had been included, and obſerved, that tho' for a while, ſhe appear'd not much diſordered, whilſt the air was pumping out; yet before the firſt minute was ended, ſhe gave manifeſt ſigns of being much diſcompoſed: and the operation being continued a while longer, convulſive motions enſued ſo faſt, that, before the

ſecond minute was expired, we were obliged to let in the air, whereby ſhe quickly recover'd.

When the receiver was pretty well exhauſted, the included bird appeared manifeſtly bigger than before the air was withdrawn, eſpecially about the crop; tho' that was very turgid before. We alſo kept the ſame duckling, in the ſame receiver, very cloſe, for above ſix minutes, without perceiving her to grow ſick upon her impriſonment; which yet laſted above thrice the time, that before ſufficed to reduce her to a gaſping condition.

It not being intended, that water-fowl, ſhould, any more than other birds, live in an exceeding rarified air, but only to be able to continue, upon occaſion, under water, it may ſuffice, that the contrivance of theſe parts relating to reſpiration, be barely fitted for that purpoſe.

Upon vipers. Vipers being endowed with lungs, tho' of a different ſtructure from thoſe of other animals; and their blood being, as to ſenſe, actually cold; I thought, it might be worth trying, what effect the abſence of the air would have upon them.

77. *Jan.* 2. We included a viper in a ſmall receiver, and as we drew out the air, ſhe began to ſwell, and afforded us theſe phenomena.

1. It was a long while after we had left pumping, before ſhe began to ſwell, ſo much as forced her to gape, which, afterwards ſhe did.

2. She continued above two hours and a half in the exhauſted receiver, without giving clear proof of her being killed.

3. After ſhe was once ſo ſwelled, as to be compelled to open her jaws, ſhe appeared ſlender and lank again; and yet, very ſoon after, appeared ſwell'd again, and had her jaws disjoin'd as before.

78. Including a viper in a ſmall receiver, we emptied it very carefully; when the viper moved up and down within, as if it were to ſeek for air; and, after a while, foamed a little at the mouth, and left off the foam, ſticking to the inſide of the glaſs: her body ſwelled not conſiderably, and her neck leſs, till a pretty while after we had left off pumping; but afterwards the body and neck grew prodigiouſly tumid, and a bliſter appeared upon her back. An hour and an half after the receiver was exhauſted, the diſtended viper gave, by motion, manifeſt ſigns of life; but we obſerved none afterwards. The tumor reach'd to the neck, but did not ſeem much to ſwell the under-jaw. Both the neck, and a great part of the throat, being held betwixt the eye and the candle, were tranſparent, where the ſcales did not darken them. The jaws remain'd mightily opened, and ſomewhat diſtorted; the *Epiglottis*, with the *Rimula Laryngis*, (which remain'd gaping) was protruded almoſt to the further end of the under-jaw. As it were, from beneath the *Epiglottis*, came the black tongue, and reached beyond it, but ſeemed, by its poſture, not to have any life; and the mouth alſo was grown blackiſh within; but the air being re-admitted, after twenty-three hours in all, the viper's mouth was preſently cloſed, tho' ſoon after, it was open'd again, and continued long ſo; whilſt ſcorching or pinching the tail, made a motion in the whole body, that argued ſome life.

79. *April*

A ſnake.

79. *April* 25. We included an ordinary, harmleſs ſnake, together with a gage, in a portable receiver, which, being exhauſted, and well ſecured againſt the ingreſs of the air, was laid aſide in a quiet place, where it continued from about ten or elven a-clock in the forenoon, till about nine the next morning: and then, looking upon the ſnake, tho' he ſeem'd to be dead, and gave no ſigns of life, upon the ſhaking of the receiver, yet, upon holding the glaſs, at a convenient diſtance, from a moderate fire, he in a ſhort time, manifeſted himſelf alive, by ſeveral tokens; and even by putting out his fork'd tongue. In this condition I left him, and came not to look upon him again, till the next day, early in the afternoon; at which time, he was grown paſt recovery, and his jaws, which were formerly ſhut, gaped exceeding wide, as if they had been ſtretched open by ſome external violence.

Frogs.

80. *Sept.* 9. We included a large, luſty frog, in a ſmall receiver, drew out the air, and left her not very much ſwell'd, and able to move her throat; tho' not ſo faſt as when ſhe breath'd freely, before the exſuction of the air. She continued alive for about two hours, that we took notice of, ſometimes removing from one ſide of the receiver to the other; ſhe ſwell'd more than before, and did not appear, by any motion of her throat, or thorax, to exerciſe reſpiration; but her head was not very much ſwell'd, nor her mouth forced open. After ſhe had remained there above three hours, perceiving no ſign of life in her, we let in the air, upon which the tumid body ſhrunk very much, but ſeem'd not to have any other change wrought in it; and tho' we took her out of the receiver, yet, in the free air, ſhe continued to appear ſtark dead. But, having cauſed her to be laid upon the graſs, in a garden, all night, we found her, the next morning, perfectly alive again.

81. *June* 29. About eleven of the clock in the fore-noon, we put a frog into a ſmall receiver, containing about fifteen ounces, and one quarter, troy-weight, of water; out of which we had, tolerably well drawn the air: (ſo that when we turn'd the cock under water, it ſuck'd in about thirteen ounces, and one quarter, of water,) the frog continued, the receiver being all the while under water, lively, till about five of the clock in the afternoon, when ſhe expired. At the firſt ſhe ſeem'd not to be much alter'd by the exſuction of the air, but continued breathing, both with her throat and lungs.

82. *Sept.* 6. We included into a pretty large receiver, two frogs newly taken; the one not above an inch long, and proportionably ſlender; the other, very large and luſty. Whilſt the air was drawing out, the leſſer frog ſkipp'd up and down very lively, and, ſeveral times, clamber'd up the ſides of the receiver, ſo that he ſometimes wreſted himſelf againſt the ſides of the glaſs. When his body ſeem'd to be perpendicular to the horizon, if not in a reclining poſture, he continued to ſkip up and down a while, after the exſuction of the air; but, within a quarter of an hour, we perceived him to lie ſtark dead, with his belly upwards. The other frog, that was very large and ſtrong, tho' he began to ſwell much upon withdrawing the air, and

ſeemed to be diſtreſs'd, yet he held out half an hour; when it was remarkable, that the receiver, though it had withſtood the preſſure of the outward air, during that ſpace, notwithſtanding a piece of it had been crack'd out, but cemented in again, yet at the end of the half hour, the weight of the outward air ſuddenly beat it in, and thereby gave the impriſon'd frog relief.

83. *Sept.* 11. We convey'd a ſmall frog into a very ſmall portable receiver, and began to pump out the air. At firſt ſhe was lively, but when the air was conſiderably withdrawn, ſhe appear'd to be very much diſquieted; yet not ſo, but that, after the operation was ended, and the receiver taken off, ſhe was perfectly alive, and continued to appear ſo for an hour, tho' the abdomen was very much, and the throat ſomewhat extended; the latter having, alſo, left off the uſual panting motion which is ſuppoſed to argue and accompany the reſpiration of frogs. At the end of about three hours and a quarter, after the removal of the receiver from the pump, the air was let in; whereupon the abdomen, which, by that time, was ſtrangely ſwell'd, not only ſubſided, but ſeem'd to have a great cavity in it, as the throat, alſo, proportionably had; which cavities continued after the frog was gone paſt all recovery.

84. *April* 14. A large frog was convey'd into a plated receiver, and the air being withdrawn, her body, by degrees, diſtended. The receiver, with the gage, was kept under water near ſeven hours; at the end of which, I found it tight, but the frog dead, and exceedingly ſwell'd: upon letting in the air, ſhe became more hollow and lank than ever.

Kitlings.

85. We took a kitling one day old, and put him into a very ſmall receiver, that it might be the ſooner exhauſted; and within about one minute after the air firſt began to be withdrawn, the little animal which, in the mean time, gaſp'd for life, and had ſome violent convulſions, lay as dead, with his head downwards, and his tongue out; but, upon letting in the air, he, in a trice, ſhew'd ſigns of life; and, being taken out of the receiver, quickly recover'd. We then ſent for a kitling of the ſame litter, which being put into the ſame receiver, quickly began, like the other, to have convulſions, and after to lie as dead; but, obſerving very narrowly, I perceived ſome little motions, which made me conclude him alive. And accordingly, tho' we continued pumping, and could not perceive that the engine leak'd, the kitling began to ſtir again, and, after a while, had ſtronger and more general convulſions than before: till at the end of full ſix minutes after the exſuction of the air was begun, the animal ſeeming quite dead, the outward air was re-admitted into the receiver; which not reviving him, as it had done the other, he was taken out of the veſſel, and lay with his mouth open, and his tongue lolling out, without any ſenſible reſpiration and pulſation; till having order'd him to be pinch'd, the pain, or ſome internal motion, produc'd by the external violence, made him, immediately, give manifeſt ſigns of life; tho' there was yet no ſenſible motion of the heart or lungs: but afterwards gaping, and fetching his breath in an odd manner, and with much ſtraining, as I have ſeen ſome fœtus's do, when cut out of the womb,

he, by degrees, within about a quarter of an hour, recover'd. We afterwards, ſent for another, kitten'd at the ſame time; and incloſing that, alſo, in the receiver, obſerv'd the violent convulſions, and, as it were, gaſping for breath, into which he began to fall, at the ſecond or third ſuck, ended in a ſeeming death, within about a minute and a half. But, cauſing the pump to be ply'd, the kitling gave manifeſt ſigns of life, after he had endured ſeveral convulſions, as great as thoſe of the firſt fit, if not greater. When ſeven minutes, from the beginning of the exhauſtion, were compleated, we let in the air; upon which, the little creature, that ſeem'd ſtark dead before, made us ſuſpect he might recover: but, tho' we took him out of the receiver, and put *Aqua Vitæ* into his mouth, yet he, irrecoverably, died in our hands.

Experiments upon the air uſually harbour'd in the pores of water, &c.

86. To determine the quantity of air harbour'd in the pores of fluids, ſeems as difficult as it would be uſeful. To ſhew how little the air, contain'd in water, would appear to leſſen the bulk of the water, if it were ſuffer'd to fly away in an open tube; we let it eſcape, in an exhauſted receiver, without any artifice to catch it: in which trial, the water did not part with any thing of its bulk, that made a diminution ſenſible to the eye. We, therefore, endeavour'd to make this loſs viſible, by ſome other experiments.

A glaſs-tube, ſeal'd at one end, and about thirty-ſix inches in length, being fill'd with water, and inverted into a glaſs-veſſel, not two inches in diameter, and but a quarter of an inch, or little more, in depth; the glaſſes were convey'd into a fit receiver, and the air leiſurely pump'd out, and ſomewhat ſlowly re-admitted; when, the numerous bubbles, that had aſcended, during the operation, conſtituted, at the top, an aerial aggregate, amounting to $\frac{3}{10}$, wanting about an hundredth part of an inch.

87. Preſently after, another tube was fill'd with the ſame water, and inverted; when, the water, being drawn down to the ſurface of that in the veſſel, and the air let in again, the water was impell'd up to the very top, within a tenth, and half a tenth of an inch.

The latter tube was forty-three inches and a half above the ſurface of the ſtagnant water; the air, collected out of the bubbles, at the top of the water, was, the firſt time, above three quarters of an inch; and the ſecond time we eſtimated it, at one half, and one ſixteenth. The firſt time, the water, in the pipe, was made to ſubſide full as low as the ſurface of the ſtagnant water; the ſecond time, the loweſt that we made it ſubſide, ſeem'd to be four or five inches above the ſurface of the water in the open veſſel.

But the air, at the top of the tube, poſſeſs'd more room than its bulk abſolutely required; becauſe it was ſomewhat defended from the preſſure of the atmoſphere, by the weight of the ſubjacent cylinder of water, which perhaps, was about three or four feet long.

88. We provided a clear round glaſs, furniſh'd with a pipe, or ſtem, about nine inches in length; the globular part of the glaſs being, on the outſide, about three inches and a half in diameter: the pipe of this glaſs

was, within an inch of the top, melted at the flame of a lamp, and drawn out, for two or three inches, as ſlender as a crow's quill, that the decreaſe of the water, upon the receſs of the air, harbour'd in its pores, might be the more eaſily obſerv'd, and eſtimated. Above this ſlender part of the pipe, the glaſs was, nearly, of the ſame ſize with the reſt of the pipe; that the aerial bubbles, aſcending thro' the ſlender part, might there find room to break, and ſo prevent the loſs of any part of the water.

This veſſel being filled, till the liquor reach'd to the top of the ſlender part; where, not being uniformly enough drawn out, it was ſomewhat broader than elſewhere; we convey'd the glaſs, together with a pedeſtal for it to reſt upon, into a tall receiver; and, pumping out the air, there diſcloſed themſelves numerous bubbles, aſcending nimbly to the upper part of the glaſs, where they made a kind of froth: but, by reaſon of the figure of the veſſel, they broke at the top of the ſlender part, and ſo never came to overflow.

This done, the pump was ſuffer'd to reſt a while, to give the aerial particles, lodg'd in the water, time to ſeparate themſelves, and emerge; when, the pump was ply'd again. Theſe viciſſitudes of pumping, and reſting, laſted for a conſiderable time; till, at length, the bubbles began to be very rare; ſoon after which, the external air was let into the receiver; when, it appear'd ſomewhat ſtrange, that notwithſtanding ſo great a multitude of bubbles as had eſcaped out of the water, I could not, by attentively comparing the place where the ſurface of the water reſted at firſt, (to which a mark had been affix'd) with that where it now ſtood, diſcern the difference to amount to above an hair's breadth; and the chief operator in the experiment profeſs'd he could perceive no difference at all.

89. Filling a glaſs of the ſame ſhape, and much of the ſame bigneſs as the former, with claret-wine, and placing it upon a convenient pedeſtal, in a tall receiver, we caus'd ſome of the air to be pump'd out; whereupon there emerged thro' the ſlender pipe, ſo very great a multitude of bubbles darted, as it were, upwards, as both pleas'd and ſurpriz'd us; but forc'd us to go warily to work, for fear the glaſs ſhould break, or the wine overflow; wherefore, we, ſeaſonably, left off pumping, before the receiver was near exhauſted, and ſuffer'd the bubbles to get away as they could, till the danger was paſt; then, from time to time, we pump'd a little more air out of the receiver; the withdrawing a moderate quantity of air at a time ſufficing, even at the latter end, to make the bubbles copiouſly and ſwiftly aſcend, for above a quarter of an hour together.

The little inſtrument made uſe of in theſe experiments, being deſign'd to examine, among other things, the quantity of bubbles lurking in ſeveral liquors, may be apply'd to ſpirit of wine, and chymical oils. And ſome circumſtances of our trials made us think, that it might be worth examining, what kind of ſubſtance may be obtain'd by this way of treating aerial and ſpirituous corpuſcles.

Shell-fiſh in an exhauſted receiver.

90. An oyſter, being put into a very ſmall receiver, and kept there long enough, to have, ſucceſſively, kill'd three or four birds, or beaſts, &c. was not thereby kill'd, nor, for ought we could perceive, conſiderably diſturb'd; only at each ſuck we perceiv'd, that the air, contain'd between the two ſhells, broke out at their commiſſure; as we concluded from the foam which, at thoſe times, came out all around that commiſſure. About twenty-four hours after, I found, that both this oyſter, and another, that had been put, at the ſame time, into the receiver, were alive.

On the ſame day we put a pretty large craw-fiſh into a large receiver, and found, that tho' he had been before injur'd by a fall, yet he ſeem'd not to be much incommoded, by being included, till the air was, in great meaſure, pump'd out; and then his former motion preſently ceas'd, and he lay as dead, till, upon letting in a little air, he ſoon began to move afreſh; and, upon withdrawing the air again, he preſently, as before, became moveleſs. Having repeated the experiment two or three times, we took him out of the receiver, when he appear'd not to have ſuffer'd any harm.

91. Having put an oyſter into a vial, full of water, before we included it in the receiver, that thro' the liquor the motion of the bubbles, expected from the fiſh, might be the better ſeen, and conſider'd; this oyſter prov'd ſo ſtrong, as to keep itſelf cloſe ſhut, and repreſs'd the eruption of the bubbles, that, in the other, forc'd open the ſhells, from time to time; and kept in its own air, as long as we had occaſion to continue the trial.

92. A craw-fiſh, that was thought more vigorous, being ſubſtituted in the place of the former craw-fiſh, tho' once he ſeem'd to loſe his motion together with the air; yet, afterwards, he continu'd moving in the receiver, in ſpight of our pumping.

A ſcale-fiſh in an exhauſted receiver.

93. We took a receiver, ſhaped almoſt like a bolt-head, containing near a pint; and the globular part of it, being almoſt half full of water, we put into it, at the orifice, a ſmall gudgeon, about three inches long; which when it was in the water ſwam nimbly up and down therein: then, having drawn out the air, ſo that about nineteen parts out of twenty, or more, were exhauſted; we ſecur'd the return of the air from injuring our experiment.

Now the neck of the glaſs, being very long, tho' there appear'd numerous bubbles all about the fiſh; yet the reſt of the water, notwithſtanding the withdrawing of ſo much air, emitted no froth, and but few bubbles.

The fiſh, both at his mouth and gills, for a long time, diſcharg'd ſuch a quantity of bubbles, as appear'd ſtrange; and for about half an hour, when ever he reſted a while, new bubbles would adhere to many parts of his body, (as if they were generated there) eſpecially about the fins and tail; ſo that he would appear almoſt beſet with bubbles; and if, being excited to ſwim, he was made to ſhake them off, he would quickly, upon a little reſt, be beſet with new ones, as before.

Almoſt all the while, he would gape, and move his gills, as before he was included; tho', towards the end of the time I watch'd him, he often neither took in, nor emitted any aerial particles that I could perceive.

After a while, he lay almoſt conſtantly with his belly upwards; and yet, would, in that poſture, ſwim briſkly as before. Nay, ſoon after, he ſeem'd to be more lively than at firſt putting in.

In about an hour and a half after he had been ſeal'd up, I found him almoſt free from bubbles, with his belly upwards, and ſeeming ſomewhat tumid, yet lively as before. But, an hour and a quarter after that, he ſeem'd to be moveleſs, and ſomewhat ſtiff; yet, upon ſhaking the glaſs, obſerving faint ſigns of life in him, by ſome languid motions he attempted to make, when excited; I open'd the receiver, under water, to try if that liquor, and air, would recover him; when, the external water ruſhing in, till it had fill'd the vacant part of the ball, and the greateſt part of the ſtem, the fiſh ſunk to the bottom of it, with a greater appearance than ever of being alive: in which ſtate, after he had continu'd a pretty while, I, by the help of the water he ſwam in, got him thro' the pipe, into a baſon of water, where he gave more manifeſt ſigns of life. But, yet, for ſome hours, he lay on one ſide or other, without being able to ſwim, or reſt on his belly, which appear'd very much ſhrunk in.

All the while he continu'd in the baſon of water, tho' he mov'd his gills, as before he had been ſeal'd up; yet I could not perceive, that he did, even in his new water, emit, as formerly, any bubbles; tho', two or three times, I held him by the tail in the air, and put him into the water again; where, at length, he grew able to lie conſtantly upon his belly; tho' that retain'd much of its former lankneſs. And he lived, in the baſon, eight or ten days longer; tho' ſeveral gudgeons, ſince taken, died there in a much leſs time.

Two animals, with large wounds in their abdomen, included in the pneumatical receiver.

94. *Sept.* 12. A ſmall bird, having the abdomen open'd almoſt from flank to flank, without injuring the guts, was put into a little receiver, and the pump being ſet a-work, continued, for ſome little time, without giving any ſigns of diſtreſs; but at the end of about a minute and an half from the beginning, ſhe began to have convulſive motions in the wings: and, tho' the convulſions were not univerſal, or appeared violent, as is uſual in other birds when the air is withdrawn, yet, at the end of two full minutes, letting in the air, and then taking off the receiver, we found the bird irrecoverable, tho' there appear'd no notable alteration in the lungs; and the heart, or, at leaſt, the auricles of it, continued beating for a while after.

95. We took, alſo, on the ſame day, a pretty large frog, and having, without violating the lungs, or the guts, made two ſuch inciſions in the abdomen, that the two curl'd bladders, or lobes of the lungs, came out, almoſt totally, at them; we ſuſpended the frog, by the legs, in a ſmall receiver, and, after we had pump'd out a large part of the air, the animal ſtruggled very much, and ſeem'd to be much diſorder'd; and when the receiver was well exhauſted, ſhe lay ſtill, for a while, as if ſhe had been

dead; the abdomen and thigh being very much ſwell'd, as if ſome rarify'd air, or vapour, forcibly diſtended them. But as, when the frog was put in, one of the lobes was almoſt full, and the other almoſt ſhrunk up; ſo they continu'd to appear, after the receiver had been exhauſted: but upon letting in the air, not only the body ceas'd to be tumid, but the plump bladder appear'd, for a while, ſhrunk up as the other; and the receiver being remov'd, the frog preſently revived, and quickly began to fill the lobe again with air.

The motion of the ſeparated heart of a cold animal in the exhauſted receiver.

96. The heart of an eel being taken out, and laid upon a plate of tin, in a ſmall receiver; when we perceiv'd it to beat there, as it had done in the open air, we exhauſted the veſſel, and ſaw, that tho' the heart grew very tumid, and, here and there, ſent out little bubbles, yet it continued to beat as manifeſtly as before, and ſeemed to do ſo more ſwiftly; as we tried by counting the pulſations it made in a minute, whilſt it was in the exhauſted receiver, and when we had re-admitted the air, and alſo, when we took it out of the glaſs, and ſuffer'd it to continue its motion in the open air. The heart of another eel, being, likewiſe, taken out, continu'd to beat in the empty'd receiver, as the other had done.

97. The heart of another eel, after having been included in an exhauſted receiver, and then accurately ſecured from leaking, tho' it appear'd very tumid, continu'd to beat there an hour; after which, finding its motion very languid, and almoſt ceas'd; by breathing a little upon that part of the glaſs where the heart was, it quickly regain'd motion, which I obſerv'd a while; and, an hour after, finding it almoſt quite gone, I was able to renew it, by the application of a little more warmth. At the end of the third hour, a bubble, that appear'd to be placed between the auricle and the heart, ſeem'd to have, now and then, a little trembling motion; but it was ſo faint, that I could no more, by warmth, excite it, ſo as plainly to perceive the heart to move: wherefore, I ſuffer'd the outward air to ruſh in, but could not diſcern, that, thereby, the heart regain'd any ſenſible motion, tho' aſſiſted with the warmth of my breath and hands.

The times wherein animals may be kill'd by drowning, or withdrawing of the air, compared.

98. *Sept.* 10. A green-finch, having his legs and wings tied to a weight, was gently let down into a glaſs body fill'd with water; the time of his total immerſion being mark'd. At the end of half a minute after that time, the ſtrugglings of the bird ſeeming finiſh'd, when being ſuddenly drawn up again, he was found quite dead.

A ſparrow, very luſty and quarrelſome, was tied to the ſame weight, and let down after the ſame manner; but tho' he ſeem'd to be more vigorous under water than the other bird, and continued ſtruggling almoſt to the end of half a minute, from the time of his total immerſion; (during which, there aſcended, from time to time, large bubbles from his mouth) yet, being drawn up as ſoon as ever the half minute was compleated, we found him, to our wonder, irrecoverably gone.

99. A ſmall mouſe, being held under water by the tail, emitted, from time to time, ſeveral aeriel bubbles out of his mouth; and, at laſt, as a ſpectator affirm'd, at one of his eyes: being taken out, at the end of half a minute,

nute, and a few ſeconds, he yet retain'd ſome motions: but they prov'd only convulſive ones, which, at laſt, ended in death.

100. We ſo tied a conſiderable weight of lead to the body of a duck, as not to hinder her reſpiration, yet keep her under water; which we had found a ſmall weight unable to do, by reaſon of her ſtrength; and even a great one, if ty'd only to her feet, in ſuch a middle-ſized veſſel as ours was; becauſe of the height of her neck and beak. With this clog, the duck was put into a tub full of clear water, under the ſurface whereof, ſhe continued quietly for about a minute; but afterwards began to be much diſturb'd: the fit being over, and perceiving no motion in her, at the end of the ſecond minute, we took her out of the water; and, finding her in a good condition, after we had allow'd her ſome breathing-time, to recruit herſelf with freſh air, we let her down again into the tub, which, in the mean time, had been fill'd with freſh water; leſt the other, which had been troubled with the ſteams and foulneſs of the body of the animal, might either haſten her death, or hinder our perceiving what ſhould happen.

The bird being thus under water, after a while, began, and, from time to time, continued to emit bubbles at her beak. There, alſo, came out at her noſtrils ſeveral real bubbles, from time to time; and when the animal had continued about two minutes under water, ſhe began to ſtruggle very much, and to endeavour either to emerge, or change poſture; the latter of which, ſhe had liberty to do, but not the former. After four minutes, the bubbles came much more ſparingly from her: then, alſo, ſhe began to gape, from time to time; which we had not obſerv'd her to do before, but without emitting bubbles; and ſo ſhe continued gaping till near the end of the ſixth minute; at which time all her motions, ſome whereof were judg'd convulſive, and others that had been excited by rouzing her, appear'd to ceaſe, and her head to hang careleſly down, as if ſhe were quite dead. Notwithſtanding which, we, for greater ſecurity, continued her under water a full minute longer; and then, finding no ſigns of life, we took her out; when, being hung by the heels, and gently preſs'd in convenient places, ſhe was made to void a conſiderable quantity of water: but whether any of it had been received into the lungs themſelves, we wanted time and opportunity to examine. All the means we uſed to recover the bird, proving ineffectual, we concluded, ſhe had been dead a full minute before we removed her out of the water: ſo that, even this water-fowl, was not able to live in cold water, without taking in freſh air, above ſix minutes.

101. A duckling, having a competent weight ty'd to her legs, was let down into a tub of water, which reached not above an inch or two above her beak: during the moſt part of her continuance wherein, there came out numerous bubbles at her noſtrils; but there ſeem'd to proceed more and greater, from a certain place in her head, almoſt equi-diſtant from her eyes, tho' ſomewhat leſs remote from her neck than they. Whilſt ſhe was kept in this condition, ſhe ſeem'd, frequently, to endeavour at diving lower under water; and, after much ſtruggling, and frequent gaping, ſhe had, ſe-

veral convulſive motions, and then let her head fall down backward, with her throat upwards. To this moveleſs poſture ſhe was reduc'd at the end of the third minute, if not ſooner; but a while after there appear'd a manifeſt tremulous motion in the two parts of her bill; which continu'd for ſome time, and was perhaps convulſive; but this alſo ceaſing at the end of the fourth minute, the bird was taken out, and found irrecoverable.

102. A viper, that we kept in an exhauſted receiver, till concluded to have been quite dead, was, nevertheleſs, not thrown away, till I had try'd what could be done, by keeping her all night in a glaſs-body, and a warm digeſtive furnace; upon which, this viper was found, the next morning, not only reviv'd, but very lively.

We therefore put her into a tall glaſs-body of water, fitted with a cork to its orifice, and depreſs'd it with a weight, ſo that ſhe could have no air. In this caſe we obſerv'd her, from time to time; and after ſhe had been duck'd a while, ſhe lay, with very little motion, for a conſiderable time. After an hour and a quarter, ſhe often put out her black tongue; at the end of near four hours, ſhe appear'd lively; and, as I remember, about that time alſo put out her tongue; ſwimming all this while, as far as we obſerv'd, above the bottom of the water. At the end of about ſeven hours, or more, ſhe ſeem'd to have ſome life; her poſture being manifeſtly changed in the glaſs, from what it was a while before. Not long after, ſhe appear'd quite dead; her head and tail hanging down moveleſs, and directed towards the bottom of the veſſel; whilſt the middle of her body floated as much as the cork would permit it.

Animals in air brought to a conſiderable degree of rarifaction.

103. In the generality of our pneumatical experiments upon animals, it ſuited with our purpoſe, to rarify the air as much, and as ſuddenly as we could; but I had other trials in view, wherein an extraordinary degree of rarifaction, yet not near the higheſt to which the air might be brought by our engine, ſeem'd likelieſt to conduce; as particularly to afford ſome light in the nature of thoſe diſeaſes, that are thought primarily to affect reſpiration, or its organs.

Wherefore having gages, by help of which ſuch experiments might be much the better perform'd, I attempted ſeveral of them in the following manner.

Aug. 16. A linet being put into a receiver, able to hold about 4 pints and a half of water, the glaſs was well clos'd with cement and a cover; but none of the air drawn out with the engine, or otherwiſe. And tho' no new air was let in, nor any change made in the impriſon'd air, yet the bird continu'd there 3 hours, without any apparent approach to death; and tho' ſhe ſeem'd ſomewhat ſick, yet being afterwards taken out, ſhe recover'd, and liv'd ſeveral hours.

104. *Aug.* 18. From the receiver above-mention'd, we drew about half the air, whilſt a linet was in the glaſs; and in that rarified air (which appear'd by a gage to continue in the ſame ſtate) the bird liv'd an hour and near a quarter before ſhe ſeem'd in danger of death; after which the air

being let in, without taking off the receiver, ſhe manifeſtly recover'd, and leap'd againſt the ſide of the glaſs; and being taken out into the open air, ſhe flew out of my hand to a conſiderable diſtance.

105. *Sept.* 9. Into a receiver, able to hold about 4 pints and a half of water, we convey'd a lark, together with a gage, by the help whereof we drew out $\frac{3}{4}$ of the air; then obſerving the bird, we perceiv'd it to pant very much. Having continu'd thus for a little above a minute and a half, the bird fell into a convulſive motion, that caſt it upon the back. And altho' we made great haſte to let in the air, yet before the expiration of the ſecond minute preceding the convulſion, the lark was gone paſt all recovery, tho' various means were uſed to effect it.

106. *Sept.* 9. Preſently after, we put into the ſame receiver, a greenfinch; and having withdrawn half the air, we ſoon began to obſerve the bird, and took notice, that within a minute after, ſhe appear'd to be very ſick; and ſhaking her head, vomited a certain ſubſtance againſt the inſide of the glaſs. Upon this evacuation the bird ſeem'd to recover, and continue pretty well, but not without panting, till about the end of the fourth minute; when growing very ſick, ſhe vomited again, but much more unqueſtionably than before; and ſoon after, eat up again a little of her vomit; upon which ſhe very much recover'd. And tho' ſhe had in all 3 fits of vomiting, yet for the laſt 7 or 8 minutes that we kept her in the receiver, ſhe ſeem'd to be much more lively than was expected; which may, in part, be attributed to a little air that by an accident got in, tho' it were immediately pump'd out again. At the end of a full quarter of an hour from the firſt exhauſtion, the bird appearing not likely to die in a great while, we took her out.

107. *April* 12. A new-caught viper was included, together with a gage, in a portable receiver, able to hold about 3 pints and an half of water. This veſſel being exhauſted, and ſecured againſt the return of the air, the animal was obſerv'd, from time to time, not only to be alive, but nimbly to put out, and to draw back her tongue, for about 36 hours after ſhe was ſhut up; we therefore continu'd the veſſel longer in the ſame ſhady place; where, over night, at the end of 60 hours, ſhe appear'd very dull and faint, and not likely to live much longer. And the next, by the afternoon, I found her ſtark dead, with her mouth open'd to a ſtrange wideneſs; wherefore ſuffering water to be impell'd, by the outward air, into the cavity of the receiver, we found, by the water that was driven in, and afterwards pour'd out again and meaſur'd, that five parts in ſix of the air in the veſſel had been pump'd out; ſo that in an air rarified, till it expanded itſelf to five or ſix times its uſual dimenſions, our viper was able to live 60 hours, and perhaps might have done ſo longer.

Animals in the ſame parcel of air changed, as to rarity and denſity.

108. In the preceding experiments, the animals were recover'd from a gaſping condition, by letting in freſh air, and not the ſame that had been withdrawn from them; wherefore I thought proper to try, whether the ſame portion of air, without being renew'd, would, by being expanded much beyond its uſual degree, and reduc'd to it again, ſerve to bring an

animal to near the point of expiring, and revive him again; ſince, by the ſucceſs of ſuch a trial, it would notably appear, that the bare change of the conſiſtence of the air, as to rarity and denſity, may ſuffice to produce the above-mention'd effects.

We included a mouſe in a fine, limber, clear bladder, made more tranſparent by oil rubb'd on the outſide, that the ſmell of it might leſs offend the animal to be included, clipping off as much of the bladder at the neck, as we judg'd abſolutely neceſſary for letting in a mouſe; we alſo provided a round ſtick, ſomewhat leſs than the orifice; that the wood being laid over with a cloſe and yielding cement, we might tie the bladder faſt, and cloſe enough, upon the ſtopple thus fitted. In the bladder was left as much air as we thought might ſuffice him, for the time the experiment was to laſt. Then putting this limber, or dilatable receiver into an ordinary one of glaſs, and placing this engine near a window, that we might ſee thro' both of them; the air was, by degrees, pump'd out of the external receiver, and thereupon the air included in the bladder, proportionably expanded itſelf, and ſo diſtended the internal receiver, till being arriv'd at a degree of rarifaction, which render'd it unfit for reſpiration, I perceiv'd ſigns in this animal, of his being in great danger of ſudden death. Whereupon the outward air being haſtily let into the external receiver, compreſs'd the ſwell'd bladder to its former dimenſions, and thereby the included air to its former denſity; by which means the mouſe was quickly revived. Having given him ſome convenient reſpite, the experiment was repeated with the like ſucceſs.

An attempt to prevent the neceſſity of reſpiration, by the production or growth of animals, in vacuo.

109. We put a large parcel of tadpoles, with a convenient quantity of water, into a portable receiver, of a round figure, and obſerv'd, that at the firſt exſuction of the air, they roſe to the top of the water; tho' moſt of them ſubſided again, till the next exſuction raiſed them. They ſeem'd, by their active and wrigling motion, to be very much diſcompos'd. The receiver being exhauſted, they all of them continued moving at the top of the water; and tho' ſome of them ſeem'd to endeavour to go to the bottom, and dived part of the way, eſpecially with their heads, yet they were immediately buoy'd up again. Within an hour, or a little more, they were all moveleſs, and lay floating on the water; wherefore I open'd the receiver; upon which the air ruſhing in, almoſt all of them preſently ſunk to the bottom, but none of them recover'd life.

110. We afterwards included a leſs number of tadpoles in a ſmaller glaſs, which was alſo exhauſted, with the like circumſtances as the former. And when I found the other tadpoles to be dead, I haſted to theſe, which did not, except perhaps one, give any ſigns of life; but upon letting in the air, theſe having not been long kept from it, ſome few of them recover'd, and ſwam up and down lively enough; tho', after a while, theſe alſo died.

111. I repeated the ſame experiment in a portable receiver, of a convenient kind; and tho' after the exhauſtion was perfected, the tadpoles, for a while, mov'd briſkly enough, on the top of the water only, yet at

the end of an hour, they ſeem'd to be, all of them, quite dead, but continu'd floating. And, though, within half an hour after that, I let in the air; yet all the effect of it was, that the moſt of them immediately ſunk to the bottom, as the reſt, ſoon after, did; none of them, that I could obſerve, recovering vital motion.

112. We procured, by preſerving ſome rain-water, four or five of thoſe odd inſects, whereof gnats have, by ſome, been obſerved to be generated about the end of *Auguſt*, or beginning of *September*. Theſe, for ſome weeks, live all together in the water, as tadpoles do; ſwimming up and down therein, till they are ripe for a tranſmigration into flies: but including them with ſome of their water, in a ſmall glaſs receiver, which being exhauſted, and very exactly cloſed, we kept, in a ſouth-window; theſe little creatures continued to ſwim up and down therein, for ſome few days, without ſeeming to be much incommoded; but at length, and all much about the ſame day, they put off the habit they had, whilſt they lived as fiſhes, and appeared with their *Exuviæ*, or caſt-coats under their feet; ſhewing themſelves to be perfect gnats, that ſtood, without ſinking, upon the ſurface of the water, and diſcovering themſelves to be alive, by their motion, when they were excited to it; but I could not perceive them to fly in that thin medium: to which inability, whether the viſcoſity of the water might contribute, I know not; tho' they lived a pretty while, till hunger, or cold deſtroyed them.

The expanſion of the blood and other animal fluids.

113. The warm blood of a lamb or a ſheep, being taken as it was, immediately, brought from the butcher's, where the fibres had been broken, to hinder the coagulation, was, in a wide-mouth'd glaſs, put into a receiver, made ready for it; and the pump being ſet on work, the air was diligently drawn out: but the operation was not always, eſpecially at firſt, ſo early manifeſt, as the ſpirituouſneſs of the liquor would make one expect; yet, after a long expectation, the more ſubtile parts of the blood would begin to force their way thro' the more clammy, and ſeem to boil in large cluſters, ſome as big as great beans or nutmegs; and, ſometimes, the blood was ſo volatile, and the expanſion ſo vehement, that it boiled over the containing glaſs; of which, when it was put in, it did not fill above a quarter.

114. Having, alſo, included ſome milk, warm from the cow, in a cylindrical veſſel, about four or five inches high, tho' the pump was not long ply'd, before any intumeſcence appear'd, yet, afterwards, when the external air was fully withdrawn, the milk began to boil, in a way, that was not ſo eaſy to deſcribe, as pleaſant to behold: and this it did for a pretty while, with ſo much impetuoſity, that it threw ſeveral of its parts out of the wide-mouth'd glaſs that contain'd it; tho' there were not above two or three ounces of the liquor, which only half filled the glaſs.

A yet greater diſpoſition to intumeſcence, we thought, we obſerv'd in the gall; which was but ſuitable to the viſcoſity of its texture.

The two laſt experiments were made with a deſign to ſhew how far the deſtructive operation of our engine, upon the included animals, might be imputed to the withdrawing of the air, whereby the little bubbles generated in the blood, juices, and ſoft parts of the body, may, by their vaſt number, and conſpiring diſtenſion, variouſly contract the veſſels in ſome places, and ſtretch them in others; eſpecially the ſmaller, that convey the blood and nouriſhment; and ſo, by choaking up ſome paſſages, and vitiating the figure of others, diſturb, or hinder the due circulation of the blood: for ſuch diſtenſions may cauſe pains in ſome nerves, and membranous parts, which, by irritating them into convulſions, may haſten the death of animals, and deſtroy them ſooner by that irritation, than they would be deſtroyed by the bare abſence or loſs of what the air is neceſſary to ſupply them with. And, to ſhew, that this production of bubbles reaches, even to the very minute parts of the body, I ſhall add, that I once obſerved in a viper, furiouſly tortured in our exhauſted receiver, the creature had a conſpicuous bubble moving to and fro, in the aqueous humour of one of its eyes.

115. To ſhew, that not only the blood and liquors, but alſo the other ſoft parts, even in cold animals, have aerial particles latent in them; we took the liver and heart of an eel, as alſo the head and body of another fiſh of the ſame kind, cut aſunder, croſs ways, beneath the heart; and putting them into a receiver, and upon withdrawing of the air, we perceiv'd, that the liver manifeſtly ſwell'd every way; and that both the upper part and lower of the fiſh, did ſo, likewiſe. At the place, where the diviſion had been made, there came out, in each portion of the fiſh, various bubbles; ſeveral of which ſeemed to riſe from the *Medulla Spinalis*, the cavity of the back-bone, or the adjoining parts: and the external air being let in, both the portions of the eel preſently ſunk; ſome of the ſkin ſeeming to be grown flaccid in each.

The power of uſe to enable animals to ſupport themſelves in air, by rarifaction made unfit for reſpiration.

116. We included, in a vial with a wide neck, (the whole glaſs being able to contain about eight ounces of water,) a ſmall young mouſe; then ty'd ſtrongly upon the upper part of the glaſs's neck a fine thin bladder, out of which the air had been carefully expreſs'd; and convey'd this veſſel into a middle-ſiz'd receiver, in which, we alſo placed a mercurial gage. This done, the air was, by degrees, pump'd out, till it appear'd by the gage, that there remained but a fourth part in the external receiver; whereupon, the air in the internal receiver, expanding itſelf, appear'd to have blown the bladder almoſt half full; and the mouſe ſeeming very ill at eaſe, by leaping, and otherwiſe endeavouring to paſs out at the neck of his priſon; we, for fear the over-thin air would diſpatch him, let the air flow into the external receiver; whereby the bladder being compreſs'd, and the air in the vial reduced to its former denſity, the little animal quickly recover'd.

117. A while after, without removing the bladder, the experiment was repeated, and the air, by the help of the gage, reduced to its former degree of rarifaction; when the mouſe, after ſome fruitleſs endeavours to get out of the glaſs, was kept in that thin air for full four minutes; at the end of which, he appear'd ſo ſick, that, to prevent his dying immediately, we

remov'd the external, and took out the internal receiver; whereupon, tho' he recover'd, yet 'twas not without much difficulty; being unable to ſtand any longer upon his feet; and, for a great while after, he continu'd manifeſtly trembling.

118. But, having ſuffer'd him to reſt for a reaſonable time, preſuming that uſe had inured him to greater hardſhips, we conveyed him again into the external receiver; and, having brought the air to the former degree of rarifaction, we were able to keep him there for a full quarter of an hour; tho' the external receiver did not at all conſiderably leak; as appear'd both by the mercurial gage, and by the remaining dimenſion of the bladder. And, 'tis worth noting, that till near the latter end of the quarter of an hour, the animal ſcarce at all appear'd diſtreſs'd; remaining ſtill very quiet. And tho', when he was put in, his tremblings were yet upon him, and continued ſo for ſome time; yet, afterwards, in ſpight of the expanſion of the air he was then in, they ſoon left him. And, when the internal receiver was taken out, he not only recovered from his fainting ſooner than before, but eſcaped thoſe ſubſequent tremblings.

119. Encouraged by this ſucceſs after we allowed him ſome time to recover his ſtrength, we re-convey'd him, and the veſſel wherein he was included, into the former receiver, and pumped out the air, till the mercury, in the gage, was drawn down near half an inch lower than before, that the air might be yet farther expanded. And, tho' this, at firſt, ſeemed to diſcompoſe the little creature; yet, after a while, he grew very quiet, and continued ſo for a full quarter of an hour; when we cauſed three exſuctions more to be made, before we diſcovered him to be in manifeſt danger, (at which time, the bladder appeared much fuller than before:) but then, we were obliged to let the air into the outward receiver; whereupon, the mouſe was more ſpeedily revived, than one would have ſuſpected.

Now the air in which the mouſe liv'd all this while, had been clogged, and infected, with the excrementitious effluvia of his body; for 'twas the ſame all along; we having, purpoſely, forborn to take off the bladder, whoſe regular diſtenſions, and ſhrinkings, ſufficiently manifeſted, that the veſſel, whereof 'twas a part, did not leak.

Air, become unfit for reſpiration, may retain its uſual preſſure.

120. We took a mouſe, of an ordinary ſize, and, having convey'd him into an oval glaſs, fitted with a ſomewhat long, and conſiderably broad neck, that it might be wide enough to admit a mouſe, in ſpight of his ſtruggling; we convey'd in, after him, a mercurial gage, in which we had carefully obſerv'd, and mark'd the ſtation of the mercury; and which was ſo faſten'd to a wire, reaching to the bottom of the oval glaſs, that the gage, remaining in the neck, was not in danger of being broken by the motions of the mouſe in the oval part. The upper part of the long neck of the glaſs was, notwithſtanding the wideneſs of it, hermetically ſealed, by means of a lamp, and a pair of bellows, that we might be ſure the impriſon'd animal ſhould breathe no other air, than what fill'd the receiver, at the time when it was ſealed. This done, the mouſe was

watched, from time to time: and tho', by reaſon of the largeneſs of the veſſel, he ſeem'd rather drooping, than very near death, at the end of the ſecond hour; yet, in about half an hour after, he was judg'd to be quite dead, tho' we ſhook the veſſel, to rouze him. The gage manifeſted no ſenſible change in the ſtation of the mercury; but cauſing the ſeal'd part of the glaſs to be broken off, I obtain'd, after a while, ſome faint tokens of life: tho', I am not ſure, that they would have continued in a veſſel, where the air was ſo clogg'd and infected, if freſh air had not been frequently blown in by a pair of bellows, whoſe noſe was inſerted into the neck of the glaſs. This freſh air ſeem'd evidently, tho' but ſlowly, to revive the gaſping animal, which I could not, conveniently, take out of the glaſs, till he had gain'd ſtrength enough to make uſe of his legs; but after that, without breaking the glaſs, we took him out, and ſoon found him able to walk up and down.

121. A like experiment we, alſo, made with like ſucceſs, upon a ſmall bird, included, with a gage, in a receiver that would hold about a quart of water. The bird, in about half an hour, appear'd to be ſick, and drooping; the faintneſs, and difficulty of breathing, increaſing for about two hours and a half after; at which time, the animal died; the gage being not ſenſibly alter'd, unleſs, perhaps, the mercury appear'd to be impell'd up a little higher than when put in; which, yet, might proceed from ſome accidental cauſe.

122. To ſhew, that it is not want of coldneſs, but ſomething elſe in the included air, that makes it deſtroy birds pent up therein, which, by the hot exhalations of their bodies, may be ſuppoſed to over-warm it; we made the following experiment.

In a glaſs-vial, able to hold about three quarts of water, we hermetically ſealed up a ſmall bird; and found, that in a few minutes, ſhe began to be ſick, and pant. Theſe ſymptoms I ſuffer'd to continue, and increaſe, till they had laſted juſt half an hour; at which time, having provided a veſſel of water, with ſal-armoniac, newly put therein, to refrigerate it; and the liquor being thus made exceeding cold, the vial, with the ſick bird, was immerſed in it; and ſo kept there for ſix minutes: yet it did not appear, that the great coldneſs which muſt be thus procured to the impriſon'd air, ſenſibly revived or refreſhed the drooping animal, who manifeſtly continued to pant exceedingly. So that this remedy, proving ineffectual, the vial was remov'd out of the water; and the bird, ſome time after, many times ſtrain'd to vomit: and, afterwards, had evacuations downward, before ſhe quite expired; which ſhe did, in almoſt an hour, from the beginning of her impriſonment.

The uſe of the air to raiſe and ſupport the ſteams of bodies, conſider'd with regard to reſpiration.

123. We made by diſtillation, a blood-red liquor, chiefly conſiſting of ſuch ſaline, and ſpirituous particles, as may be obtained from human blood; which is of ſuch a nature, that if a glaſs-vial, about half fill'd with it, be kept well ſtopt, it will reſt as quietly as an ordinary liquor, without ſending up any ſmoke, or viſible exhalation; but, if the vial be open'd ſo, that the external air is permitted to come in, and touch the ſurface of the liquor,

within a quarter of a minute, or leſs, there will be elevated a copious white ſmoke, which not only fills the upper part of the glaſs, but plentifully paſſes out into the open air, till the vial be again ſtopp'd.

When this vial has lain ſtopp'd and quiet for a competent time, the upper half of it appears deſtitute of fumes, whereof the air, it ſeems, will imbibe, and conſtantly retain but a certain moderate quantity; which may give ſome light towards the reaſon, why the ſame air, quite clogg'd with ſteams, will not long ſerve for reſpiration. And if the unſtopp'd vial were placed in our vacuum, it would emit no viſible ſteams at all, not ſo much as to appear in the upper part of the glaſs itſelf that held the liquor; but when the air was, by degrees, reſtored at the ſtop-cock, without moving the receiver, to avoid injuring its cloſeneſs, the returning air would preſently raiſe the fumes, firſt into the vacant part of the vial, whence they would aſcend into the capacity of the receiver: and likewiſe, when the air, requiſite to ſupport them, was pumped out, they alſo accompanied it, as their unpleaſant ſmell made manifeſt; whilſt this red ſpirit, though it remain'd unſtopp'd, emitted no more fumes, till the new air was let in.

Snails, a ſlow-worm, and a leech, in vacuo.

124. Two ordinary white ſnails, without ſhells, differing in ſize, (the biggeſt being about an inch and a half, and the other about an inch in length) were included in a ſmall portable receiver, which was carefully exhauſted, and ſecured againſt the return of the air; and preſently after, being removed from the engine, it was eaſy to diſcern, that both the ſnails thruſt out, and drew in their horns at pleaſure; tho' their bodies had, in the ſofter places, numerous newly generated bubbles ſticking to them: and tho' they did not loſe their motion near ſo ſoon, as other animals, in our vacuum; yet, after ſome hours, they appeared moveleſs, and very tumid; and, at the end of twelve hours, the inward parts of their bodies ſeemed to be almoſt vaniſh'd, whilſt they appeared to be two ſmall full-blown bladders: and, on letting in the air, they immediately ſo ſhrunk, as if the bladders had been prick'd, the receding air had left behind it nothing but ſkins: nor did either of the ſnails, afterwards, tho' kept for many hours, give any ſigns of life.

125. We included in a receiver, whoſe globular part was about the bigneſs of a large orange, one of that ſort of animals, vulgarly call'd efts, or, ſlow-worms: having withdrawn the air, and ſecured the veſſel againſt the return of it, we kept him there about forty-eight hours; during which, he continued alive, but appeared ſomewhat ſwell'd in his belly; his underchap moving on the very firſt night, but not the day and night following. At length, by opening the receiver, under water, we perceiv'd, that about half the air had been drawn out. As ſoon as the water was impell'd into the glaſs, the animal, which was before dull and torpid, ſeemed, by very nimble and extravagant motions, to be ſtrangely revived.

126. We took a leech, of a moderate bigneſs, and having included it, together with ſome water, in a portable receiver, able to hold about ten or twelve ounces of that liquor; the air was pump'd out, after the uſual manner;

manner, and the receiver being remov'd to a light place, we obſerv'd, that the leech keeping herſelf under water, there emerg'd from ſeveral parts of her body, numerous bubbles, ſome of them in a diſperſed manner, but others, in rows, or files, that ſeem'd to come from determinate points. Tho' this production of bubbles laſted a pretty while, yet the leech did not ſeem to be very much diſcompos'd. This done, we ſet the receiver, which was well ſecured from the outward air, in a quiet place, where we viſited it, at leaſt once a-day; and found the leech ſomewhat faſten'd by her tail, to that part of the glaſs which was under water, and ſometimes wandring about that which was quite above water; and always, when we endeavoured to excite her, ſhe quickly manifeſted herſelf to be alive; and, indeed, appear'd ſo lively, after the full expiration of five natural days, that expecting ſomething might have happen'd to the receiver, I open'd it under water; when the outward air, impell'd in ſo much of that liquor, as ſatisfied me the receiver was well exhauſted.

Creeping inſects in vacuo.

127. Five or ſix caterpillars, all of the ſame ſort, being put into a ſeparable receiver of a moderate ſize, had the air drawn away from them, and carefully kept from returning. But, notwithſtanding this, I found them, about an hour after, moving to and fro in the receiver; and even above two hours after that, I could, by ſhaking the veſſel, excite ſome motions in them, that I did not ſuſpect to be convulſive. But looking upon them again, about ten hours after they were firſt included, they ſeem'd to be quite dead; and, tho' the air were forthwith reſtored to them, they continued to appear ſo: yet leaving them all night in the receiver, I found, the next day, that three, if not four of them, were perfectly alive.

128. We took from an hedge a branch, that had a large cob-web of caterpillars in it, and dividing it into two parts, we put them into like receivers; and in one of them ſhut up the caterpillars, together with the air, but from the other it was exhauſted. Now, in that which had the air, the little inſects, after a ſmall time appear'd to move up and down as before, and ſo continued to do for a day or two: but in that glaſs, whence the air had been extracted, and continued kept out, they ſhewed, after a very little while, no motion that we could perceive.

Winged inſects in vacuo.

129. *Nov.* 12. About 8 a-clock at night, there were taken four middle-ſized fleſh-flies, which having their heads cut off, were incloſed in a portable receiver, furniſh'd with a large pipe, and a bubble at the end. As ſoon as the receiver was exhauſted, the flies loſt their motion; an hour or two after, I approach'd them to the fire, which reſtor'd not their motion: wherefore I let in the air upon them; after which, in a very ſhort time, they began, one after another, to move their legs, and one or two of them to walk. And having kept them all night, in a warm place, they manifeſted, for a while, ſome ſmall motion.

130. *Sept.* 11. About noon we cloſed up ſeveral ordinary flies, and a bee, or waſp; all which, when the air was fully withdrawn, lay as dead; only, for a very few minutes, ſome of them had convulſive motions in their legs. They continued in this ſtate forty-eight hours; after which, the

air was let in upon them; and that not producing any ſigns of life, they were laid in the noon-day ſun: but none of them ſeem'd, in any degree, to recover.

131. *Dec.* 11. We put a great fleſh-fly into a very ſmall portable receiver, where, at firſt, he appear'd to be very briſk and lively; but, as ſoon as the air was drawn out, he fell on his back, and ſeem'd to have convulſive motions in his feet, and *Proboſcis*; from whence he preſently recover'd, upon letting in the air; which being drawn out again, he lay as dead: but, within a quarter, or half an hour after, I perceiv'd, upon ſhaking the receiver, that he ſtirr'd faintly up and down. This was done pretty late over night, and next night I found the fly not to be ſoon revived, either by warmth, or letting in the air. However, in a while after, he recover'd; and being, next morning, ſealed up again in that glaſs, and kept forty-eight hours, tho' over the chimney, he died beyond recovery.

132. We took a large graſs-hopper, whoſe body, beſides the horns and limbs, was about an inch in length, and of great thickneſs, in proportion to that length; and convey'd him into a portable receiver of an oval form, and able to hold about a pint of water; and having, afterwards, pumped out the air, till, by the gage, it appear'd to have been pretty well drawn out, we took care that no air ſhould re-enter. The ſucceſs was this. Firſt, tho' before the exhauſtion of the air begun, the graſs-hopper appear'd lively, and continued ſo for a while: yet, when the air began to be conſiderably rarified, he ſeem'd to be very ill at eaſe, and to ſweat out of the abdomen, many little drops of liquor, which being united, trickled down the glaſs like a little ſtream, that made, at the bottom, a ſmall pool of clear liquor, amounting to near a quarter of a ſpoonful; and by that time the receiver was ready to be taken off, the graſs-hopper was fallen upon his back, and lay as dead. Secondly, tho' having, a little after, laid the glaſs in a ſouth-window, on which the ſun then ſhone, I perceived ſome ſlow motions in the thorax, as if he ſtrained to fetch breath; yet, I was not ſure but they were convulſive motions; however, they laſted but a while, and then the animal appeared to be quite dead, and to continue ſo for three hours, from the removal of the receiver. Thirdly, that time being expired, the glaſs was open'd and the air let in; notwithſtanding which, there appear'd no ſign at all of life: but letting the glaſs reſt in a convenient poſture, that the water which came from him, might not endanger him, for a quarter, or half an hour; tho' I then perceiv'd no ſigns of life, yet I caus'd him to be carried into a ſun-ſhiny place, where the beams of a declining ſun preſently began to make him ſtir his limbs, and, in a ſhort time, brought him perfectly to life again.

135. *April* 15. We took one of thoſe ſhining beetles called roſe-flies, and having included it in a very ſmall round receiver, which we exhauſted, it ſtruggled much whilſt the air was withdrawing; yet preſently after, I could perceive but little motion: about ſix hours after, the fly ſeemed quite dead, and diſcover'd no motion upon that of the glaſs. And within about an hour more, tho' I let the air ruſh in, yet no ſign of life enſued, neither immediately, nor for a pretty while after. So that ſuſpecting the beetle to

be really dead, I yet, three or four hours after, found him lively. Whereupon, I caus'd the glaſs to be again exhauſted, and ſecur'd from the air; during which time, the animal ſeem'd to be much diſquieted, but did not loſe his motion, ſoon after.

134. With butter-flies I made ſeveral tryals, and, having obſerv'd them, not only to live, but to move, longer than was expected; I choſe to include ſeveral of them in receivers, ſomewhat large, that I might ſee, whether, in ſo thin a medium, ſome or other of them, by help of their large wings, would be able to fly. But, tho', whilſt the air continu'd in the glaſſes, they flew actively, and freely up and down; and tho', after the exhauſtion of the air, they continu'd to live, and were not moveleſs; nay, tho' at the bottom of the receiver, they would even move their wings and flutter a little; yet, I could not perceive any of them to fly, or have a progreſſive motion, ſupported by the medium, only. And, by frequently inverting the receiver, which was long, they would fall, like dead animals, without diſplaying their wings, tho', juſt as they came to touch the bottom, ſome of them, would, ſometimes, ſeem to make uſe thereof, but not enough to ſuſtain themſelves, or break their falls.

The neceſſity of air, to the motion even of ants and mites.

135. A number of ants being included in a ſmall portable receiver, exhauſted about noon; between ſix and ſeven in the evening, they ſeem'd to be all quite dead; and the rather, becauſe, tho' they appear'd very lively juſt before they were ſeal'd up, running briſkly up and down the bubble they were in; yet, they grew almoſt moveleſs, as ſoon as the air was exhauſted; and a little while after appeared more ſo: tho' I a little ſuſpected, that they were much incommoded, by ſome glutinous ſubſtance, that ſeem'd to have got into the receiver, from the vapours of the cement. When upon opening the glaſs, the air ruſhed in, no ſign of life appear'd, for a great while, in any of the ants: but next morning, about nine a-clock, I found many of them alive, and moving about.

We convey'd a number of mites, together with the mouldy cheeſe, wherein they were bred, into three or four portable receivers, which were, all of them, very ſmall, and not much differing in ſize. From all of theſe, except one, we withdrew the air; and, then, making uſe of our peculiar contrivance to hinder its return, we took them, one after another, from the engine, and laid them by, for further obſervation. That wherein, to obſerve the difference, we left the air, was ſealed at a lamp-furnace, after the uſual manner. Our trials afforded us the enſuing phenomena.

(1.) The mites, incloſed in the ſmall glaſs, that never came near the engine, continu'd alive, and able to walk up and down, for above a full week after they had been put in; and poſſibly, would have continued much longer, if the glaſs had not been accidentally broken.

(2.) As ſoon as ever one of the receivers was remov'd from the engine, I look'd with great attention upon it; and tho', juſt before the withdrawing of the air, the mites were ſeen to move up and down in it; yet, within a few minutes, after the receiver was apply'd to the engine, I could diſcern in them no life at all; nor was any perceived by younger eyes than mine.

Nay,

Nay, by the help of a double convex-glaſs, I was not able to ſee any of them ſtir up and down. And no motion was taken notice of in the other ſmall receiver of a like ſhape and bigneſs. About an hour after, I look'd upon the receiver attentively again, but could not perceive any of the mites to ſtir; and the like unſucceſsful obſervation I made two or three hours after that. And at firſt letting in the air, to try if the mites were not quite dead; I could not perceive, upon its ruſhing in, any of them to ſtir: yet, I left the receiver unſtopt as it was in the window, upon a ſuſpicion, that the air might not be able to exerciſe its operation upon them, in a ſhort time.

(3.) And, about two or three days after, I found a number of my little animals reviv'd; as an attentive eye might eaſily perceive, by the motion of certain little white ſpecks, when aſſiſted to obſerve it by little marks, that I made on the outſide of the glaſs, (which was purpoſely choſen thin and clear) near this, or that mite, with a diamond; by the approach to, or receſs from which marks, the progreſſive motion became, perhaps, within a minute, plainly diſcoverable; eſpecially, if, when the eye perceiv'd little white ſpecks, that look'd like mites, the receiver ſhould be ſo turn'd, that the bellies and feet of thoſe little creatures were uppermoſt; notwithſtanding which, they would not eaſily drop down, but continue their motion: and theſe ſpecks being made upon the concave ſurface of the thin glaſs itſelf, were thereby render'd much more eaſily viſible.

(4.) But becauſe it doth not, by the third phenomenon, appear, whether in caſe our mites had been kept in a moveleſs ſtate, for a much longer time, than three or four hours, they would have been recoverable, by the admiſſion of the air; I ſhall add, that one of the portable receivers, abovementioned, being exhauſted, and carefully ſecured from the air, was kept from monday morning to thurſday morning: after all which time, being unable to diſcover any ſigns of life, among the included mites, the air was let in upon them, which ſoon had ſuch an operation upon them, that both I, and others could plainly ſee them creep up and down in the glaſſes again.

An attempt to produce living creatures in vacuo.

136. Having procured a large number of ſilk-worms eggs, and cauſed three very ſmall receivers to be purpoſely made, that differ'd very little, either in ſize or figure; we convey'd into each of them, together with a ſmall ſtock of mulberry leaves, ſuch a number of eggs as, we thought, made it morally certain, that at leaſt, ſome of them ſhould prove prolifick. This done, we carefully exhauſted one of our receivers, and ſecured it againſt the return of the air; and the two others were left full of air: but having left in the one a little hole for the air to get in and out at, we ſtopped the other ſo cloſe, as to hinder all intercourſe between the internal and external air. Things being thus prepared, we expoſed the receivers to a ſouth window, where they might be quiet, and where I either came, or ſent to look on them, from time to time; the ſpring being then ſo far advanced, that I ſuppos'd, the heat of the ſun would be, of itſelf, ſufficient to hatch them, in no long time. And both I, and others, took notice, that, in the unexhauſted receivers, there were ſeveral eggs hatch'd into little inſects, that perforated their ſhells, and crept out of them; tho', afterwards, for want

of change of food, or air, or both, few, or none of them, proved long lived. But tho' the eggs, in theſe receivers, began to afford us little animals, in a few days; yet the eggs, in the exhauſted receiver, afforded none in ſo many more, that we left off to expect any from them.

We took ſeveral of thoſe little ſwimming creatures, which in autumn, eſpecially towards the end of it, are turn'd into gnats; and, having put a convenient number of them together, in a fit quantity of rain-water, wherein they had been found and kept, into a ſmall receiver; the air was pump'd out, and the veſſel ſecured againſt its return, and then ſet aſide in a place, where I could obſerve, that, on the day after, ſome of theſe little animals, were yet alive, and ſwimming up and down, not without minute bubbles adhering to them; but in a day or two after that, I could not perceive any of them alive: nor did any of them recover, upon the admiſſion of freſh air. Indeed, the weather was ſo cold and unſeaſonable, that a number of theſe little creatures, put up with water in another ſmall receiver, all died within a few days, tho' none of the air was exhauſted. And ſeveral that I kept in an ordinary glaſs, which was often unſtopp'd to give them freſh air, periſh'd very faſt.

The ſurprizing rarifaction of air without heat.

137. We took a round glaſs-egg of clear metal, and furniſh'd with a ſhank, ſome inches in length; this we fill'd with water, and convey'd both it, and a vial, containing water, into a receiver, of a convenient ſize; and by pumping the air out of it, we made bubbles both in the egg, and the vial, to diſcloſe themſelves in great numbers; ſo that the liquor, in the glaſs-egg, ſeem'd to boil, and caus'd all that was in the ſhank, to run over. When we thought the water was ſufficiently freed from air, we took out the glaſſes, and fill'd up the ſhank of the egg with water taken out of the vial, and inverted it into more of the ſame water, in ſuch manner, that the egg was quite full, ſhank and all, excepting a ſmall bubble of air, that we, purpoſely left, to gain the top of the egg; where we meaſured it as accurately as we could, and found it to be a tenth, and leſs than two hundredths of an inch. Then putting the glaſſes again into the receiver, the pump was work'd, and the little bubble, after a while, began to expand itſelf; which, when it had once done, it, at each ſuck, ſtrangely increas'd, till, at length, it drove all the water out of the round part of the glaſs. And, leſt it might be objected, that 'twas only the ſubſiding of the water, upon the withdrawing of the outward air, that before kept it up to the top of the glaſs; we caus'd the pumping to be continu'd, till the expanded air had, ſeveral times, driven the water, in the pipe of the egg, a pretty way beneath the level of the external and ſurrounding water in the other glaſs. This done, we let in the air, by degrees, with a deſign to obſerve what bubble we ſhould find at the top of the egg, when the water ſhould be again driven up into its cavity. But the expanded air had forced over ſo much water, that there remain'd not enough to fill the globular part of the egg. We, therefore, made the experiment again; and, when we had proceeded thus far, compar'd the above-mention'd diameter of the ſmall bubble, with that of the ſpherical

part of the glaſs, which we took with a pair of callaper-compaſſes; and tho' we found it to be ſomewhat more than twenty times as great, yet we ſuppoſed the two diameters to be only as 1 to 20; and, conſequently, ſince the proportion between ſpheres is triplicate to that of their diameters, the air appear'd to have, by expanding itſelf, poſſeſs'd eight thouſand times the ſpace it took up before. Nor was it overſeen by us, that ſuch glaſſes as we uſed, are ſcarce ever ſpherical. But Dr. *Wallis*, who aſſiſted at the experiment, concluded, with me, that the cavity of the ſhank, from whence the expanded air drove the water, which we did not compute, would make abundant amends for any inaccuracies. After this, for farther ſatisfaction, we took water, laboriouſly freed from air; and, putting it into the ſame glaſs-egg, we inverted it, as before, but left not any bubble in it. This we did, that, in caſe we could make the water ſubſide, the experiment might prevent a ſuſpicion, that ſome air, latent in the water, increaſed the bubble, formerly left in it. Having, then, exhauſted the receiver, at leaſt as much as before, the water, in the egg, did not at all ſubſide: but at length, with obſtinate pumping, a bubble diſcloſed itſelf, and drove all the water clear out of the round part of the glaſs. And tho', by reaſon of ſome ſmall leaks, that we could not find, or ſtop, we were not able as before, to make the expanded air depreſs the water in the ſhank, beneath the ſurface of the external water; yet we wanted very little of it: and, then, giving over, we found, that when the water was impell'd up again into the egg, there was, at the top of it, a bubble, whoſe diameter we meaſur'd, and found it to be to the diameter of the globular part of the glaſs, as 1 to 14: ſo that, though the little bubble had been a perfect ſphere, it muſt, when expanded, have been 2744 times as big as when unexpanded. But Dr. *Wallis*, obſerving the great thinneſs of the bubble, poſitively affirm'd, that he could not eſtimate it to be, at moſt, any bigger than the third part of a perfect ſphere of that diameter: by which eſtimate, the expanſion of the bubble muſt have reach'd to 8232 times its natural dimenſions. Yet by letting as much water into the receiver as it would admit, we found, that we had not exhauſted all the air.

138. At another time, a ſmall, and almoſt inviſible bubble, expanded itſelf, when the ambient air was pretty well exhauſted to more than ten thouſand times its former extent. We took a ſmall bolt-head, blown at a lamp, which contain'd, in all, about eighty grains of water; and inverting the ſmall neck into a jar of water, it was included in the receiver; and the ambient air being exhauſted, numerous bubbles roſe out of the water, and, expanding, quickly drove all the water out of the bolt-head. Then, re-admitting the outward air, the bolt-head was preſently almoſt fill'd, and all the expanded air ſhrunk into a bubble little bigger than a ſmall pin's head; when, taking the bolt-head out of the water, and inverting it, that the bubble might get out at the neck, we carefully fill'd it up with the water that had been freed from air; and, then, inverting it, as before, into the jar with water, we again included it; and, after ſome exſuctions, found, that there was got out of the water,

into the neck, a very conſpicuous bubble, which, upon admitting the air, ſhrunk almoſt into an inviſible one, and aſcended into the head of the glaſs. Then, again exhauſting the receiver very well, we found it expand itſelf, ſo as to fill the capacity of the bolt-head, and to drive out almoſt all the water. And, upon re-admitting the air, it again ſhrunk into a bubble, whoſe diameter (according to our beſt eſtimate) was not more than a two and twentieth part of the diameter of the head of the above-mention'd glaſs; ſo that, to fill the whole cavity of the head only, it expanded itſelf 10648 times: but becauſe it fill'd, likewiſe, the greateſt part of the neck, we found, by weighing the water which fill'd that part, and the water which fill'd the head, that the capacity of that part of the neck, was almoſt a third of the capacity of the head; being as 141 to 481. If, therefore, 481, the capacity of the head, contain'd it 10648 times; 141, the capacity of the neck, muſt contain it 3121 $\frac{167}{481}$ times; ſo that in all, the ſmall bubble of air was expanded to above 13769 times its former bulk.

The diameter of the ſmall bubble contracted, was $\frac{1}{27}$ inch.

The diameter of the outſide of the head of the glaſs was $\frac{22}{27}$ inch.

The water, that fill'd the head only, weigh'd ſixty grains and a half.

The water that fill'd the head, and as much of the neck as the air had before expanded itſelf into, weigh'd ſeventy-eight grains, and one eighth; whence that part in the neck weigh'd ſeventeen grains, and five eighths. The bolt-head itſelf weigh'd fifteen grains.

139. We tried this experiment again, and found a ſmall bubble, much about one twelfth of an inch in diameter, fill'd not only the ball at the end of the bolt-head, (which was an inch and a half in diameter,) but the whole neck, which contain'd near as much water as the head; and beat down the ſurface of the water within the pipe, much below that of the water external to it.

Theſe experiments may give riſe to inquire, what figures and motions in the particles of the air, can explain ſuch a wonderful rarifaction, perhaps, without quite loſing its durable ſpring; how the air comes to be rarifiable ſo many times more without heat, than hitherto we have found it by heat; and, laſtly, what might, reaſonably, be conjectur'd about that part of the cavity of an exactly cloſed glaſs, where tho' the eye diſcovers no viſible ſubſtance, it appears not, that the common air adequately fills ſo much as the ten thouſandth part.

The duration of the ſpring of expanded air.

140. It has not, that I know of, been attempted to diſcover, whether the air either in the utmoſt, or in the intermediate degrees of rarifaction we can bring it to, retains a conſtant and durable elaſticity; and what other properties it either gains or loſes by confinement *.

To attempt ſomething of this kind, I cauſed a good bubble of glaſs, with a ſtem, to be ſo blown at the flame of a lamp, that whilſt the ball was

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* Mr. *Hauskbee* has ſhewn, by experiment, that the ſpring of the air may be ſo diſturb'd by violent preſſure, as to require a conſiderable time to recover its natural tone and temper; and that this tone will be as the force employ'd, or its continuance in ſuch a violent ſtate. *Hauksb. Experim.* p. 110--112. and p. 162---166.

yet exceeding hot, and conſequently, contain'd none but highly rarify'd air, the ſtem was ſuddenly ſeal'd up. This bubble, many months after, I inverted into a baſon of water; and, having broken off the ſeal under the ſurface of it, the liquor was violently impell'd into the cavity, yet was not able to fill it; a conſiderable part being defended from the farther aſcent of the water, by the ſpring of the remaining air; which, for all the long ſtretch it had been put to, had not, that we obſerv'd, loſt any thing of its ſpring. At another time, leaving a very ſmall proportion of air in the folds of a fine limber bladder, whoſe neck was very cloſely tied; by the help of the air-pump, it was ſo expanded, that at length, it ſeem'd to fill the whole bladder, and reduce it to the extent it had, juſt before 'twas empty'd. And the bladder, by a peculiar contrivance, was ſo included in another veſſel, that, being protected from the outward air, it maintain'd its tumid-figure; and in that unwrinkled ſtate it continued for near three years.

I, afterwards, contrived an inſtrument, fit to diſcover how long air, brought to the greateſt expanſion I could conveniently reduce it to in my engine, will retain its ſpring; and by what degrees, or ſtages, and periods of time, the decreaſe, if any happen, is made. But I could not, by its means, obſerve any remarkable diminution in the air's elaſticity, tho' it was preſs'd, and as it were, clogg'd with a weight, which one would wonder how it could, when 'twas ſo highly rarify'd, ſupport for one minute. And in one of them, we found not, in ten weeks time, any conſiderable variation; for the little ſhrinking of the air, diſcoverable by an attentive eye, might be probably, aſcribed to the change of the weather to a far greater degree of coldneſs.

I alſo, contriv'd a little portable inſtrument, wherein the air being expanded, as one may gueſs, to five or ſix hundred times, (perhaps a thouſand times) its wonted extent, has not only, for a long time, preſerved its ſpring; but alſo, tho' very much dilated, without heat, the heat of the hand, apply'd to the outſide of the veſſel, has a quick, and very manifeſt operation; and upon the withdrawing thereof, the air quickly returns to its former dimenſions, and temper; ſo that it may be employ'd as a kind of weather-glaſs.

141. A cylindrical glaſs, blown at a lamp, and having a long ſtem coming out at the unſeal'd end, was quite fill'd with water, and inverted into more, placed at the bottom of a large pipe, ſeal'd at one end, and of three or four feet in length; this external pipe was exhauſted, till the air, that diſcloſed itſelf in the water of the internal one, had forc'd out the water, in the cylindrical glaſs, as low as the upper part of the ſtem; at which great expanſion of the air, the external pipe, being ſpeedily and ſecurely cloſed by a certain contrivance, the air thus rarify'd, was kept ſometimes in my own chamber, that was warmer; ſometimes in an under-room; and after it had been kept, from firſt to laſt, about eleven weeks, or three months, without any other remarkable variation, than that in the cold room, the water aſcended a little at that part of the internal pipe,

where the lower-end of the cylinder gradually leſſen'd itſelf into the ſlender ſtem. At length we broke off the cloſed apex, when the water was but leiſurely (becauſe of the ſlenderneſs of the orifice made for the air to get into it) impell'd up into the deſerted cavity of the cylinder, which it wholly filled, except a little bubble, exceeding ſhallow. We made uſe of our eyes, at a fit diſtance, and of compaſſes, both ordinary and callaper, to obtain theſe meaſures. The cylindrical part of the internal pipe was three inches in length; and three fifths of an inch, or leſs, in diameter, on the outſide. The bubble was two tenths in diameter, and about two hundredths in depth. From all which, according to Dr. *Wallis*, who aſſiſted in the experiment, the natural bubble was, to the ſpace it poſſeſſed, when expanded, as 1 to 1350.

The condenſation of the air by cold, and its compreſſion without mechanical engines.

142. After the middle of *September*, on a ſun-ſhiny day, about noon, we took a bolt-head, or round vial, furniſh'd with a long ſtem, and placed it in a frame purpoſely provided, ſo that the ſtem was perpendicular to the horizon, and the globular part ſupported by ſuch a veſſel, that thorough a hole made in its middle, the ſhank reached downwards, till the orifice of it was a little immers'd beneath the ſurface of a glaſs of water, placed at the bottom of the frame. This done, we took a large proportion of beaten ice, and mix'd it with a due quantity of bay-ſalt, and not only laid it round about the lower part of the ball; but the veſſel, contiguous to that part, being purpoſely made with turn'd-up brims, we heap'd up the frigorific mixture, ſo as to bury the whole ſpherical part of the glaſs in it, and cover the very top of it therewith to a conſiderable thickneſs; whereby the air within being exceedingly cooled, the water, in which the ſhank terminated, was made to aſcend faſt along the cavity of that ſhank, till we perceived it would reach no higher: but, after a while, it began to ſubſide again; which nick of time being carefully watched, we made a mark at the higheſt ſtation of the fluid, and then taking out the bolt-head, we fill'd it with water; allowing for that ſmall part of the ſtem which was immers'd at the beginning of the operation. This water weighed nineteen ounces, and ſix drams; then weighing as much water, as ſufficed to fill the ſhank up to the mark before-mentioned, we found that to be one ounce and three drams; by which number, the former being divided, the quotient is $14\frac{4}{11}$ drams: ſo that the proportion of the two quantities of water, being as 11 to 158, the ſpace into which the air was condenſed by refrigeration, was to the ſpace it poſſeſſed in its former ſtate of laxity, as 147 to 158; and, conſequently, the greateſt condenſation, that ſuch a time of the year, ſuch weather, and ſo high a refrigeration, could bring the air to, made it loſe but $\frac{11}{158}$ of its former extent.

But, in the following condenſation, or compreſſion of air, tho' cold were, indeed, employ'd, yet that could not contract the air to any thing near ſuch a degree, where the frigorific mixture did not primarily, or immediately, compreſs the included air; but only ſo affected the water that was ſhut up with it in the ſame veſſel, as to make it ſwell, and, conſequently, crowd the aerial particles into leſs room.

The experiment was this. We took a new glaſs bolt-head, with a neck not long, and filled it ſo far with common water, that, being hermetically ſeal'd, the liquor reach'd within three inches of the top; and making an eſtimate of the ſharp end, left ſo for the conveniency of ſealing up the glaſs, we gueſs'd it to be about a quarter of an inch in length; then, applying ſnow and ſalt to the lower part of the bolt-head, we readily drove out the water further and further into the neck, till at length it was got up to the baſis of the ſharp conical end, where the glaſs was ſeal'd; and then, juſt as I was looking upon it, the glaſs flew, with a noiſe, about my ears; being broke into many pieces, which argued the compreſſion of the air to have been very great. And Dr. *Wallis*, who was preſent, and meaſur'd it from time to time, deſir'd me to regiſter the experiment, with his eſtimate; which is, that the air was reduced into the fortieth part of its former dimenſions.

This condenſation of the air is the more ſurprizing, becauſe ſome of the greateſt mathematicians of our age, have not, with wind-guns, and other forcible engines, been able to crowd the air into leſs than the fifteenth part of its uſual extent.

The ſurprizing difference in extenſion of the ſame quantity of air rarified and compreſs'd.

143. Tho' we could not find, that cold, in our climate, would reduce the air into near the twentieth part of its natural ſpace, by condenſation; yet, heat will advance it to near ſeventy times its uſual laxity, by rarifaction.

But, as by engines, and artificial contrivances, the air may be two or three times more compreſs'd, than naturally it is, even in froſty weather; ſo, on the other ſide, it may, by means of art and inſtrumenss, be much more rarified, and expanded, than has been hitherto found, by the bare application of external heat, even that of an intenſe fire.

We may, alſo, obſerve, how much the utmoſt degree of its rarifaction by heat, mention'd by *Merſennus*, falls ſhort of the degree to which it has been advanced in our pneumatical engine; the proportion betwixt the two being that of about 1 to 70.

But the air we make our trials with, upon the ſurface of the earth, is not, properly, in a free and indifferent ſtate, with regard to rarifaction and condenſation; but already highly compreſs'd by the weight of the atmoſphere reſting upon it; whilſt the air to be rarified, has, by virtue of its ſpring, a ſtrong tendency to dilate itſelf.

Here, then, ſeems to be a ſurprizing mutability of the air, as to rarity and denſity; whereby the ſame quantity of air, being, ſometimes, compreſs'd, and ſometimes dilated, may change its dimenſions to a degree, that ſeems, almoſt, to tranſcend the power of nature and art; and, by conſequence, might, probably, be rejected as incredible, if it were abruptly and nakedly propoſed: for, we can ſcarce ſafely put determinate limits to the ſtupendous rarity, which the upper part of the atmoſphere, being, almoſt totally, uncompreſs'd, by incumbent particles of air, may be ſuppoſed to have by nature, unaſſiſted with art.

But to compare together the ſmalleſt extent to which we have reduc'd the air, by condenſation, and the greateſt to which we have advanc'd it by rarifaction; the extent of the ſame quantity, highly rarified, is, to leave out ſome odd hundreds, 13,000 times greater than before; which, being multiplied by 40, the degrees of the air's compreſſure, it will amount to 520,000, for the number of times, by which the air, at one time, may exceed itſelf in bulk at another: a difference truly ſurprizing, tho', doubtleſs, it might be carried vaſtly higher! *

SECT. III.

BEfore we proceed to our other pneumatical experiments, 'tis neceſſary to premiſe, what relates to the improvements of the chief engine, wherewith they were made, and to the other inſtruments employ'd therein.

A deſcription of an engine with a double barrel for exhauſting the air. *Fig.* 68.

In our engine, with a double barrel, for exhauſting the air, AA, are two pumps made of braſs.

BB, two ſuckers or *Emboli*, hollow within, and open below.

CC, two holes in the upper part of the ſuckers, with valves opening outwards, to let the air eſcape, and hinder it from coming in.

DDDD, iron rods, ſerving to move the *Emboli*, being annex'd to them.

EE, two flat iron ſtirrups, at the top of the rods DD, on which the operator muſt ſtand to work the engine.

GGG, a cord join'd to the two ſtirrups, and running in the pully H.

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* Air, near the earth's ſurface, poſſeſſes about 850 times the ſpace of an equal weight of water; and, therefore, ſays Sir *Iſaac Newton,* "a cylindrical column of "air, 850 feet high, is of the ſame weight "with a column of air a foot in height, "and of the ſame diameter. But a co"lumn of air, reaching to the top of the "atmoſphere, is equal in weight to a co"lumn of water, about 33 feet high; if, "therefore, the lower part of the whole "aerial column of 850 feet high, be de"ducted, the remaining upper part will "be equal, in weight, to a column of wa"ter 32 feet high. Now, ſince the air "is compreſs'd, in proportion to the "atmoſphere that reſts upon it; and "ſince gravity is reciprocally as the "ſquare of the diſtance of the place "from the earth's centre; I have found," ſays he, "that air, in aſcending from the "ſurface of the earth, to the height of "one ſemi-diameter thereof, is rarer than "with us in a far greater proportion, "than that of all the ſpace below the "orb of *Saturn,* to the ſphere of an inch "diameter. Conſequently, ſuch a ſphere "of our air, of the rarity it has at the "height of a ſemi-diameter of the earth, "would fill all the regions of the planets, "as far as the orbit of *Saturn,* and vaſtly "farther!" *Newton. Princip.* p. 470.

This prodigious degree of rarifaction, ſeems unintelligible to Sir *Iſaac Newton,* by feigning the particles of air to be ſpringy and ramous, or rolled up like hoops: or, by any other means than a repulſive power; which is much greater here than in other bodies, becauſe air is very difficultly generated out of very fix'd bodies; and ſcarce without the aſſiſtance of fermentation; for thoſe particles recede from one another with the greateſt force, and are moſt difficultly brought together, which, upon contact, cohere moſt ſtrongly. *Newton. Optic.* p. 371, 372.

LL, two valves at the bottom of the pumps, opening inwardly, to admit the air out of the tube MM.

MM, a tube reaching from both pumps to the plate OO, by means of the curvature PPQQ; which ought to be ſo long, that the tube PQQ, may not hinder the pumper from ſtanding conveniently on the ſtirrups EE.

OO, a plate bored in the middle, on which, the receivers, to be evacuated, are placed; as R, for example.

Before this engine can be fit for uſe, it is to be put into a frame of wood, to ſupport it, as *Fig.* 69. and as much water is to be poured thro' the hole Q, in the plate OO, into the pumps, as will fill the cavities of the ſuckers, and a little more: then, a perſon muſt ſtand on the two iron ſtirrups EE, and alternately depreſs and elevate them. By this means, the ſuckers following the motion of the ſtirrups, in their aſcent, will leave the ſpace in the bottom of the pumps empty; and ſince, as all other paſſage is denied from the air, that alone, which is contain'd in the receiver R, is convey'd into the pumps, by the tube QQPPM, and opens the valve L, which being preſently ſhut, hinders the ſame air from returning: whereſore, the ſucker afterwards deſcending, compreſſeth that air; whence of neceſſity, the valve C muſt be open'd, and all the air paſs out at it; becauſe the water in the bottom of the pumps exactly fills all the ſpace, and alſo regurgitates thro' the valve C.

This double engine is, upon many occaſions, preferable to a ſingle one; ſince it doth not only produce a double effect, but alſo performs it much more eaſily: for, in thoſe engines which are furniſhed but with one tube, whilſt the ſucker is drawn up to evacuate one pump, the whole pillar of the air incumbent on the ſucker, is to be elevated by force; and again, when the ſucker returns, it is alſo by force to be reſtrain'd, leſt it ſhould be too ſwiftly impell'd by the air, and ſo break the bottom of the engine; but in theſe double engines, the operator is in a manner wholly free from that toil. For, in the firſt ſuctions, the *Emboli* are eaſily lifted up, becauſe the air, immediately deriv'd from the receiver R, into the pumps, preſſeth the ſuckers downwards, almoſt as ſtrongly, as the external air, incumbent on the oppoſite part; and when the quantity of the internal air is diminiſh'd, the ſucker to be depreſs'd, tends downward with the greater force, and ſo, by means of the cord GGG, compaſſing the pully, draws the other *Embolus* upwards, and, at the ſame time, hinders it from deſcending with too great velocity; and, by this means, both ſuckers, at one and the ſame time, will aſſiſt the pumper. And, as the *Emboli* make but a very ſmall reſiſtance, the two pumps of this engine may be ply'd with greater eaſe and expedition, than one pump in ſingle engines; whence, this contrivance is of great uſe in thoſe experiments, which cannot well be made ſlowly.

A mercurial gage. Fig. 70.

The whole gage ABCDE, conſiſts of three glaſs-tubes, all well cemented together, ſo, that a paſſage remains open, from one to the other; the firſt of theſe tubes AB, being open at the extremity A, is of leſs capacity than the tube BCD, but of greater than ED. The tube BCD, is crooked in the middle, and the tube ED, ought to be hermetically ſeal'd at the extremity E; but the part BCD, muſt be fill'd with mercury.

If this inſtrument, thus prepar'd, be put into a receiver, out of which the air is to be extracted, the air remaining in the part ED, will, by its ſpring, compreſs the mercury DCB, and force it to aſcend into the part BA, and itſelf will be dilated in the cavity DC. If then, the following proportions be duly obſerv'd, between the magnitude and length of the tubes, when the air is extracted, the merucry will almoſt reach to the top A, and the air in the other leg, being ſo dilated, that it cannot ſuſtain a greater body of mercury, will remain included in that ſpace.

But, that this inſtrument may exactly ſhew the quantity of the air produced in a receiver; the tubes AB, ED, are to be diſtinguiſh'd by marks into ſeveral parts; and when the *Torricellian* experiment is made, upon the plate LM, of the pneumatic engine, as *Fig.* 70. a receiver FGE, is to be taken, perforated, at the top F, and the tube HI, is to be tranſmitted thro' the hole, that ſo the receiver may be apply'd to the plate; and then the hole F being ſtopp'd, and the gage ABCDE put into the receiver, the air is to be exhauſted; the air then, being dilated in the receiver, the mercury cannot be ſuſtain'd ſo high in the tube HI, but muſt deſcend by degrees; and at the ſame time, the air of the tube ED, gradually drives the mercury into the tube AB. Now when the mercury, in the tube HI, deſcends to the height of twenty-nine inches, and remains at that height, if we mark how high the mercury hath aſcended into the tube AB, we may know, that as often as the mercury in the gage ſhall reſt at that height, the air in the ſame receiver, will be able to ſuſtain, only twenty-nine inches of mercury; whence that place in the gage muſt be mark'd with the figure twenty-nine; and ſo every inch of the mercury's deſcent in the tube HI, may be marked in our mercurial gage, when the part AB, will ſhew all degrees of the rarifaction of the air.

But now, if the air be condens'd in the receiver, above its wonted preſſure, and all ways of its eſcape be ſtopp'd, it may immediately be known, by the tube ED; for the mercury will be impell'd into it by the incumbent air, thro' the open hole ſo much the higher, as the compreſſure of the air in the receiver ſhall be the greater; and how great that is, and what an altitude of the mercury it can ſuſtain, may eaſily be found, by computation, thus.

It has been prov'd, that the ſpace poſſeſs'd by air, is diminiſh'd in the ſame proportion, as the compreſſing force increaſes, and *vice verſâ*.

Let then the ſpace A, be poſſeſs'd by a certain quantity of air, whilſt the compreſſing force is F; if we increaſe that force by the addition of G, which is equal to it, our ſelf-ſame quantity of air will be reduc'd to half its ſpace, ſo that B, the remaining ſpace, will be half of the total ſpace A, as the former preſſure F, is half of the total preſſure F and G. And, if we further increaſe the preſſure, by the addition of H, ſo that the firſt preſſure F, is only one fourth of the total preſſure F and G and H, the air can poſſeſs only the ſpace C, which is one fourth of the total ſpace A.

Thus, the remaining ſpace will always be in the ſame proportion to the total ſpace, as the firſt preſſure is to the total preſſure.

So that the remaining ſpace, being to the total ſpace, as the firſt preſſure is to the total preſſure; three of theſe terms being known, it will be eaſy to find a fourth, by the rule of proportion. For inſtance, in our gage let the tube ED, be the total ſpace into which the air is compreſs'd, by the uſual preſſure of the air, which in *England* is equivalent to thirty inches of mercury; the firſt preſſure therefore, will be thirty inches of mercury. Now, if that preſſure be increaſed, and the air reduced into a leſs ſpace, ſuppoſe in the ſpace NE; to find the quantity of this preſſure, I meaſure the remaining ſpace NE, and conſtitute that, ſuppoſe ſix inches for the firſt term of the proportion; then the ſecond term will be the total ſpace DE, ſuppoſe twelve inches; the third term the height of thirty inches of the mercury, which was the firſt preſſure; and ſo the fourth term, or total preſſure, will be found to be ſixty inches of mercury; whence I conclude, that the preſſure of the air in the receiver can ſuſtain the mercury to the height of ſixty inches; and ſo of the reſt.

From the ſame principle, it will be eaſy to find, what ought to be the proportion, between the ſize of the tubes AB and ED. For that depends on the length of the legs, which the higher they are, ſo much the better they reſtrain, and keep in the air, but little dilated, in the ſeal'd part. For inſtance, let the length AB, be ten inches, which height of the mercury is one third of the accuſtom'd preſſure, and it is ſufficient, that the tube HB, be twice as big as the tube ED; for after the mercury hath aſcended to the top of the tube AB, the air included in the other leg, expanding itſelf into the ſpace forſaken by the mercury, will poſſeſs three times more than its former ſpace; and ſo one half of the firſt preſſure, which is ten inches, will be ſufficient to curb its ſpring. But, if the legs were ſhorter, the mercury would be expell'd, by the included air, at leaſt in part. And therefore, the magnitude of the tube AB, ought to have a greater proportion to the magnitude of the tube ED, that the aſcending mercury may afford more ſpace to the air, to be dilated; ſo that the ſpring of the air being weaken'd, the weight of the mercury cannot be overcome. And thus it would happen, if the height of the gage were to the height of thirty inches, in the ſame proportion with the firſt ſpace of the air, to the total ſpace it would poſſeſs *in vacuo*.

The height of the tube ſhould rather be too long, than too ſhort; becauſe if it be too ſhort, the mercury will be expell'd in part, and ſo not ſhew all the degrees of rarifaction; but if it be too long, the mercury will only, not reach to the top, and ſo the gage will, nevertheleſs, ſhew all the variations, tho' they be leſs ſenſible.

But the tube DC, ought to contain a ſufficient quantity of mercury, at the leaſt, to fill the tube AB, before any paſſage be open'd for the air included in the tube ED.

A condenſer. Fig. 72.

In our engine to compreſs the air, AA, is a glaſs-veſſel, whoſe orifice is exquiſitely fitted to the flat plate BB.

BB, is a flat plate of braſs, made to cloſe the veſſel AA exactly.

CC, a ſmall tube of braſs, paſſing thro' the middle of the plate, and faſtened thereto.

E, a little valve, opening inwardly, to ſhut the ſmall tube C.

F, the ſpring depreſſing the valve E.

GGG, the gnomon faſtened to the plate BB, made for reſtraining the ſpring F.

II, a ſquare lath, ſuſtaining the plate BB, and bored thro' in the middle, to tranſmit the little tube C.

LLL, LLL, two iron-wires, which paſſing thro' the holes in the lath II, and compaſſing the upper part of the iron-plate KK, hinder the plate from being much moved from the lath.

KK, an iron-plate, with a hole in the middle, formed into a female-ſcrew, to receive the male-ſcrew MM.

MM, an iron-ſcrew, ſtraitly to conjoin the receiver AA, with the plate BB; and leſt the braſs-veſſel ſhould be broken, it is proper to put ſome wood and leather between the ſcrew, and the upper part of the receiver; leather, alſo, is to be put upon the plate BB, both to prevent the breaking of the glaſs, and the more exactly to ſhut the receiver.

NN, a pump faſtened to the tube C, below the plate BB.

OO, the ſucker of the pump NN.

P, a little hole in the lower part of the pump, by which the air enters into it, when the ſucker is brought to the loweſt part thereof.

To compreſs the air by means of this engine, we put the bodies, whereon the experiment is to be made, into the receiver AA; and laying it on the plate BB, firmly bind it thereto, by help of the ſcrew MM. This done, the ſucker or plug OO, is to be drawn, till the external air, by the hole P, can fill all the upper part of the pump; then, if the ſucker be drawn upwards, the air finding no other paſſage, will open the valve E, and enter into the receiver AA; from whence there is no regreſs, becauſe the valve E, is preſently depreſſed by the ſpring F, and ſhuts the hole C. And ſo we may repeat the compreſſion of the air into the veſſel AA, at pleaſure; whilſt the quantity thereof is eaſily known by the mercurial gages.

But I ſo faſhion the pump, that it may be fitted by a ſcrew, to the tube C; for thus, when one receiver is full, we may take away the pump, and uſe it to fill others.

Now, becauſe in theſe engines, mercurial gages ſerve to ſhew the degrees of compreſſion; there is no occaſion for the gages before deſcribed; for thoſe are made with more difficulty, and beſides, afford but a ſmall ſpace, wherein to note the degrees of compreſſion. It is therefore, better to bend the glaſs-tube, ſeal'd at one end, in ſeveral places, as in the figure T, that a long tube may be contain'd in a ſhort receiver; ſo that the mercury, being put in thro' the open end, as much as will ſuffice to fill the length of one inch; all the reſt of the ſpace, fill'd with air, will ſerve for marking the degrees of compreſſion, much more ſenſibly than can be done in a ſhorter tube.

Here we muſt note, that when the mercury tends downwards, in ſuch an inflected gage, the weight thereof forwards the external preſſure; but when it is impell'd upwards, the ſame weight reſiſts it: a difference to be regarded in very accurate experiments.

To mix liquors or powders in compreſs'd air. Fig. 72.

In order to make mixtures in compreſſed air, let the receiver be AA, in which we would mix either liquors, or powders.

Let QQ, RR, be two tubes, each of them ſeal'd at one end, and open at the other.

Let RQS, be a veſſel of braſs, to be laid upon the orifice of the tubes as in the figure.

The liquors to be mixed, muſt be poured into the tubes QQ, RR, each liquor in its own tube; and let the veſſel RQS, being inverted, be laid on the orifices of the tubes; and in that poſture, let all be cover'd with the receiver AA; let the ſcrew be driven, and the air intruded after the manner juſt deſcribed; and when the gage TT, ſhews that the compreſſure is arrived at the degree intended, the engine is to be inverted, and ſo the liquors will flow down from the tubes into the veſſel RQS, and be mix'd there. If more liquors or powders are to be mix'd, the number of the tubes is to be increas'd accordingly.

To make and remove artificial air from one receiver into another.

Fig. 74, & 75.

To tranſmit air out of one receiver into another, we uſe the following contrivance.

AA, is a flat plate of metal, with a hole in the middle.

BB, is the ſtop-cock, faſtened to the hole in the middle of the plate AA, one of whoſe ends is form'd into a male-ſcrew.

DC, is a copper-funnel, open below, with a broad orifice, (that it might be eaſily ſet upon the pneumatic engine, and there ſtand firm;) and the upper part of the orifice D, is faſhion'd into a female-ſcrew, to receive the male-ſcrew of the ſtop-cock BB.

EE, is a ſmall tube, open at both ends, which are cut into a female-ſcrew, to receive the male-ſcrew of the ſtop-cock BB.

FF, is the receiver laid on the plate AA, and exquiſitely fitted thereto.

Now to make factitious air, we muſt put the matter which is to produce the air, into the receiver FF; and placing that on the plate AA, by means of the ſcrew, we ſtrongly faſten it thereto, as in our engine for compreſſing the air; the ſtop-cock BB, we inſert into the female-ſcrew D: then the orifice C, and with it the receiver, is to be placed upon the pneumatic-engine, and the ſtop cock B, being open'd, the air is to be extracted. When the receiver FF, is emptied of air, the ſtop-cock B, is to be ſhut, that all paſſage to external air into the receiver may be denied; and the ſtop-cock, being taken out from the female-ſcrew D, the receiver is preſently to be immers'd in water; ſo that at leaſt the plate AA, with the ſtop-cock, may be cover'd therewith: thus no air from without can find entrance; and the air, produced out of the matter in the receiver, will be perſerv'd unmix'd; whilſt the degrees of its rarifaction, or compreſſion, are known, as thoſe of common air.

But

Fig. 63. p. 511.

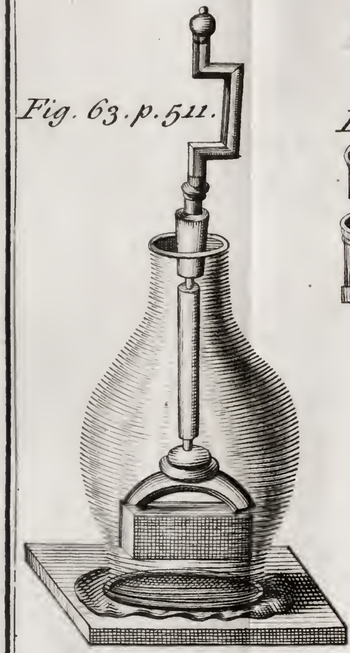


Fig. 64. p. 513.

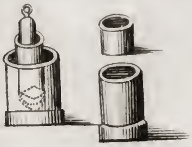


Fig. 65. p: 514.

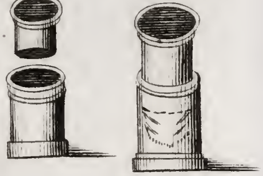


Fig. 66. p. 514.

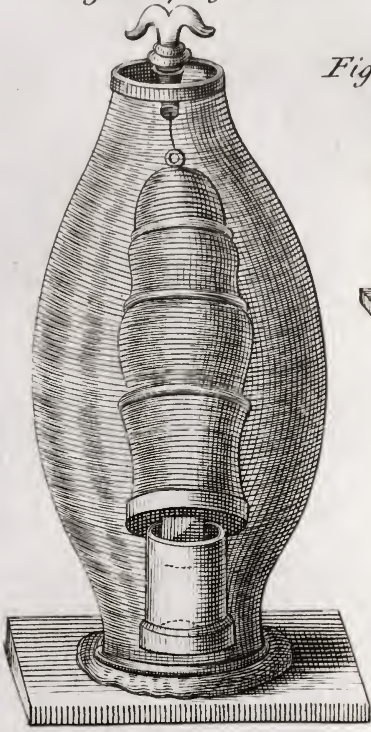


Fig. 67. p. 516.

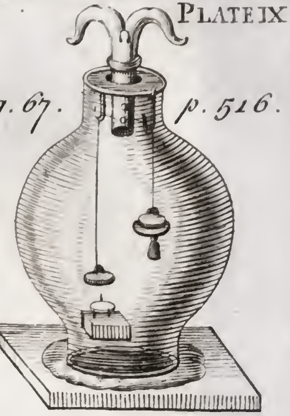


Fig. 71. p. 557.

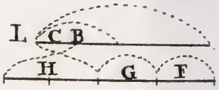


Fig. 72. 558.

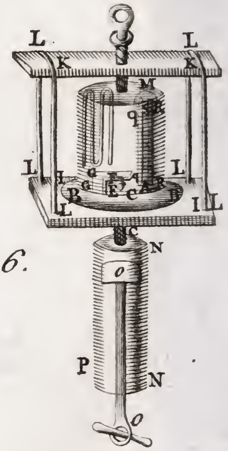


Fig. 68. p. 555.

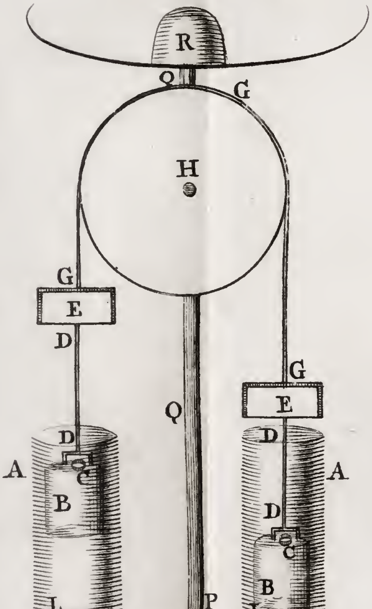


Fig. 69. p. 556.

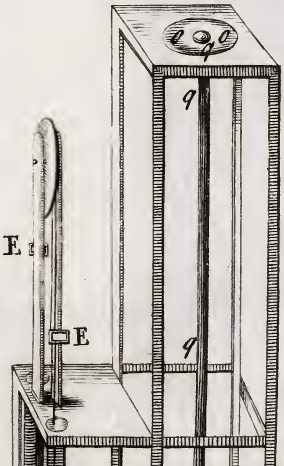


Fig. 70. p. 556.

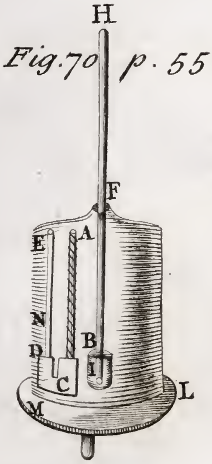
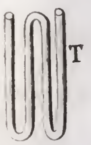


Fig. 73. p. 559



J. Mynde sc.

But if we would tranſmit that air into another receiver; another receiver FF, with another plate A A, and a ſtop-cock B B, is to be procured, and evacuated: then, by means of the ſmall tube E E, we join the ſtop-cocks B B, of both receivers, when all ſuſpected places are to be ſtopp'd with cement, that no external air may find entrance. Then, the ſtop-cocks being open'd, the air, produced in the former receiver, flows into the latter; and the ſtop-cocks being again ſhut, and pluck'd out from the tube E E, the receivers may be kept a-part: when if there be any matter included in the latter receiver, we may eaſily view what influence the factitious air hath upon it. *Fig.* 74.

But, becauſe the mercurial gages, lately deſcrib'd, are ſpoil'd if they be inverted, and the crooked gages preſently expel their mercury, if the air be rarify'd in their receivers; and, ſince the operation, here deſcrib'd, cannot be perfected, but both receivers muſt be inverted, and both, likewiſe, emptied of air; gages of another ſort are to be made, after the manner following.

AA, is a glaſs vial, fill'd with mercury to the ſuperficies DD. *Fig.* 76.

BB, is a glaſs tube, very well cemented, in the orifice of the vial.

CC, is another tube tranſmitted thro' the tube BB, and reaching to the bottom of the glaſs. This tube muſt be ſeal'd above, and open below; neither muſt it ſo exactly fill the tube BB, but that paſſage may be given to the external air, within the glaſs AA.

If this inſtrument be put into a receiver, from which, the air muſt be, afterwards, extracted, both tubes will be exhauſted of air; and, when you invert the receiver, to take in new air; as in *Fig.* 74. the mercury will flow down to the orifices of the vial, and be there kept, below the orifice of the tube BB; when the new air entring, will eaſily fill both tubes, and the vial: then, the receiver being erected, the mercury will again reſt, in the bottom of the vial, and the orifice of the tube CC, will be plunged in it. And, if any air be produc'd, out of the bodies included in the ſame receiver, the mercury will aſcend into the tube CC, and there, reducing the air into a narrower ſpace, ſhew the degrees of compreſſion.

The inſtrument wherewith we filtred air thro' water, was thus contrived. *To filter air thro' water.*

AA, is a glaſs receiver, whoſe orifice, laid upon the plate BB, agrees exquiſitely therewith.

BB, is a plain plate with a hole in the middle, to tranſmit the tubes, CC, DD. *Fig.* 77.

CC, DD, are two tubes, cemented to the plate BB; one of which is no higher than the plate, but the other reacheth almoſt to the top of the receiver.

EEEE, is a ſtop-cock, to whoſe holes the extremities of the tubes, CC, DD, are faſtened.

FF, is the key of the ſtop-cock unperforated, wherein is only one chink GG.

HH, is the receiver, compaſſing the end of the ſtop-cock, and faſten'd to it, preventing the entrance of the outward air, and communicating with the pump II.

LL, is a glaſs veſſel.

M, is a hole in the top of the receiver, whoſe ſtopple is faſtened with a ſcrew.

Fig. 78. The next figure exhibits a ſtop-cock, cut tranſverſly, that the two tubes CC, DD, may be the better diſtinguiſh'd, and their inſertion into the ſtop-cock be perceived.

This inſtrument is thus to be uſed: we put the thing about which the experiment is to be made, into the veſſel; and the receiver AA, being laid on the plate BB, we pour water into the hole M, till the receiver be about half full, and the veſſel LL, with the matter contain'd therein, ſwims on the top thereof; then we ſtop the hole exactly, and faſten it with a ſcrew. The key is afterwards to be ſet ſo, that the chink GG, may communicate with the tube CC; then the plug being brought to the loweſt part of the pump, the air of the receiver AA, entring through the upper orifice of the tube CC, will flow down thro' the chink GG, into the receiver HH, and into the pump. Then the key being inverted, ſo that the chink GG, may anſwer to the inſertion of the tube DD, the plug is to be impelled upwards; when the air will be expelled from thence, and, finding no other paſſage, be driving through the chink GG, into the tube DD; and from thence it will emerge to the upper part through the water ſtagnant in the receiver. And by repeating this proceſs, we ſtrain the air thro' the water, as often as we pleaſe; and thence know whether it acquires any new qualities, in reſpect of the body included with it.

How to condenſe and rarify the ſame parcel of air. *Fig.* 79.

Let the receiver AA be placed upon the plate BB, and ſcrewed on to it.

CC, is the ſtop-cock, faſten'd to the hole in the midſt of the plate BB.

DD, is a pump joined to the ſtop-cock C, with a ſcrew.

E, is a veſſel, ſo large, that it may fluctuate in the receiver AA, without danger of being overturn'd.

Let ſome animal be put into the veſſel E, and let the receiver AA, be put upon it, and ſcrewed to it, as the figure ſhews. Then let the pump be fill'd with water, and, by a ſcrew, be fitted to the ſtop-cock; the ſtop-cock, being then open'd, let the plug C, be forced upwards, and the water aſcending thro' the ſtop-cock will, in part, fill the receiver AA, and reduce the air, contain'd therein, into a narrower ſpace, without any addition of the new air: if, then, you draw the plug downwards, the ſame numerical air will be again rarified. Thus you may both condenſe and rarify the ſame air as often as you pleaſe; and, by this means, you may find, whether the condenſation of the air contributes to prolong the life or health of animals.

A wind-gun. *Fig.* 80.

In our wind-gun AA, is a hollow copper globe.

BB, a tube, faſtened to the globe.

F, a valve opening inwardly, and ſhutting the tube BB.

G, the ſpring depreſſing the valve.

H, a gnomon affixed to the globe AA, and making faſt the ſpring G.

CC, a tube of iron, faſtened to the tube BB, and the globe AA.

DD, a plug exactly fitted to the tube.

EEE, another plug fitted alſo to the tube BB, with an iron wire, reaching almoſt to the valve F.

R, the protuberance of the tube CC, ſomewhat hollowed above, to receive the end of the iron LL.

LL, a crooked iron, moveable about the extremity in R, ſo that it ſerves as a lever to lift up the plug.

OPO, a crooked iron, faſtened in M, that the thumb reſting in the angle P, the reſt of the fingers may attract the lever L, and ſo force the plug EEE, upwards. But the curvature is deſign'd, that the one end O, might be applied to the ſhoulder, in aiming at a mark.

TT, a rectangular piece of iron, compaſſing the lever LL and the iron OPO, to keep the lever in its poſture; for otherwiſe, the plug EEE, would be thruſt far out, whilſt the air is intruded into the globe AA.

II, an elliptic hole, in the upper part of the globe, very well ſhut with a valve, opening inwardly, to give liberty of inſpection, and of amending what is amiſs; for the valve may be drawn through the hole, by reaſon of its elliptic figure.

SS, a metalline plate tranſverſly placed above the hole II, and perforated to tranſmit the ſcrew V, by help whereof the valve, ſhutting the hole II, is ſuſtained, and applied cloſely to it.

Q, a hole in the lower part of the tube CC, by which the air enters into the tube, whilſt the plug D, is brought to the loweſt part thereof.

The air is forced into this engine, by ſetting the foot upon the crooked end of the plug DD, that it may not be removed from the ground, and lifting the engine upward, till the upper part of the plug comes below the hole Q; and then the air entring through the hole, wholly fills the tube CC.

Then, by forcibly depreſſing the engine, the air contained in the tube CC, opens the valve F, and is thruſt into the globe AA; whence it cannot return, becauſe the valve preſently ſtops the paſſage: and thus by repeated ſtrokes, we may condenſe the air in the globe, till the force of its ſpring cannot be overcome by our ſtrength.

If we would diſcharge the air ſo condenſed, the plug DD, is wholly to be drawn out, and a bullet to be put into the bottom of the tube CC: then, by means of the lever LLL, the plug EEE, is to be impell'd upward, as we ſaid before, when the extremity of the iron-wire, opens the valve B, and the air breaking out therefrom, expels the bullet through the tube CC, with great violence.

But before the plug DD, is again put into the tube CC, for the compreſſion of the air, about half an ounce of water is to be pour'd into the tube. For by this means, no air at all can eſcape out by the plug; and moreover, that water exactly filling the upper part of the tube CC, the whole compreſſed air will be intruded within the cavity AA, and ſo the condenſation be perfected much ſooner, than if at every turn, part of the compreſs'd air remain'd below the valve F.

This engine has ſeveral advantages above the common wind-guns. 1. Becauſe one valve ſerves both for the letting in, and diſcharing the air; whence it is leſs ſubject to be ſpoiled, or impaired, than if two valves were uſed for that purpoſe. 2. If any diſorder happen in other guns, they remain uſeleſs; but here, by the elliptic hole, we may take out the ſpring and the valve, and ſo mend whatever is amiſs. 3. In other guns, the valves being cover'd with leather, are put in, before the engine is cloſed on every ſide; and therefore ſilver-ſolder could not be uſed in joining the parts, but only lead-ſolder, by which the air, being much compreſſed, could by no means be reſtrained; but here all things are well cemented with ſilver-ſolder, without danger of burning; ſince the valve cover'd with leather, is put in afterwards thro' the elliptic hole II. 4. But this engine is chiefly to be preferred before others, becauſe here we can put ſeveral bodies into the receiver, through the elliptic hole, and ſo make many experiments in highly compreſſed air.

An engine wherewith to diſtil in vacuo. *Fig.* 81.

We alſo contrived an engine, which ſhould diſtil *in vacuo*, thus.

AA, is a braſs veſſel ſhut below, and open above.

BB, a diaphragm of tin, whoſe edges are ſo poliſh'd on both ſides, that they exquiſitely agree and ſuit with the edges of the veſſels AA, DD, which are alſo poliſhed, and ſo keep out the external air.

CC, a tube faſten'd to a hole in the middle of the diaphragm BB.

DD, a braſs veſſel, whoſe aperture is applied to the diaphragm BB.

EE, a ſtop-cock faſtened to the hole of the diaphragm BB.

FF, a tube reaching from the ſtop-cock EE, to the hole made for ſuction in the pneumatic engine.

GG, a metalline veſſel, including the junctures of the veſſels with the diaphragm, and alſo the ſtop-cock, that being filled with water, it may keep all ſafe from the external air. This is to be ſolder'd to the veſſel AA.

To uſe this engine, we take away the diaphragm BB, and put the ingredients into the veſſel AA, and ſet it in a convenient place, till it is to be evacuated; then putting on the diaphragm BB, and the veſſel DD, we apply all to the pneumatic engine, and by means of the tube FF, the air is pumped out of the veſſels, the veſſel GG being yet firſt filled with water. Then the ſtop-cock is ſhut; and taking away the tube FF, we may place the evacuated engine on the fire, when the vapours, aſcending through the tube CC, are condenſed in the upper veſſel, and ſo we have a liquor diſtilled *in vacuo.* The quantity of the generated air, is known by the mercurial gage H; but that muſt be kept in the top of the receiver, leſt the mercury exhale, by reaſon of the heat.

Round pieces of paper, perforated in the middle, are to be laid over the orifices of the veſſels AA, DD, that they may be the better joined with the diaphragm; the commiſſures of the tube FF, with the ſtop-cock, and pneumatic engine, are to be fortified with cement; and the ſtop-cock EE, is ſo to be diſpoſed with the veſſel G G, that part of the key may be prominent, without the veſſel, thro' the hole, to be conveniently turned; nevertheleſs, the ſtop-cock, with the diaphragm, may be taken out of the veſſel G G, whilſt the veſſel A A, is to be filled with the deſigned matter. And that is very eaſily done, becauſe the key conſiſts of two parts, one of which M, is turned in the ſtop-cock itſelf, by means of a certain chink, which receives the ſmall protuberance of the other part O O, that exactly fills the ſmall pipe N N, faſten'd to the veſſel G G; and being prominent outwardly, may eaſily be turned in it, and communicate its motions to the other part M; but it is drawn outward, whilſt the diaphragm B B is to be taken out of the veſſel G G.

Fig. 82. ſhews another inſtrument, differing from the former, in that it almoſt wholly conſiſts of glaſs, and affords a longer paſſage for the vapours.

B B, is not a diaphragm, but a ſmall tube, poliſhed at both ends, that it may exquiſitely ſuit with the orifices of the veſſels A and D.

A A, D D, are two glaſs veſſels, whoſe orifices are applied to the tube B B, whence the vapours are eaſily tranſmitted from the one to the other.

E E, F F, G G, and I, have the ſame uſe as in the former figure; and the whole inſtrument is to be evacuated after the ſame manner, and plac'd upon the fire; except that here the veſſel A A, as being made of glaſs, muſt not be put on an open fire, but ſet *in balneo Mariæ*, or on ſand; and the vapours will be condenſed in the veſſel D D.

Several ways to forward the production of air; and firſt, air produc'd from bread.

(1.) *July* 11. 1676. I included a little piece of bread, very moiſt, and a little kneaded, with a mercurial gage, *in vacuo*.

July 12. In ſix hours time, no air was produc'd yeſterday; but this night a little broke into the receiver, and ſuſtain'd 3 inches of mercury; for I had neglected to fortify the cover with turpentine.

Towards the evening I found the mercury higher by about an inch, and am very certain that nothing had enter'd from without.

July 13. This night alſo the mercury aſcended higher, but my gage was not exact enough to diſcover how many degrees.

July 26. The bread disjoin'd its receiver from the cover, by the force the air produced, and the ſmell of it was acid.

Hence it follows, that water is a fit menſtruum to draw air out of bread.

(2.) *July* 11. I try'd to extract air from bread by the help of a burning-glaſs, wherewith I burnt bread *in vacuo*, and found it generate much air, which, ever and anon, broke out, as by fulmination; whence it ſeems probable, that air is contain'd in bread, but ſo cloſely compacted therein, that no eaſy operation can give it vent; but that if any thing could diſſolve and looſe that knot, it may then produce great effects.

From grapes.

(3.) *Sept.* 22. I took 8 ounces of dry'd grapes, and with 7 ounces of water, included them in a receiver, able to hold 22 ounces of water. The grapes were bruiſed.

Sept. 23. The receiver lay bury'd under the water all this night, yet the mercury aſcended two whole inches.

Sept. 30. In ſeven days time the mercury roſe to the height of thirteen inches.

Octob. 5. In five days more the mercury aſcended 12 inches, and was now 25 inches high.

Octob. 18. The mercury continu'd not to aſcend with the ſame ſwiftneſs, and the air began to paſs out of the receiver; but not before this day; yet theſe grapes produc'd much more air, than thoſe which I included without water.

From raiſins. (4.) *July* 12. I included 10 ounces of raiſins of the ſun, bruiſed *in vacuo*, with a ſufficient quantity of water, to promote fermentation.

July 14. In two days they had produc'd ten inches of air.

About evening the mercury was 15 inches high; the 15th day the mercury had almoſt reach'd to its accuſtom'd height.

July 16. In the morning I found the receiver ſever'd from its cover; and the air breaking out thro' the water, in which it was plunged, I included the ſame raiſins again *in vacuo*.

July 18. This day, in the morning, I found the air again breaking out.

July 19. I ſhut up the ſame raiſins in the ſame empty receiver.

July 21. This day I found the receiver full, and the air breaking out of it.

I again ſhut up the ſame raiſins in the ſame exhauſted receiver.

July 23. Yeſterday, about noon, I found the whole receiver almoſt full of air; and this day, in the morning, perceiv'd it to paſs out very often.

It appears then, that grapes without water, can generate but little air; whence it is manifeſt, that water is a fit medium to draw air out of them; it is alſo evident, that the production of air is not begun preſently upon the affuſion of water, but proceeds with greater ſwiftneſs, after the parts of the water, in 5 or 6 days time, have more deeply ſunk into, and pervaded the grapes.

From plumbs. (5.) *Aug.* 13. 1677. I included pears in two exhauſted receivers, and plumbs in another.

Aug. 16. In three days time all my receivers were fill'd with air, newly generated; and one of them, which included the pears, becauſe I had left it expoſed to the ſun, was, in the ſpace of 24 hours, ſeparated from its cover; whence we may conjecture, that the production of air is very much promoted by the heat of the ſun.

From grapes. (6.) *Octob.* 16. 1677. I took two ounces of grapes bruiſed, and ſecur'd them from the air, in an exhauſted receiver, capable of containing 20 ounces of water.

Octob. 17. The mercury roſe higher about one half inch.

Octob. 18. Theſe laſt 24 hours, the mercury ran up about another half inch.

Octob. 20. The height of the mercury was two inches.

On the 22d it was almoſt 4. And on the 27th it was almoſt ſix inches.

Jan. 2. 1678. The mercury, yet, aſcended not to the height of ten inches.

Octob. 16. 1677. I put three ounces of bruis'd grapes, with half an ounce of ſpirit of wine, into a receiver, able to hold thirty ounces of water; and then I exhauſted the air.

Octob. 17. The mercury aſcended but a very little.

Octob. 18. The mercury came not up to the height of one quarter of an inch.

Octob. 20. The mercurial gage was out of order.

Jan. 2. 1678. I, this day, found my receiver fill'd with air; and, alſo, when part of the liquor was pour'd out, ſome bubbles were form'd in the turpentine, about the orifice, and broke outwardly.

From this experiment, made in two receivers together, it ſeems to follow, that ſpirit of wine much advances the production of air *in vacuo*; tho', in common air, it wholly hinders it.

From muſt.

(7.) *July* 19. 1678. I put muſt, expreſs'd from grapes bruis'd, and kept for ten months in a veſſel, ſtopt with a ſcrew, into the ſame receiver, being alſo ſtopt with a ſcrew.

July 21. The mercury had not aſcended at all.

23*d*. The height of it was three.

24*th*. The height was five.

25*th*. In the morning it was an hundred and four.

Towards the evening, the height was an hundred and thirty ſeven; and the muſt got out.

26*th*. The muſt was almoſt all got out of the receiver; and altho' the air now poſſeſſed double the ſpace it did yeſterday, yet it kept up the mercury to the ſame height.

27*th*. About half of the remaining muſt broke out this night, becauſe I had omitted to ſet the ſcrew, leſt the receiver ſhould be broken.

From this experiment, it follows, that grapes, kept for ſo long a time, rather acquire, than loſe a fermentative virtue.

From apples.

(8.) *Jan.* 30. I put two quantities of apples, boil'd the day before, into two receivers, ſtopt with ſcrews; with one of them I mix'd a third part of ſugar, the other had no ſugar at all. Theſe receivers were quite full.

Jan. 31. I included raw apples, bruis'd, in three receivers; in one of them I mix'd a third part of ſugar; the ſecond was without ſugar, and ſo was the third; but it differed herein from the ſecond, that it was ſix times as big; for, by this means, we may know, whether the capacity of the veſſel, or the mixing of ſugar, or the crudity of the fruit, can promote, or retard the production of air.

Febr. 10. In that receiver only, which contain'd the raw apples, with ſugar, ſome air was produc'd.

Febr. 14. The raw apples, with ſugar, had impell'd the mercury up to thirty inches; thoſe that were boil'd with ſugar, to two only; in the other receivers no air was produc'd.

Febr. 18. In the receiver, containing the raw apples, with ſugar, the mercury came to the height of fifty-ſix inches; in that containing the boil'd apples with ſugar, the height was three: in the other receivers, there was, alſo, ſome air produced, except in that wherein the boiled apples, without ſugar, were put. I open'd that receiver, in which the apples had produced ſo great a quantity of air; yet the apples ſeem'd hardly to be fermented, but had a moſt pleaſant taſte.

Febr. 21. The boil'd apples, without ſugar, had loſt ſome of their juice; and, opening the receiver, I found the cover broke, and yet the apples were not at all rotten.

March 1. In the great receiver, containing the raw apples, the mercury was twenty-five inches high; in the little one, only ſeven: but in that where were the apples boil'd with ſugar, the mercury had aſcended to nine inches.

March 8. In the great receiver, the height of the mercury was twenty-nine; in the leſſer, twenty-two and a half; and where the boil'd apples with ſugar were, the altitude was nine inches.

March 17. The juice got out of the great receiver; in the little one, the height was ſixty-ſeven; where were the apples boil'd with ſugar, it was fifteen inches.

From this experiment it ſeems, that ſugar, the crudity of the fruit, and the largeneſs of the receiver, all contribute to the production of air.

Several ways to hinder the production of air; for inſtance, in paſte.

(9.) *December* 21. 1678. I made paſte of wheat-flower, without leaven, and put it into an exhauſted receiver; then I put the receiver in an apartment, with a fire, which there kept a greater heat than is uſual in the middle of ſummer; yet the paſte produced no air in ten hours ſpace: whence it ſeems to follow, that if dough hath once ſuffer'd too much cold, it can ſcarce recover its faculty of fermenting; for, ſome years ago, when I made dough without leaven, in the ſummer-time, it ſoon produced very much air *in vacuo.*

(10.) *May* 23. I included three ounces of dough, kneaded with leaven, in a receiver, capable of holding fifty ounces of water; I alſo pour'd upon it ſome quantity of ſpirit of wine, to try whether fermentation would be hinder'd by that means.

May 24. The mercury was three inches high.
26. Little change.
27. No change.
May 29. No change.
June 2. It ſeem'd to have aſcended a little higher.
14. No change.

December 14. No more air being produced from the dough, I took it out of the receiver, and found the ſmell of it not grateful, but inclining to acid: I put it into an empty receiver, and there it ſwell'd to double its uſual ſpace, and made a little ebullition.

May 23. I included three ounces of dough, kneaded with leaven, in a receiver, able to hold fifty ounces of water; but here I mixed no ſpirit of wine.

Fig. 74. p. 560.

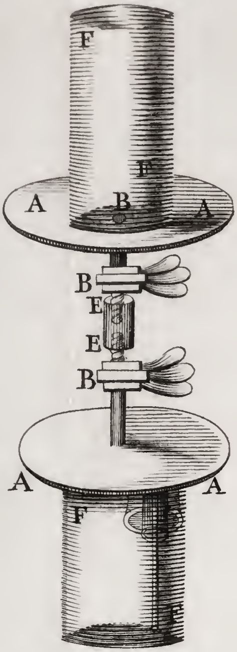


Fig. 75. p. 560.

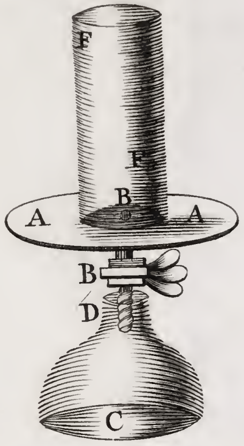


Fig. 76. p. 561.

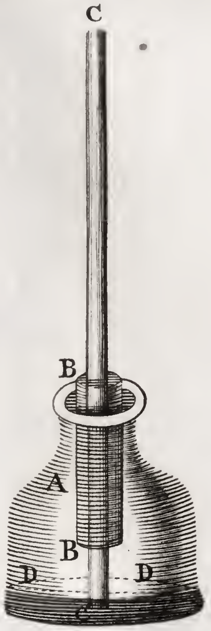


Fig. 77. p. 561.

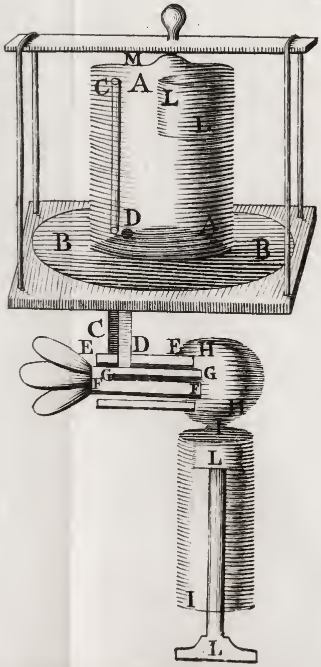


Fig. 78. p. 562.

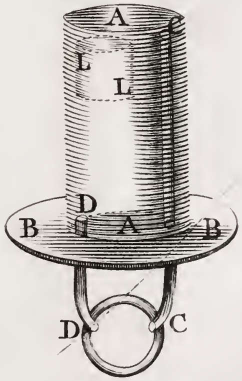
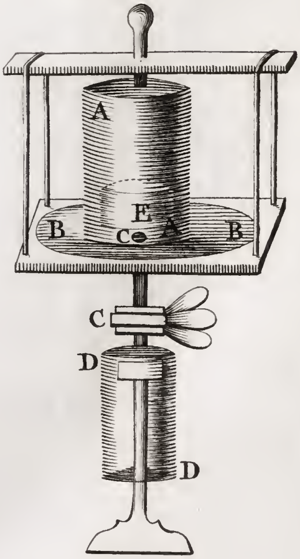


Fig. 79. p. 562.



J. Mynde

G

May 24. The mercury was nineteen inches and a half high.

May 26. 'Twas 38 inches high.

27. There was no change.

December 14. The mercury continued at the ſame height; and this day opening the receiver, I found the dough had a very acid ſmell.

From this experiment it ſeems to follow, that ſpirit of wine, even in dough kneaded with leaven, hinders the production of air.

In pears.

(11.) *Auguſt* 29. I included pears, with a mercurial gage, in a receiver full of water, and then intruded air into it, till the mercury reſted twenty-ſix inches higher than uſual; within a quarter of an hour, one of the pears was broken, and afterwards, almoſt all of it reduced to pulp.

Aug. 30. In twenty-four hours ſpace, the pears ſeem'd to have afforded no air; but on the contrary, the mercury in the gage was depreſſed an inch and a half.

Aug. 31. I found no change in the height of the mercury.

Sept. 1. The pears began to produce air, and the mercury was almoſt twenty-ſeven inches high.

Sept. 2. In twenty-four hours time, the mercury aſcended more than eight inches; and now 'twas thirty-five inches high.

Sept. 3. The height of the mercury was increaſed ſeventeen inches; ſo that now it was about fifty-two inches high.

Sept. 4. Within twenty-four hours, the mercury roſe ſeven inches higher, and then reſted at fifty-nine.

Sept. 5. It was ſixty-four inches high; and a pear being broken, was become black.

Sept. 6. Three inches and more, being added to the height of the mercury, it came now to ſixty-ſeven inches, and one fourth, beyond what it was accuſtom'd.

Sept. 7. It deſcended three inches, and reſted again at ſixty-four.

Sept. 8. The mercury was depreſſed to fifty-eight inches; and ſome of the water having broke out, I ſet the receiver with a ſcrew.

Sept. 9. The mercury aſcended full three inches, and was now ſuſpended above ſixty-ſeven.

Sept. 10. In twenty-four hours it mounted one and a half, and ſtopp'd almoſt at ſixty-nine.

Sept. 11. Now it began to deſcend again, and ſtood no higher than ſixty-ſeven inches; yet I am certain, nothing had eſcaped out of the receiver; but it was a ſharp cold night.

Sept. 12. No change happen'd.

Sept. 13. The height of the mercury again decreaſed, and it was not above ſixty-four inches. The cold increaſed.

Sept. 14. In twenty-four hours it became higher by ſix inches, reaching to ſeventy.

Sept. 16. It was about ſixty-nine inches high.

19. It remained the ſame.

Sept. 20. It again aſcended to 71.

23. The mercury was again depreſſed to ſixty-nine.

Octob. 1. It came to the height of ſeventy-five inches.

PNEUNATICS. *Octob.* 3. Yeſterday I found no change at all in the mercury ; but to-day it reſted at ſeventy ; and the cold was very ſevere.

Octob. 5. Yeſterday the mercury remained in the ſame place ; but this day it reach'd to ſeventy-five. It was a rainy day.

Octob. 7. It continu'd rainy ; and the mercury continu'd at the ſame height.

Octob. 10. Hitherto the mercury was not changed ; but this day I found it had deſcended to ſixty-nine inches ; tho' the rain ceas'd not.

Octob. 12. Yeſterday the mercury ſtood ſtill ; but this day it was depreſs'd to ſixty-five inches ; and the cold weather return'd.

Octob. 13. The height of the mercury was ſixty four.

14.	The height was	ſixty-nine.
15.		ſeventy-four.

24. The height was ſixty-eight. It was a cold ſeaſon.

Nov. 2. The height was ſixty-four. The cold increas'd.

Nov. 5. The height was eighty and a half. The cold abated.

22. The height was ſixty-five. It was a hard froſt.

27. The height was ſixty-eight. It thaw'd.

Decemb. 6. The height was ſixty-one. It was a very ſevere froſt.

From this experiment we may learn, that fruits, in a great compreſſure of the air, cannot produce ſo great a quantity of air ; for when I made an eſtimate of the quantity of the fruits, and of the ſmall ſpace to be fill'd with air ; I found that quantity of air was not one eighth part of what had been produced in a large empty receiver : tho' the coldneſs of the water might alſo, hinder the generation thereof, as the following experiment will ſhew.

'Tis farther manifeſt, that the air is produced by ſtarts, and as it were, by reciprocations ; as all bodies in motion, by the force of their gravity, or of their ſpring, are carried beyond their point of reſt, and ſo make many vibrations, or returnings. And tho' cold and heat are not the ſole cauſes of ſuch reciprocations, yet they ſeem to contribute much thereto.

In paſte again. (12.) *Feb.* 22. 1677. I included ten ounces of paſte in a receiver, that would hold twenty-two ounces of water ; and afterwards, I thruſt as much air into it, as ſufficed to ſuſtain ſeventy-three inches of mercury beſides the wonted preſſure. In two hours ſpace I perceived no ſenſible change.

Febr. 23. In eighteen hours time, the mercury roſe ſeven inches only, its height being eighty.

In ſix hours it aſcended three ; and its height was eighty-three.

Febr. 24.	Its height was	90
25.		97
26.		101
27.		105
28.		107½
March 1.		112

And water ſeem'd to be expreſs'd out of the maſs.

March 2.	Its height was	120
3.		121

4 & 5. It remain'd at 121.

March 8. During theſe two or three laſt days, the froſt breaking, the mercury ran up 4 inches; and the height thereof was 125.

March 10. Yeſterday the mercury remain'd at the ſame height, but this day mounting 6 inches, it reſted at 131.

March 21. The cold continuing long, no air was produc'd; but in the three laſt days the mercury aſcended 7 inches, and remain'd at 138.

April 4. Yeſterday the mercury had aſcended, but I deferr'd meaſuring the quantity till to-day; in the night one of the iron wyres, that ſtraiten'd the receiver, was broken, and the receiver thrown to the diſtance of 4 or 5 foot.

Hence we may conjecture, that the compreſſion of the air very much hinder'd the production thereof; for that is uſually perform'd in paſte, in 2 or 3 days time. Cold alſo much hinders its production.

In raiſins and vinegar.

(13.) *March* 1. 1677. I included 2 ounces of bruiſed raiſins of the ſun, with 6 ounces of vinegar, in a receiver; upon which numerous bubbles broke out.

March 2. The mercury, in 24 hours ſpace, aſcended not to the height of half an inch; yet ſome bubbles ſtill appear'd.

March 25. The vinegar always appear'd interſpers'd amongſt ſome of the bubbles, yet the mercury aſcended not to the height of one inch.

Hence it appears, that vinegar hinders the production of air and fermentation; for raiſins of themſelves afford much air.

In paſte.

(14.) *April* 7. I included 10 ounces of paſte, in a receiver capable of holding 22 ounces of water; afterwards I intruded as much air into it, as ſuffic'd to ſuſtain 128 inches of mercury, beſides its accuſtom'd height.

In 6 hours time the mercury roſe 4 inches, and reſted at 132.

April 8. In 16 hours the mercury ran up 9 inches higher, and ſtay'd at 141.

Nine hours after, the mercury manifeſted no change.

April 9. In the morning I perceiv'd ſome air had broke forth, and the mercury was depreſs'd to 130 inches; therefore I ſcrew'd the receiver tighter, and thruſt in 11 inches of new air; the height was 141.

Apr. 10.	The height was	151	*Apr.* 14.	The height was	183
11.		158	15.		183
12.		168	16.		187
13.		176	17.		191

April 27. For eight whole days the mercury kept its ſtation; but on the two laſt it aſcended 7 inches, and continu'd at 198, above its wonted height.

April 30. The mercury perſiſting at the ſame height, I eaſed the ſcrew, ſo that ſome air might break out; and when the mercury had ſo far deſcended, as to exceed its accuſtom'd height, only 50 inches, I preſently ſet the ſcrew, to ſee whether, that remiſſion of the ſpring of the air, would afford any place for new air to be generated; and in 2 or 3 minutes time I found the mercury to have aſcended ſenſibly higher.

Three hours after, the mercury was found 12 inches higher; for it came to 62.

In five hours it aſcended one inch and a half.

May 1. In 15 hours the mercury roſe only one inch.

May 3. Yeſterday it appear'd at the ſame height, but this day it was higher by one and an half, and remain'd at 66.

May 4. The mercury was not chang'd, and therefore I ſuffer'd all the air to eſcape; but I could not quickly ſet the ſcrew; whence it is probable, that very much air, which at that time was produc'd, got out of the receiver; nevertheleſs, after the receiver was again well ſtopt, I perceiv'd that 2 inches of air and more had been produc'd in 5 or 6 minutes time.

May 7. The mercury, in 3 days, again amounted 2 inches.

May 8. The mercury was higher by half an inch.

May 11. During theſe two laſt days, the mercury again ran up half an inch. I ſet this maſs, almoſt unfit, as it ſeem'd, to produce air *in vacuo*; when in 5 minutes ſpace, the mercury aſcended to the height of an inch.

May 21. It aſcended not quite 3 inches.

May 30. The mercury reſted at the height of 4 inches and a half.

By this experiment it appears, that all the air producible from paſte, may, after a ſort, be generated in a great compreſſion; yet it is ſomewhat reſtrain'd thereby, for in a leſs compreſſion it will ſoon break out.

Hence alſo we ſee, that air is producible by ſtarts; and that it riſes more ſlowly in compreſs'd than in free air; for ſuch a production in the latter, is uſually over in 2 or 3 days time.

Plumbs and apricocks in artificial air.

(15.) *July* 30. 1677. I included plumbs and apricocks, many of them being firſt cut aſunder, in a receiver, and afterwards as much air produc'd out of cherries, as was ſufficient to ſuſtain 64 inches of mercury.

Auguſt 1. The fruits had produc'd no air, but grew yellower than thoſe which were in common air.

Auguſt 3. The mercury roſe a little higher, and the apricock, which remain'd whole, ſeem'd full of drops, like water.

Auguſt 7. The whole apricock grew ſofter; the mercury ſtood at 59 inches above its uſual ſtation.

Aug. 8.	The height of it, was	61	*Aug.* 13	The height of it, was	78
9.		65	14		80
10		71	15		80
11		74	16 and the days following it remain'd at the ſame height.		

24. The height of it was 77; tho' I certainly knew that nothing had iſſued out of the receiver.

29. Finding neither the fruits, nor the height of the mercury, changed any more, I open'd the receiver, and perceiv'd, that the apricocks had kept their colour very well; but the fleſh of them was ſpongy, and their taſte inclined to acid. Many bubbles had broke from them, at the time they were freed from the ſurrounding preſſure.

Plumbs and apricocks in common air.

July 30. 1677. I included the halves of the fruit, juſt mention'd, in a receiver full of common air; and with them, others of the ſame kind, uncut.

July 31. The mercury had gain'd the height of eight inches.

Auguſt 1. At ſix a-clock in the evening, the mercury was twenty-one inches high; but, in the other receiver it remain'd unmov'd.

Auguſt 3. They kept their firmneſs much better than thoſe included with artificial air. The height of the mercury was thirty-five inches.

Auguſt 4. The height of the mercury was forty-two inches.

Auguſt 6. The whole apricock ſeem'd not at all alter'd. The height of the mercury was fifty-ſeven.

Aug.	The height of it, was
7	81
8	95
Aug. 9	113
10	124

The colour of the whole apricock, yeſterday, began, and now proceeded to grow yellow. No moiſture appear'd.

Aug.	The height of it, was
11	131
13	157
14	163
Aug. 15	171
16	171

17 and the days following, it remain'd at the ſame height.

Auguſt 27. The height was one hundred and eighty-two.

Auguſt 29. When, neither the fruit, nor the height of the mercury changed any more; I open'd the receiver, and found the apricocks of a more acid, and leſs grateful taſte, than the others, in factitious air; tho' their pulp was of a very good colour, but ſpungy: they alſo yielded many bubbles, as did the others.

Hence, 'tis probable, that the artificial air of the cherries, greatly hindred the apricocks from producing air; tho' it promotes the alteration of their colour and firmneſs; and alſo, ſerves to preſerve their taſte.

Grapes in common air.

(16.) *October* 10. 1677. I included an ounce and an half of bruiſed, unripe grapes, in a receiver, that would hold ten ounces of water; and drew out no air.

Octob. 11. The mercury aſcended a little.

12. There was but a ſmall change.

13. The height was half an inch.

17. The height was one inch.

18. The height was one and an half.

19. The height was almoſt four.

20. The height the ſame; but ſome mouldineſs appear'd on their ſuperficies.

21. The height was four and an half.

22	The height remain'd the ſame; but the mouldineſs encreaſed.
23	
24	

	The height was
26	$5\frac{1}{2}$
27	6
30	$6\frac{1}{2}$

Nov. 2. The height was ſeven and an half.

	The height was
Nov. 6	9
8	10
9	12
12	15
14	17
Nov. 18	23
21	26
Dec. 8	36½
12	39
27	39

Jan. 6. 1678. The height was 36. The air broke out.

Grapes with ſpirit of wine.

Octob. 10. 1677. I made the ſame experiment in another receiver, obſerving the ſame circumſtances; only here I mixed two drams of ſpirit of wine with the grapes. –

Octob. 11. The mercury was not changed.
12. There was no change.
13. The mercury was not moved.
Oct. 17. It aſcended a little.
18. The height of it was not yet a quarter of an inch.
19. It was moved but a very little.

Jan. 6. The grapes, during all this time, had produced no air.

Whence it appears, that ſpirit of wine hinders fermentation.

A peach in an exhauſted receiver, with ſpirit of wine.

(17.) *Octob.* 17. 1677. I put a peach into an exhauſted receiver, with ſome quantity of ſpirit of wine, which could not touch the peach, unleſs in vapour.

March 27. 1678. I took out the peach, which had kept its colour, but loſt its firmneſs. Though the receiver was ſmall, yet it was not filled with air; for when open'd, the air ſeem'd to ruſh into it: the peach being ſoftned, was ſo depreſs'd, that the lower part of it touch'd the ſpirit of wine; the ſuperior part, alſo, had contracted the taſte of the ſpirit of wine, as well as that which was immerged in it.

Peaches in air with ſpirit of wine.

(18.) *Octob.* 17. I included five peaches in an unexhauſted receiver; and with them, ſome ſpirit of wine, which could not touch the peaches, unleſs it were elevated in vapour.

Octob. 18. The mercury aſcended not at all.

20. The height of the mercury was three and an half.

	The height of it was
21	5½
22	7½
23	9
26	9½
Nov. 2	12

	The height of it was
Nov. 6	14
12	16

14, 16. It kept the ſame height.

	The height of it was
Dec. 8	18
16	19½
27	20½

Jan. 6. 1678. It was 23.
March 28. 1678. It was 31½.

Peaches in air without ſpirit of wine.

Octob. 17. I included five peaches in a receiver full of common air, without ſpirit of wine.

Octob. 11. The mercury aſcended not at all.

Octob. 20. The height of the mercury was five inches.

Octob. 21	The height of it was	8
22		10
23		11
26		12
Nov. 2		15
6		17 $\frac{1}{2}$

Nov. 12	The height of it was	20
14		20
16		21
Decemb. 8		26
16		26 $\frac{1}{2}$
27		28 [illegible]

Jan. 6. 1678. The height was 32.

March 28. 1678. The height was 33 $\frac{1}{2}$.

April 15. The liquor in the lower part of the receiver, had all broke out, and the air followed it; then I took out the peaches.

Hence we learn, that the very vapour of ſpirit of wine, ſomewhat hinders fermentation; yet much leſs than the ſpirit itſelf.

Paſte with leaven, in common air.

(19.) *April* 27. 1678. I included an ounce and a half of paſte, mixed with leaven, in a receiver full of common air, able to hold twenty-three ounces and a half of water.

April 28. The height of the mercury in the gage was two and a half.

April 30. The height of it was three and a quarter.

May 4. The mercury was depreſſed, tho' no air broke away, and the paſte was mouldy. The height of it was two and a half.

May 6	The height of it was	2 $\frac{3}{4}$
8		3
10		3 $\frac{1}{2}$
14		4

May 17	The height of it was	4 $\frac{1}{2}$
20		5
24		6
28		8

June 2	The height of it was	9
6		10
14		10 $\frac{1}{2}$

July 5	The height of it was	13 $\frac{1}{2}$
19		15

Paſte without leaven in common air.

April 27. 1678. I included an ounce and a half of unleavened paſte, with common air, in a receiver, capable of holding twenty-three ounces and a half of water.

April 29. Hitherto the mercury had not aſcended; but this afternoon it roſe a quarter of an inch.

April. 30. There was no change.

May 4. The mercury aſcended but very ſlowly, and the paſte was mouldy.

May 6. The height of the mercury was four inches.

May 8	The height of it was	5 $\frac{1}{2}$
10		7 $\frac{1}{2}$
14		10 $\frac{1}{2}$
17		12 $\frac{1}{2}$
20		13 $\frac{1}{2}$

May 24	The height of it was	16
28		18 $\frac{1}{2}$
June 2		20 $\frac{1}{2}$
6		21 $\frac{1}{2}$
14		25

Hence it ſeems, that leaven rather hinders than forwards the production of air, if the paſte be not made in a hot place.

Paſte with ſpirit of wine.

(20.) *May* 23. I included an ounce and a half of unleavened paſte, in a receiver capable of holding twenty-five ounces of water, and pour'd ſpirit of wine upon it.

May 24. The mercury was one inch high.

May 26. It was almoſt two inches high.

27. It was two and a half.

31. There was no change.

June 2	The height of it was	3½
6		4
10		4½

July 19. No change.

Decemb. 14. When the height of the mercury alter'd no more, I open'd the receiver, and found that the paſte had an acid ſmell.

Paſte without ſpirit of wine.

May 23. I included an ounce and a half of unleavened paſte, in a receiver, capable of holding twenty-five ounces of water; but added no ſpirit of wine.

May 24. The mercury aſcended not.

May 26. It was three inches high.

May 27	The height of it was	4½
28		5½
29		7
30		9½
June 2		12
June 6	The height of it was	17
10		22
July 4		30

19 The mercury a little exceeded thirty inches. This day the air broke out, and, therefore, I ſet the ſcrew.

Decem. 14. The mercury return'd to the height of fifteen inches; when, I open'd the receiver, and found the paſte very acid.

Hence it ſeems to follow, that ſpirit of wine greatly obſtructs the production of air; and the more, if the paſte be fermented; and that unfermented paſte will, in tract of time, produce no leſs air than that which is fermented.

New ale included in receivers.

(21.) *Octob.* 11. I exactly filled a receiver with new ale, ſo that no air might be left; and included another quantity of the ſame in another receiver, wherein ſome ſpace was allowed for the air.

Octob. 12. The cover of that receiver, which contained ſome air was broken; and, therefore, I poured the ſame ale into another receiver, wherein there was room enough left for the air: in the receiver, exactly fill'd, the mercury aſcended a little.

Octob. 13. In the receiver, exactly fill'd, the height of the mercury was twelve inches; in the other, thirteen inches; tho' it had been ſhut up a ſhorter time, and a much larger ſpace was left, whereinto the air, newly produced, might have been dilated.

Octob. 14. In the full receiver, the height was thirteen; in the other eighteen. Towards evening, the full receiver work'd the faſteſt; for the height of the mercury in it was twenty-two; and the other but twenty.

Octob. 15. In the full receiver, the height of the mercury was forty-two; in the other, twenty-ſix. And ſome bubbles of air, which, in the full receiver, had poſſeſſed its upper part, wholly vaniſhed; and the ale poſſeſſed a long ſpace, in the mercurial gage, wherein it was not found before.

October 16. In the full receiver, the height was 60 inches.
In the other 30.
18. In the full receiver, the height was 90.
In the other 40.
22. In the full receiver, the height was 90.
In the other 42.
23. In the full receiver, the height was 108.
In the other 50.
26. In the full receiver, the height was 108.
In the other 60.
28. In the full receiver, the height was 133.
In the other 63.

The bubbles appear'd again, yet nothing flowed out.

Nov. 8. The full receiver loſt much of its liquor; wherefore, I open'd it; when all the ale ſeem'd as if it would have vaniſh'd into froth, unleſs I had ſuddenly ſtopt the little hole, that gave it vent. I many times tried, that, if the hole were opened in the gage, the mercury would preſently deſcend; but if the hole were again ſtopt, it would ſpeedily aſcend. The ale had a moſt pungent taſte.

Nov. 9. I opened the other receiver, and obſerved almoſt the ſame things.

Hence it ſeems to follow, that ale, if the air be wholly excluded from the containing veſſel, will ferment more ſlowly, than if ſome air be left therein; and that, in time, it makes a greater compreſſion, if no room be left for its dilatation.

Peaſe with ſpirit of wine, in an exhauſted receiver.

(22.) *June* 27. I put green peaſe into an exhauſted receiver, with ſpirit of wine. Towards the evening, the receiver ſeem'd to admit the external air, and the mercury roſe to the height of eighteen inches, when I cloſed the cover with turpentine.

June 30. I perceiv'd no more change in the height of the mercury.

July 7. No air was produced, even in the moſt vehement heat.

Peaſe without ſpirit of wine, in an exhauſt-receiver.

June 27. I put other peaſe into an exhauſted receiver, without ſpirit of wine. The receiver, and the quantity of the peaſe, were the ſame as in the laſt experiment.

June 28. The receiver was full of air; tho', I think, it was not exactly ſhut; and therefore, I again included the ſame peaſe. Towards evening, the height of the mercury was five inches.

June 29	The height of it was	10
30		16
July 1		19
July 5	The height of it, was	26
7		30

July 8. The air got out of the receiver.

Hence it appears, that ſpirit of wine hinders the production of air in peaſe.

That the effects of artificial air differ from thoſe of the common, ſhewn in cherries.

(23.) *June* 9. 1677. I put cherries into an exhauſted receiver, and in ſix hours time the mercury came to the height of five inches and a half.

June 20. The mercury aſcended three and a half. Towards the evening it was two.

The aſcent is here always to be underſtood, as added to the former.

June 21	The aſcent was	1½
22		1½
23		2
24		1½
25		1½

June 26	The aſcent was	3
27		3
28		5
30		1¾

July 1	The aſcent was	3
2		4
3		2

July 4	The aſcent was	2½
5		3

The height was forty-eight. But, tranſmitting the air into another receiver, the mercury was depreſſed to thirty-five inches.

July 6. The aſcent of the mercury was 4 inches in one night's time.

7. The aſcent of it was five and a half in twenty-four hours time.

8.	The aſcent of it was	5.
9.		5.
10.		6.

11. The aſcent of it was twelve, in the ſpace of thirty-four hours.

12. The aſcent of it was ſeven.

13. The aſcent of the mercury was three; the height about ninety-two inches: but the air being transferred into another receiver, the mercury reſted at fifty.

14	The aſcent was	14
15		11

16	The aſcent was	13
17		5

18. The aſcent of the mercury was 9; the height of it 102.

19. The height of the mercury was 92; for I tranſmitted part of the air into another receiver.

20. The aſcent of the mercury was fifteen.

22. Some air got out, and the height of the mercury was 63 ½.

23. The aſcent of it was 12 ½.

24. The aſcent of the mercury was 4; the height of it 79 inches; but the air being tranſmitted into another receiver, the mercury reſted at 62.

25	The aſcent was	8
26		9

27	The aſcent was	4
28		5

30. The aſcent of it was 10; the height ninety-eight. Part of the air being tranſmitted into another receiver, it reſted at ſixty-four.

31	The aſcent of the mercury was	6
Aug. 1		9
2		4

3. I tranſmitted the air into another receiver, and the mercury remain'd at ſixty-eight.

4. I tranſmitted the air again into another receiver, and the mercury reſted at fifty-four.

6. The

6.	The aſcent of the mercury was	7.
7.		4.

8. There was no aſcent.
9. The aſcent was three inches.

The receiver being open'd, I found the cherries of a whitiſh colour, and of very little taſte, though not ungrateful; their fleſh was ſpongy.

Hence it ſeems to follow, that cherries contain much air, and that they produce it very irregularly.

(24.) *July* 13. 1677. I put cherries into an exhauſted receiver; and then tranſmitted into the ſame, as much air, produced from other cherries, as ſufficed to ſuſtain fifty inches of mercury.

July 15. Yeſterday, the mercury had not aſcended at all; but this day it was two inches higher; that is, twenty-two, above its wonted ſtation.

July 16. The height of the mercury was twenty-three and a half.

July 17. The height of it was twenty-five.

26. The height of it was forty-three. Some air got out.

27. The height of the mercury was forty-five. More air eſcaped.

30. The height of it was fifty-two.

31. The height of it was ſixty-one inches.

Auguſt 1. The height of the mercury continued nearly the ſame, though the air broke out.

Auguſt 27. The air, having been all broke away for ſome time, I took out the cherries, and found them not to have loſt their colour, as in the former experiment: they had contracted no putrefaction, nor mouldineſs, but taſted a little more acid than uſual; and being opened, there were many cavities in their fleſh, as in fermented paſte, or dough, but not quite ſo thick.

From this experiment, compared with the former, it may, probably, be inferred, that in artificial air, fruits produce leſs air, and ſo the better preſerve their colour and taſte; for the cherries, in the former experiment, remain'd in the receiver, not much longer than in this.

Grapes in common air.

(25.) *September* 10. 1677. I put ſix ounces of unripe grapes into a receiver, with common air, capable of containing twenty-five ounces of water; and ſtopp'd it firmly by means of a ſcrew.

Sept. 11. The mercury aſcended not at all.

Sept. 12. The mercury ſtopp'd a little below one inch.

Sept.	The height of it was	*Sept.*	The height of it was
13	3½	18	16
14	7	19	18
15	10	20	20
16	12½	21	22
17	14	22	23½

September 23. The height of it was 27. The grapes were not altered.

September 24. The height was 30.

25. The height was 31. The grapes began to grow yellow.

Septemb. 26	The height of it was	32	*Sept.* 29	The height of it was	35
27		34½	30		35

October 1. The height remain'd at 35.

October 2	The height of it was	36	*Octob.* 10	The height of it was	35
5			25		32½
6					

The air got not out, but the cold began, and increaſed.

November 9. The height remained the ſame.

December 19. Almoſt all the air eſcaped.

December 20. I took out the grapes, and found by their ſmell and their taſte, that they had contracted ſome mouldineſs, though not diſcernible by the eye. They were more firm than before.

Grapes in factitious air.

(26.) *Sept.* 10. 1677. I included two ounces of crude grapes in a receiver, capable of holding eight ounces of water; and to the common air added air produced out of pears, till the mercury reſted ten inches above its ordinary ſtation.

Sept. 11. The mercury deſcended, and its height was eight inches.

Sept. 12 The height of it was 11. The aſcent 3.

Sept 13	The height of it was	16	*Sept.* 15	The height of it was	23
14		20	16		24

Sept. 17. The height was 28. The grapes turned yellow.

Sept. 18	The height of it was	29	*Sept.* 22	The height of it was	35
19		30	23		20
20		31	Some air had broke out; and the grapes were of a yellow colour.		
21		33			

Sept. 24	The height of the mercury was	25
25		22

26 The height almoſt the ſame.

27	The height of it was	22
29		27
30		28

Octob. 1. and 2. The height 28.

Octob. 5	The height was	30	*Octob.* 10	The height was	31½
6		31	13		31

Novemb. 9. The height was 13. Some air had got out.

December 19. The height of the mercury was 20 inches.

Decemb. 20. I took out the grapes, and their ſmell and taſte were more grateful than of others; their firmneſs rather increaſed, than diminiſhed.

Hence, factitious air ſeems fit to alter colour, and to preſerve taſte; but the firmneſs might be increaſed here, as in turpentine; the ſpirits, in time, being exhaled.

Oranges in common and factitious air.

(27.) *July* 18. I took two pieces of orange, and, by the help of a ſcrew, ſtopped them cloſe up in a receiver, with common air; when, into the ſame receiver, I put air, produced out of cherries, as much as ſufficed to ſuſtain

12 inches of mercury. At the ſame time I put a piece of the ſame orange into another receiver, with common air alone, and uncompreſs'd.

July 20. The orange in the common air began to contract a mouldineſs, but the other ſeemed not at all altered.

July 23. The mouldineſs of the orange, in the common air, increaſed; the other piece remained ſound.

July 16. The orange, in the common air, did not increaſe its mouldineſs, but ſeemed wholly rotten; the other alſo began to putrify, but remained free from mouldineſs.

Aug. 1. Perceiving that the oranges were no longer ſenſibly changed, I opened the receivers; and tho' the air, wherewith I had mixed the artificial air, was ſo compreſs'd in its receiver, that it could not now ſuſtain 26 inches of mercury above its wonted preſſure, yet the fruits were far better preſerved in it than in the other; only the ſuperficies ſeemed to have loſt its juice; but all the inner parts, with the rind, were very well coloured, well taſted, and firm; in the other receiver the whole orange ſeemed almoſt rotten, as well as the rind. The orange was more corrupted in the compreſſed air, becauſe, as it ſeems, no factitious air had been mixed with it.

It ſeems worth obſerving, that the ſame air generated from cherries, is apt to produce different effects upon fruits of a different kind; for here it retarded the alteration of colour and firmneſs, which, when I included air with apricocks, it accelerated.

Beef in factitious air.

(28.) *July* 20. 1676. I included a ſmall piece of beef in an exhauſted receiver, and put as much air produced from cherries into it, as ſuſtain'd 27 inches of mercury.

July 21 22 23 25	The mercury remained almoſt at the ſame height.

July 26. The beef had removed the receiver from its cover; and becauſe it was very fetid, we threw it away.

Beef in common air.

July 20. 1676. I put a piece of beef into a receiver full of common air, and carefully ſtopt it in by means of the ſcrew.

July 21. The mercury had not at all aſcended in the gage.

July 22. The height of the mercury was one inch.

23	The height of it was	5 ½	
25		9 ½	
26		14 ½	In the evening 18.
27		21 ½	In the evening 25.

28. The ſcrew not being tight, ſuffered the air to break out.

Hence it appears, that air produced from cherries, is a great hindrance to the production of air from fleſh.

Onions in common air.

(29.) *March* 14. 1676. I put 2 onions into a receiver full of common air, with a mercurial gage, and faſten'd the ſtopple with a ſcrew, to ſee whether vegetation would increaſe or diminiſh the quantity of the air.

March 28. The mercury ſeemed depreſſed one quarter of an inch; but it afterwards recovered its former height, and two inches more; and now the air broke out, and the roots grew longer.

April 28. About ten or twelve days ſince I perceived the roots to be corrupted; and now they were wholly putrified.

May 9. The mercury continued at the ſame height, for the air had broke away; and therefore I took out the onions, and found their roots putrified, but they were not at all mouldy.

Onions in factitious air.

(30.) *March* 17. 1676. I included two onions in an exhauſted receiver, and afterwards put air produced from paſte into it.

March 28. The onions took root, at leaſt as well as thoſe which I kept in the common air.

April 28. The ends of the roots began to putrify, yet they were in far better caſe than thoſe ſurrounded with common air. Perhaps the cauſe of this difference is, that a greater quantity of water was included with the artificial air. The mercury mounted higher by 9 or 10 inches.

May 18. Hitherto the onions ſeemed not at all corrupted; but this day I found one of them to be a little ſo, tho' different from a mouldineſs.

Hence we may gather, that artificial air doth not at all hinder vegetation; and that not only the ſenſible magnitude of the body, but alſo the quantity of the air, is increaſed by vegetation.

Unripe grapes in common air.

(31.) *Auguſt* 25. I included 6 ounces of unripe grapes in a receiver, capable of holding 25 ounces of water; but did not exhauſt the air.

Auguſt 26. The mercury aſcended a little.

27. The height of the mercury was one inch.

28. The height of it was 1 $\frac{1}{4}$.

29. The height was 1 $\frac{1}{4}$.

Auguſt 30. The mercury ſeemed to have deſcended, rather than aſcended. The colour of the grapes was leſs altered here, than in the receiver containing air produced from pears.

Aug. 31. The receiver broke, and I left the grapes expoſed to the free air.

Sept. 7. The grapes being left in the free air, ſtill kept their green colour, and were of a grateful taſte, tho' leſs pungent than before.

Unripe grapes in factitious air.

Auguſt 25. I included two ounces of unripe grapes in a receiver, capable of holding eight ounces and a half of water; and having ſtopt it cloſe with a ſcrew, I filled it further with air produced from pears, till it ſuſtained 15 inches of mercury.

Auguſt 26. Some air eſcaped, and therefore I crouded in new, produced out of the ſame pears, till the mercury reſted 17 inches above its wonted height.

Auguſt 27. The mercury was depreſſed below the 16th inch, yet no air had broke out. Towards evening the mercury again aſcended to 17.

Aug. 28	The height of it was	19	*Aug.* 31	The height of it was	23 $\frac{1}{2}$
29		21	*Sept.* 1		24
30		22	2		24

Sept. 4. The laſt height continued, and the grapes had all contracted a yellow colour.

Sept. 5. The air broke out.

Sept. 7. The air continued to get away, by degrees; I took out the grapes, and found them very inſipid, and of an ungrateful taſte.

This experiment confirms the efficacy of artificial air, to alter the colour of fruits. 'Tis alſo, very obſervable, that here it damaged the taſte, and promoted the production of the air, contrary to what had happened in the former experiments. It might be worth while to try, whether the ſame would happen in all unripe fruits.

A July-flower in factitious air.

(32.) *Auguſt* 2. 1676. I ſhut up a July flower in a receiver, with air produced from paſte, made with meal, and not mixed.

Aug. 4. The flower began to change its colour, and to grow moiſt.

Aug. 9. The July-flower was a little alter'd.

Aug. 12. The moiſture gradually increaſed, but no mouldineſs appear'd.

Aug. 31. The July-flower ſeem'd little alter'd, tho' it was leſs freſh than thoſe which were kept *in vacuo.*

In common air.

Aug. 2. I ſhut up a July-flower in a receiver, with common air, not mixed.

Aug. 4. The flower was not changed.

Aug. 9. It grew moiſt, and had almoſt loſt all its colour.

Aug. 12. A great mouldineſs cover'd all the flower.

July-flowers in vacuo.

Aug. 2. I included two July-flowers *in vacuo*, and took ſpecial care, that no humidity ſhould be included with them.

Aug. 4. 1676. One of them began to appear moiſt.

Aug. 31. 1677. During the whole year, the July-flowers had ſuffered no change.

Hence it ſeems probable, that factitious air haſtens the change of colour, yet it prevents mouldineſs as a *Vacuum.*

Apricocks and plumbs in common air.

(33.) *July* 24. I put apricocks and ſome plumbs, ſeveral of which were cut in pieces, in a receiver full of common air, and ſtopped it firmly with a ſcrew.

July 25. The mercurial gage was ſpoiled: ſo that I could not, by any means, perceive the quantity of the air generated.

July 30. The fruit ſeemed not at all alter'd, except that one of the cut plumbs had contracted ſomething of mouldineſs.

Aug. 2. I open'd the receiver, and found all the fruit firm, of a good colour, and a grateful taſte.

The ſame in artificial air.

July 24. I made the ſame experiment in another receiver, with the ſame circumſtances; only into this laſt receiver, I intruded air, produced from cherries, till it ſuſtain'd twenty-two inches of mercury.

July 25. The mercury deſcended three inches, and reſted at nineteen. Toward the evening, it recover'd its former height, and reſted at twenty-two.

July 26	The height of it was	28	*July* 28	The height of it was	36
27		34½	29		40

July 30. The height was forty-four. The apricocks which were cut, began to moiſten, and diſſolve into water.

July 31 } The height was { 51
Aug. 1 } The height was { 60

Aug. 2. The height was ſixty-five. Towards evening, when ſome liquor had eſcaped out of the receiver, I ſcrewed it tighter; but one of the iron wires being broken, all the air got away. Wherefore, I took out the fruits, and found them very ſoft; eſpecially thoſe, whoſe lower parts were immers'd in the water; the reſt were a little more firm, but all of them retain'd a grateful taſte.

Hence it ſeems, that air produced from cherries, promotes the alteration both of colour, and firmneſs in apricocks.

It appears alſo, that ſome part of ſuch air is deſtroyed at the firſt.

Plumbs in common air, in artificial air, and in vacuo.

(34.) *July* 30. 1676. I put plumbs, cut aſunder, into three receivers; one of which was full of artificial air, produced from gooſeberries; the ſecond full of common air, and the third exhauſted.

Aug. 2. In the artificial air, the plumbs were not changed; in the common air, they began to be mouldy; but in the evacuated receiver, they retain'd their colour, and were ſoft.

Aug. 5. In the artificial air, the plumbs had contracted a red colour, humidity, and ſoftneſs; in the common air, they ſeemed black and mouldy, yet retain'd their firmneſs; in the evacuated receiver, they were almoſt diſſolved.

Aug. 7. The plumbs in the common air, began to ſoften.

Aug. 8. The plumbs in the common air, ſeemed to have loſt their black colour, and to have contracted a red one; as it happen'd three days before, to the plumbs in the artificial air.

In this experiment, artificial air ſeems to have promoted an alteration.

Peaches in common and artificial air, mixed.

(35.) *Sept.* 24. I put five peaches into a receiver, with common air, mixed with ſome produced from grapes; and included the grapes themſelves in the ſame receiver, that the common air might be the better ſaturated with the artificial.

Sept. 25. The height of the mercury was twenty-one inches.

Sept. 26 } The height of it was { 23
27 } The height of it was { 31
28 } The height of it was { 39

Sept. 29 } The height of it was { 42
30 } The height of it was { 45
Octob. 1 } The height of it was { 48

Octob. 2. The ſame height continued.
3. The height of it was 52 and a half.
5. The height the ſame; but the peaches ſeem'd moiſt.
6. The height of it was 58.
7. The height of it was the ſame.
8. The height of it was 61.
11. The mercury aſcended a little.
19. The height of it was 65.
25. The height of it was 61. The cold was ſharp.
27. The cold abated, and the mercury aſcended.
30. The height was 61, and a little more.

Nov. 2. The height of the mercury was 59. It was ſevere cold weather.

6. The height was 61. The froſt broke, and it thaw'd.

7. The mercury ſeemed ſomewhat higher.

9. The mercury perſiſted at the ſame height.

Dec. 9. In one month's ſpace, the mercury aſcended, by degrees, to the height of eighty inches.

April 1. 1678. The mercury came to ninety-ſix inches above its wonted height. I now opened the receiver, and whilſt the air was breaking out, the peaches emitted many bubbles thro' their ſkins, not without a violent noiſe; and the ſkin, in ſome of them, was broken: they had preſerved their taſte, and the colour of their pulp; but loſt their firmneſs, as if they had been boiled; being left in the air for three hours, they were all rotten.

This experiment proves, that common air corrupts bodies, tho' much the leſs for being mixed with factitious air.

Pears in common air.

(36.) *Auguſt* 4. The firſt receiver. I cut five pears, each of them into four parts; and put one part of each into a receiver full of common air, and ſtopp'd it cloſe with a ſcrew.

Aug. 6. The colour of them was a little alter'd, and the mercury aſcended not at all.

Aug. 7. The pears were little alter'd, and the mercury was a little higher.

Aug. 8. The pears underwent no great change; the height of the mercury was four inches.

Aug. 9. The height of it was four and a half.

Aug. 10	The height of it was	6	*Aug.* 13	The height of it was	16
11		10	14		20

The pears began to be ſoft.

Aug. 15. The height of it was 21.

16. The height of it was 19. I believe the air had gone out.

17. Now I found the air had eſcaped.

18. The air being almoſt all got out, ſince yeſterday in the evening, and the fruit looking worſe, I took the pieces out, and found them putrefied.

Pears in artificial air.

Aug. 4. The ſecond Receiver. I took one quarter of each of the aforeſaid pears, and included them, after the ſame manner; and, afterwards, added air, produced out of cherries, till the mercury poſſeſs'd twenty-three inches extraordinary.

Aug. 6. The fruit was not alter'd, except a little in their colour.

Aug. 7. Almoſt all the pieces ſeem'd rotten; the mercury remaining at the ſame height.

Auguſt. 8. The pears were not alter'd much: but I could not ſee the mercury.

Aug. 10. They, gradually, grew ſofter; and the mercury was forty inches above its wonted height.

Aug. 11	The height of it was	51	*Aug.* 14	The height of it was	67
13		61	15		73

Aug. 16. The mercury deſcended; yet nothing had got out.

Aug. 17. The mercury exceeded not ſixty-ſeven inches in height; yet the air could by no means eſcape.

Aug. 18. The mercury remained at the ſame height; but ſuffering the air to break out, it had a ſharp odour; and the taſte of the fruit ſeemed very acid, and the pulp exceeding ſoft.

Pears in an unſtopp'd receiver.

Aug. 4. 1677. The third receiver. I put a quarter of each of the aforeſaid pears into a receiver, not exactly ſhut.

Aug. 6. The pears ſeem'd to change their colour.

Aug. 7. One of the pieces began to loſe its firmneſs; but, in the artificial air, another piece yeſterday ſeem'd wholly rotten.

Aug. 8. One piece was mouldy; the reſt were ſoft.

Aug. 9. The pears gradually grew more rotten.

Aug. 11. They were wholly mucid, and rotten.

This receiver, compared with the firſt, ſhews, that corruption begins not in the free air ſooner than in included air; but, when begun, that it is much more violent and ſudden; becauſe the included air may be ſatiated.

Pears in vacuo.

Aug. 4. 1677. The fourth receiver. I included one quarter of each of the ſaid pears *in vacuo.*

Aug. 6. The height of the mercury was 5.

Aug.	The height of it was	*Aug.*	The height of it was
7	8	13	20
8	10	14	23
9	12	15	25
10	14	17	28
11	16		

20. Hitherto the pears had undergone no alteration; but this day they began to be ſoft. The mercury aſcended not.

Aug. 26. Neither the pears, nor the height of the mercury, were at all alter'd.

This production of the air ſeems very regular.

Hence we find the aptneſs of artificial air to ſoften fruits.

And that the production of air was here promoted by artificial air, is very probable; tho' it had ſucceeded otherwiſe with apricocks.

Apricocks in common air.

(37.) *Auguſt* 21. 1677. The firſt receiver. I divided ſix apricocks, each into four parts; and put one piece of each into a receiver full of common air, and ſtopp'd it firmly with a ſcrew.

Aug. 22. The apricocks ſeem'd riper than yeſterday; but no air was produced by them.

Aug. 23. One piece, contiguous to the water, began to be mouldy, and the reſt inclined to putrefaction. The mercury ſeem'd to have aſcended a little.

Aug. 24. A piece next the water, was cover'd with much mouldineſs; another piece, more remote from the water, was ſomewhat mouldy alſo; but all were rotten.

Aug. 25. The fruit contracted no more mouldineſs; but the putrefaction increaſed. The height of the mercury was ſeven inches.

Aug. 26. The height of the mercury was 15.
28. The height of it was 30.
29. The ſame height continued.
30. The height of it was 33. The fruits were almoſt all diſſolved.
31. The height of the mercury was 38.
Septemb. 1. The height of it the ſame.
2. The ſame height ſtill.
3. The mercury aſcended a little.

Septemb. 4	The height of it was	41	*Septemb.* 7	The height of it was	45
5		43	8		46

Septemb. 9. The ſame height continued.

Sept. 22. Little or no change appear'd in the height of the mercury; but the fruit was almoſt diſſolv'd into water.

Octob. 1. When the mercury continued at the ſame height, and the fruit ſeem'd almoſt vaniſh'd, I open'd the receiver, and found the apricocks very much impaired and ſoft; yet they retained a taſte not unpleaſant, but tending to acid.

Apricocks in an open receiver.

Aug. 21. 1677. The ſecond receiver. I cover'd one quarter of each of the aforeſaid apricocks, with a receiver, not defended againſt the external air.

Aug. 22. They were flaccid, as if they had been dry, or wither'd.

Aug. 23. Many of them appear'd rotten and mouldy.

Aug. 24. The apricocks were wholly putrefied, and mouldy.

The ſame in one unexhauſted, with an addition of factitious air.

Aug. 21. The third receiver. I included firmly, by the help of a ſcrew, one quarter of each of the aforeſaid apricocks, in an unexhauſted receiver; to which I, afterwards, added air produced from pears, till it ſuſtain'd 20 inches of mercury.

Aug. 22. The mercury aſcended not at all; but the fruit ſeemed to have acquired a greater degree of maturity, than that included in common air.

Aug. 23. Theſe ſeemed leſs alter'd, than thoſe which were in common air.

Aug. 24. They remain'd unalter'd.

Aug. 25. The fruits began to produce air, but I could not diſcern the quantity.

Aug. 26. Little alteration in the fruit.

Aug. 28. It began to moiſten, yet was far leſs alter'd than that which remain'd in common air.

Aug. 30. The mercury emerg'd above the bodies, by which it was hid. Its height above the wonted ſtation, was thirty inches.

Aug. 31. The height of the mercury was forty inches.
Sept. 1. The height of it was the ſame.
2. The ſame height continues.
3. The height 45.
8. The height was little changed.
9. The height 40; yet no air got out.
11. The height was 38.

12. The mercury continued to deſcend.

13. The height of it was 33.

Sept. 14. The mercury was ſo depreſſed, as to appear no more.

Sept. 22. The mercury emerged again; its height was 33. The fruit was cover'd with a kind of mucor.

Octob. 1. When neither the apricocks, nor the height of the mercury, were any more alter'd, and the mucor vaniſhed, I open'd the receiver, and found the apricocks not impaired, but of a good colour, their pulp ſpungy and ſoft, and of a taſte inclining to acid.

Apricocks in an unexhauſted receiver, whoſe air was afterwards condenſed.

Aug. 21. The fourth receiver. I took a quarter of each of the ſaid apricocks, and ſhut them up firmly, with a ſcrew, in an unexhauſted receiver, into which, afterwards, I intruded air, till the mercury roſe 90 inches above its ſtandard height.

Aug. 22. Our receiver broke into an hundred pieces, by the force of the air compreſſed within it; whereupon I put the fruit into another, and added only ſuch a quantity of air as was able to ſuſtain ſixty inches of mercury.

Aug. 25. The apricocks had contracted much mouldineſs; and I added new air.

Aug. 26. They were wholly infected with mouldineſs and rottenneſs.

This receiver, if compared with the former, ſhews, that the quantity of corruption depends on the quantity of the air.

Hence we have it confirm'd, that alterations are made more ſuddenly in factitious air; and that in time, the corruption is far greater in common air.

That the effects of compreſſed air differ from thoſe of the common, ſhewn by onions in condenſed air.

(38.) *March* 21. 1677. I put two onions into a receiver, which was to be ſtopp'd cloſe with a ſcrew; and intruded ſo much common air thereinto, as raiſed the mercury ſixty inches above its uſual ſtation.

March 28. The onions took root as well as other onions which I included in common air at the ſame time.

April 28. The onions included in common air, eight days ago, were cover'd with mouldineſs, though, in the beginning, they had ſhot numerous roots: the onions in the other receiver began to corrupt at the ends of their roots; but the compreſs'd air, ten days before, had found a gradual paſſage out, and now was almoſt wholly eſcaped. I, therefore put in new air, till the mercury had attain'd to the height of ſixty inches above its uſual ſtandard.

April 29. The onions in the compreſs'd air, were cover'd all over with mouldineſs.

Hence it ſeems to follow, that a little compreſſure doth not prejudice bodies to be expanded by vegetation.

And the new air, which was intruded, ſeems to have promoted the mouldineſs, though probably, in the beginning, the compreſſure of the air retarded both the mouldineſs, and the corruption.

Tulips and lark-ſpurs in common and compreſſed air.

(39.) *May* 9. I put two equal quantities of tulips and lark-ſpurs, into two receivers of an equal bigneſs, and ſtopp'd them up firmly with ſcrews:

I

I left one of them with common air only, but compreſſed the other by the intruſion of new air, till the mercury exceeded its wonted height by ſeventy inches.

May 11. Two tulips in the common air, contracted mouldineſs; but all things remained unalter'd in the compreſſed air.

May 12. A third tulip, in common air, began to be finewed; but nothing like it happen'd in the compreſs'd air.

May 14. One tulip, in the compreſs'd air, was finew'd; but thoſe in the common air, were all very mucid; and one of the lark-ſpurs, in the common air, had alſo contracted a mucor.

May 17. Three of the tulips in compreſs'd air, had contracted a finew; but not half ſo much as thoſe in the common air. Two of the lark-ſpurs in the common air, appear'd finewed alſo; but thoſe ſhut up in compreſs'd air, were preſerved freſh, and wholly free from mouldineſs, or finew.

May 21. The flowers in the common air, were all rotten and putrified; but thoſe in the compreſſed air, received no further alteration: and the tulips, which had contracted ſome finew, ſeem'd rather to loſe it, than to acquire new.

May 30. When the flowers in the common air, being wholly putrified, were diſſolved into water, I took them out, and kept the liquor in the veſſel, to try whether any inſects would breed therein. In the compreſſed air, the flowers ſuffer'd no more ſenſible alteration; I therefore, took them out, and found them moiſt, and of an acid odour.

Hence it ſeems, that compreſſed air, hinders putrefaction and mouldineſs, in ſome plants.

Orange in compreſſed and common air.

(39.) *May* 21. 1677. I cut an orange into two equal parts, and incloſed one of the halves in a receiver, with air ſo compreſſed, that it would ſuſtain an hundred inches of mercury above its wonted height: I left the other half in another cloſe receiver, only with common air.

May 25. Each half of the orange had contracted mouldineſs; but that in the common air was much more mucid than the other.

May 26. The compreſſed air had entirely got out, and therefore I put in new.

May 30. I every day perceiv'd ſome air had eſcaped, and therefore, daily ſupplied freſh. And the orange, by receiving new air ſo often, contracted a mucor, notwithſtanding the compreſſure, much more than the other piece that was left in the ſame air without preſſure.

June 1. I took out the two half oranges; and that which lay in the compreſſed air, ſeemed to have contracted a corruption, at leaſt three times greater than that which had continued in the common air.

Hereby the diſpoſition of compreſſed air, to retard corruption, is confirmed; yet in time, 'tis very probable, that the quantity of corruption may depend upon the quantity of the air.

Roſes in common and compreſſed air.

(40.) *May* 31, 1677. I included two equal quantities of roſes, in two receivers, ſtopp'd by the help of ſcrews; into one of which I intruded as much air as would ſuſtain ninety inches of mercury, beſides its accuſtomed preſſure; but I left the other with common air only.

June 11. The roſes in the common air were free from mouldineſs, only they ſeemed to have loſt ſomething of their colour: but thoſe ſhut up in the compreſſed air, had almoſt all contracted a yellow colour, as if they had wither'd in the open air; yet they were not mucid. or finewed.

June 18. This laſt week, the flowers in the common air, ſuffer'd not the leaſt change; but thoſe in the compreſſed air, grew yellower. I open'd both receivers, and found the roſes to have retain'd their ſcent, yet it was ſomewhat alter'd, neither were they dry, or wither'd. I kept them apart in the open air, and found that thoſe taken from the compreſſed air, were not ſo ſoon alter'd by the contact of new air, as thoſe which had remained in uncompreſſed air.

Hence it ſeems to follow, that compreſſed air is ſometimes fitter to alter colour than common air. And perhaps, it may be worth our notice, that roſes ſo included, contract not a mouldineſs, but only a yellow colour; tho' in tulips and lark-ſpurs, it's otherwiſe.

Orange in compreſs'd air, and common.

(41.) *June* 1, 1677. I put the two halves of the ſame orange into two receivers; in the one I increaſed the quantity of air till it ſuſtained the mercury an hundred inches above its wonted height, but left the other uncompreſſed, only exactly ſhut.

June 6. Each half of the orange grew mouldy; eſpecially that, whoſe ambient air was compreſſed. But new air was every day ſupplied; for the compreſſed air in 24 hours time, had almoſt all got out. But in the former, it had remain'd very well ſhut in, for ſix whole days.

June 11. The orange in the common air, contracted no more mouldineſs; but in the compreſſed air, the mouldineſs gradually increaſed.

June 18. Finding the mouldineſs of the orange, in the common air, to diminiſh, rather than increaſe, I took it out; and perceiving further, that, in compreſs'd air, the orange was not more mucid, after I had ceaſed to intrude new air, I was willing to try whether the new air ſupplied new ſtrength to the orange, to exert and thruſt out its mouldineſs; and therefore, made the mercury in the gage, by means of the air intruded, to exceed its wonted height 80 inches.

June 20. Two days after I had intruded new air into the receiver, the mouldineſs of the orange appeared to be manifeſtly greater.

Hence we may gather, that the quantity of the mouldineſs depends on the quantity of the air.

Shrew-mice in common and compreſſed air.

(42.) *June* 17, 1677. I put two ſhrew-mice into two receivers, of equal bigneſs, and ſtopped them up carefully; in one of them I left only common air; into the other, I intruded air, till the mercury was higher, by 30 inches, than uſual: the mouſe in the common air, was included about 52 minutes paſt 5 of the clock; and 6 minutes after the other.

The mouſe in the compreſs'd air, ſeemed to loſe his ſtrength much ſooner than the other, the motion of his breaſt being leſs frequent: yet about 18 minutes after 6 o'clock, the mouſe in the common air, which ſeemed the ſtronger, fell into convulſive fits, and died; but that in the compreſſed air, ſeemed then, and ſome time after, to be as well, as he was an hour and half before.

About eleven of the clock, the mouſe in the compreſſed air, ſtill breathed; but about four in the morning, he was found dead, in the ſame poſture wherein he was ſeven hours before: whence we may conjecture, that he was free from convulſive fits.

I muſt not omit, that the mouſe, in the common air, had conſumed ſomething of that air; ſo that the mercury ſtood at 29 inches, and when the receiver was opened, preſently aſcended to 30.

Hence we learn, that compreſſed air ſeems fitter than the common, to prolong life; ſince the one mouſe lived ſo much longer, tho' only a double quantity of air was included in the receiver.

Flies in common and compreſſed air.

(43.) *June* 13. 1677. I put four flies into a receiver, and afterwards intruded air, till the mercury roſe ſixty inches above its wonted height; and at the ſame time, included three other flies, in another receiver, with common air not compreſſed.

June 14. In the morning all the flies were well. In the afternoon, I found two of them dead in the compreſs'd air; but in the common, they were all alive. About five o'clock, one of the flies, in the compreſs'd air was alive, and three in the common air.

June 15. This morning I found all the flies in common air dead; but that ſingle one which remain'd alive in the compreſs'd air, ſeem'd ſtill to be very well; and being taken out of the receiver, flew briſkly away.

Hence it ſeems, that flies are not very ſenſible of the air's compreſſure; and that they die more for hunger, than want of air: for the fly which remain'd ſo long well, fed upon the carcaſſes of thoſe which were dead; ſo that ſhe ſeem'd not to be diſtemper'd.

(44.) *June* 15. I repeated the preceding experiment, only including four flies in each receiver, and compreſſing the air ſomewhat more.

June 16. This morning I found two of the flies, in the common air, dead; and but one in the compreſs'd air.

About two in the afternoon, the four flies in the common air, ſeemed to be dead; but in the compreſs'd air, the three were alive.

Hence the compreſſure of the air ſeems of ſmall conſequence to flies; and indeed, they are not prejudiced by the rarifaction of it, without great difficulty, and unleſs there be almoſt a compleat vacuum.

Frogs in common and compreſſed air.

(45.) *June* 18. I included two frogs in two receivers, and ſtopped them by the help of ſcrews; the one only contain'd common air, the other, air compreſs'd, till it ſuſtain'd ſeventy inches of mercury.

June 19. Both the frogs were alive; and the height of the mercury, in both receivers, remained the ſame.

June 20. Neither of the frogs were dead; and they ſeemed rather to diminiſh, than increaſe the air: but the difference was ſo ſmall, that I dare not be poſitive therein.

June 21. In the morning, both the frogs were alive; but towards evening, that in the common air was found dead.

June 22. At evening the frog in the compreſs'd air, was alive.

June 23. In the morning I ſound it dead.

Oranges in common and compreſſed air.

(46.) *June* 18. 1677. I ſhut the two halves of the ſame orange, in two receivers, and ſtopped them by the help of ſcrews; the one with common air, the other, with air compreſs'd to ſuſtain ninety inches of mercury.

June 22. This morning I found the orange, in the common air, mouldy; but the other was ſound.

At three in the afternoon, the orange in the compreſs'd air, ſeemed alſo, to have contracted ſome mucor.

June 23. The orange in the common air was far more mucid than the other.

June 24. The orange in the common air, did not increaſe its mouldineſs; but the other was cover'd all over with it.

June 28. The mouldineſs produced in the common air, was now wholly vaniſhed; in the other receiver, I perceiv'd no further alteration in the fruit.

June 30. Both remaining in the ſame ſtate, I took them out. The part which was kept in common air, ſeemed half rotten; but the other, beſides its finew, appear'd wholly putrified.

Hence 'tis confirm'd, that the quantity of the mouldineſs depends on the quantity of the air.

It ſeems alſo worth obſerving, that the mouldineſs appear'd a little later in the compreſs'd air, that in the common, tho' afterwards, it increaſed much more.

Roſes in common and compreſſed air.

(47.) *June* 29. 1677. I included roſes in two receivers, ſtopp'd by the help of ſcrews; I left one with common air only, but filled the other with ſo much, that the mercury aſcended ninety inches above its uſual height.

July 14. Four or five days ago, I found the roſes, in the compreſs'd air, wither'd, and degenerated to a yellow colour. There was not the leaſt alteration in the other receiver.

July 17. When I perceived that this experiment proceeded after the ſame manner as that above-mention'd, I took out the roſes. Thoſe kept in the compreſs'd air, were very much corrupted, and of a very ungrateful ſmell; but the others were little alter'd, and their ſcent not unpleaſant.

Hence we have a further confirmation, that the quantity of corruption depends on the quantity of the air.

Lemmons in common and compreſſed air.

(48.) *July* 4. I cut a lemmon aſunder, and put the halves into two receivers, to be ſtopp'd by ſcrews; the one I left with common air only, but the other was fill'd with ſo much compreſs'd air, that it ſuſtain'd ninety inches of mercury above its uſual ſtandard.

July 7. This day, both parts of the lemon ſeem'd to grow mouldy at the ſame time.

July 17. That in the compreſſed air had contracted much more hoar than the other; and perceiving no farther alteration, I took them out, and found the lemon, in the compreſſed air, far more putrid than the other.

Hereby it is confirm'd, that the quantity of corruption depends on the quantity of the air.

It ſeems alſo, that a triple compreſſion of the air, in reſpect of a lemon, is too weak, ſenſibly to retard the production of a finew.

July-flowers in common and compreſſed air.

(49.) *July* 18. 1677. I included two parcels of July-flowers, equal in number, in two equal receivers, and ſtopt them cloſe with ſcrews; I fill'd the one with compreſſed air, till it ſuſtain'd an hundred inches of mercury extraordinary; but the other was left with common air alone.

July 23. In the compreſſed air, the July-flowers ſhew'd ſome hoarineſs; the others appear'd only moiſt; but the mercury exceeded its wonted height only ſeventy inches; for ſome of the air had got out.

July 25. In the compreſſed air, the July-flowers proceeded to corrupt much faſter than the others. They had wholly loſt their colour.

July 26. In the compreſſed air, the July-flowers were wholly putrefied, and cover'd with a hoary finew; the others were moiſt only in ſome places.

Auguſt 1. Perceiving no farther alteration, I took the flowers out of their receivers; thoſe which were kept in compreſs'd air were rotten, and fetid; the other kept their colour, and their ſmell was not offenſive; but they were moiſt.

And this is a farther confirmation, that the quantity of the air increaſes corruption.

We may alſo obſerve, that the mouldineſs is not produced, but in compreſs'd air; nor is it probable, that this happen'd by chance; ſince, in each receiver, there were three or four July-flowers included.

A ſhrew-mouſe confined in common air.

(50.) *July* 21. 1677. I included a ſhrew-mouſe in a receiver, with common air, and ſhut him in firmly with a ſcrew, to try whether he would produce, or conſume air.

After two hours, the mouſe died, and ſome air was conſumed; but a leſs quantity than in the former experiment of this kind.

July 24. Hitherto I found no change in the height of the mercury.

Towards evening, it ſeemed a little higher.

July 25. This morning much air was produced *de novo.*

July 26. The quantity of the produced air increaſed.

Hence we have a confirmation, that living animals conſume air; but dead ones produce new.

Pears in compreſs'd air.

(51.) *Auguſt* 31. I put pears into a receiver; whereto, after it was well ſtopt, I added as much air, as ſufficed to ſuſtain thirty inches of mercury, extraordinary.

September 1. The mercury was depreſs'd.

Sept. 2. The height of the mercury decreaſed; it exceeded not twenty-five inches.

Sept. 3. The mercury roſe one inch higher; and ſtaid at 26.

Sept. 4. The height thereof was 28.

Sept. 8. The receiver leaking, I put in new air; and, this day opening the receiver, to compare the taſte of this fruit with that of the other; I found, that five of the pears had loſt their firmneſs, but two retain'd it.

Pears in common air.

Auguſt 31. I included pears, of the ſame kind, in another receiver, with common air only, not compreſs'd.

September 1. The mercury was a little depreſs'd, as if it had been in compreſs'd air; the cauſe whereof might be only the cold.

Sept. 2. The mercury varied not.

Sept. 3. The height of the mercury was one inch above the uſual ſtandard.

Sept. 4	The height of it was	4	*Sept.* 6	The height of it was	6¼
5		6¾	7		12

Sept. 8. The height of the mercury was twenty. The pears being taken out of the receiver, had preſerved their taſte much better than thoſe included *in vacuo*. They alſo retain'd their firmneſs.

Pears in vacuo.

Auguſt 31. I included pears of the ſame ſort *in vacuo*; but ſome external air brake in, and the height of the mercury was one inch.

Sept. 1	The height of it was	4	*Sept.* 5	The height of it was	19
2		8	6		23
3		12	7		27
4		16	8		30

The pears being taken out, had kept their firmneſs, but loſt much of their taſte.

Hence it ſeems to follow, that, in a greater compreſſure, a leſs quantity of air is produced.

A ſmall bird in compreſſed air.

(52.) *December* 7. I ſhut up a ſmall bird in a receiver, capable of holding twenty ounces of water. The bird began to be ill, before I had ſet the ſcrew; but, after I had intruded ſo much air, as to ſuſtain thirty inches of mercury, above its wonted height, ſhe ſeemed to recover; but, ſoon after, began again to be ſick: and, therefore, I intruded air the ſecond time, till the mercury reſted forty-five inches above its uſual height; whereby the bird was again reſtored: but, in a little time, ſhe began to gaſp again; then, opening the receiver, after ſhe had ſtaid in it twenty-eight minutes, ſhe flew out, and was very well.

A ſhrew-mouſe in compreſſed air.

(53.) *January* 20. 1678. I put a ſhrew-mouſe into the receiver of the wind-gun, above deſcribed; and, immediately, ſo far condens'd the air, till it was reduced to about the twentieth part of its ſpace: then I preſently diſcharged that air, and the elliptic hole being open'd, I ſuſpected that the mouſe had been only a little convulſed; but, when he was taken out, there were no ſigns of life in him. Whether the cauſe of his death were to be aſcribed to the narrowneſs of the receiver, or to the compreſſure of the air, is a queſtion.

I put another mouſe into the ſame receiver; and the air being reduced to a third or fourth part of its natural ſpace, I opened the receiver, but not ſo carefully as in the former experiment; yet the mouſe, taken out therefrom, was found to be very well.

I afterwards repeated the experiment; the air being about ſeven or eight times condens'd; and the mouſe ſeem'd to ſuffer no inconvenience thereby.

I made the ſame experiment again, in air compreſſed ſeven times, and left the mouſe included for twenty-four minutes; which time being elapſed, I diſcharged the air, and opening the hole, perceiv'd the mouſe to fetch many deep groans, as it were: I took him out, but he could not recover.

Hence it is manifeſt, that a great compreſſion of air is pernicious and deſtructive to animals.

A ſhrew-mouſe in condenſed air.

(54.) *January* 28. 1678. I put a ſhrew-mouſe into a glaſs, to whoſe neck we tied a bladder, that ſtopp'd the orifice. Theſe were put into a receiver for compreſſing of the air. Soon after, when the mouſe began to be ſick, I compreſſed the air, and the bladder was ſtraitned; ſo that the mouſe was in condenſed air, whilſt no new air could paſs to him: then he ſeem'd to be much better, and his heart beat leſs frequent; when, opening the receiver, he was, in a ſhort time, as well as ever.

I repeated the experiment, and left the mouſe ſo long, that he could hardly breathe, whilſt I began to compreſs the air; and the compreſſure ſeem'd again to abate his reſpiration; the receiver being open'd, and the mouſe expoſed to the air, he could not breathe much more freely; but if I blew the air on him with bellows, he ſeem'd to be ſomething reliev'd. Being again committed to the compreſs'd air, he breath'd leſs frequently, and at laſt died.

March 25. Becauſe, in the preceding experiment, it was not clear, whether the air enter'd thro' the ligature of the bladder, I uſed the inſtrument deſcribed, *Fig.* 79. And when I perceiv'd the mouſe was ſick, and breathed ſeldom, I intruded water into the receiver, ſo that the air was reduced to half of its ſpace; and then, the mouſe breath'd more rarely. But if, extracting the water, I left the whole ſpace entire for the air, his reſpiration ſeem'd more vivid; and the air being thus, many times, contracted and dilated, the ſick mouſe ſeem'd to reſpire more freely in the common air than in the compreſſed. Whence, I conjectur'd, that air is to animals, like food; the quantity whereof ought to bear ſome proportion to their ſtrength. And, that I might more certainly know this, I put the ſame mouſe into my pneumatic engine, and rarify'd the air, ſo that it poſſeſs'd more than double the uſual ſpace. Whilſt the air was rarifying, the mouſe preſently began to be better; yet, a little while after, he ſeem'd ſick: and when the air was reſtor'd, it no wiſe affected him. I thus repeated the rarifaction three times, with the ſame ſucceſs; but, at laſt, the mouſe died.

The effects of artificial air upon animals; and, firſt, upon a bee included with diſtilled vinegar, and powder'd coral.

(55.) *May* 5. 1677. I put a bee, with diſtill'd vinegar, and pulveriz'd coral, into a receiver; and the air being wholly exhauſted, I order'd the matter ſo, that the coral fell into the glaſs of vinegar: but the air, produced from thence, did not reſtore any power of motion to the bee; but, when ſhe was expoſed to the open air, ſhe ſoon began to move herſelf.

Hence a ſuſpicion ariſes, that artificial air is unfit to preſerve the life of animals.

Flies in artificial air of gooſberries.

(56.) *Auguſt* 12. 1676. I put two flies into a receiver, and, exhauſting the common air, ſubſtituted, in its ſtead, air produced from gooſberries, till it ſuſtained twenty-ſix inches of mercury.

Afterwards, I put two other flies, alſo, *in vacuo*; but with this difference, that I let ſo much common air into them, as could ſuſtain twenty-three inches of mercury.

Within a quarter of an hour, the latter flies, upon the reſtitution of the air, recovered that power of motion, which they had loſt *in vacuo*, and flew about in the rarified air; but the former lay without any motion, tho' they had received a greater quantity of air.

Aug. 13. The flies, in the artificial air, ſeem'd ſtill dead; but the others were luſty.

The flies, taken out of the artificial air, and expoſed to the common air, remain'd ſo, all this whole day, without recovering life.

Aug. 18. I repeated the experiment, with the ſame ſucceſs; tho' I had reſtored a greater quantity of artificial air.

Hence we have an high confirmation, that artificial air is noxious to animals.

Flies included with fire in the artificial air of paſte.

(57.) *June* 22. 1677. I put paſte into three receivers, out of which, I, afterwards, exhauſted the air.

June 23. When the three receivers did, this day, regurgitate with air produced from the paſte, I kindled a perfumed cone, and put it into one of the receivers; which being preſently ſtopt, the fire, within one minute, went out: then, by blowing, I expell'd the artificial air from the receiver, and put fire into it, as before, and it burn'd bright for a conſiderable time; tho' I had ſhut the receiver as ſpeedily, and as accurately, as before.

I made another experiment, after the ſame manner, with a fly; and, in the artificial air, ſhe preſently ſeem'd to be dead; but, afterwards, being expoſed to the ſun, ſhe, in a ſhort time, grew well again. Then I blow'd common air into the receiver; but the fly, included as before, ſuffer'd no inconvenience thereby.

I repeated the ſame experiment, with the ſame fly, in the third receiver, fill'd with artificial air, with the ſame ſucceſs; only this fly, when taken from the artificial air, could not be reſtored, without longer time, becauſe ſhe was left longer therein.

Hence it appears, that factitious air is prejudicial to fire, as well as to the life of animals.

Flies and frogs in artificial air.

(58.) *June* 25. 1677. I put paſte into four receivers; and totally exhauſting the air from two of them, I pump'd out only half the air from the other two.

June 26. I found the two receivers, which I had left half full of common air, to be quite fill'd with air newly produced; but know not whether they had, for ſome time, regurgitated or no, ſo that the quantity of common air was much diminiſh'd. However, I put two flies at once into one of the receivers, after the manner before deſcribed; and ſoon after they touch'd the bottom of the receiver, they remain'd without motion. I put a third fly into the receiver, after the ſame manner, and found ſhe liv'd a little longer there than the former. A fourth fly being put in, maintain'd her life longeſt of all; yet at laſt ſuffering ſome convulſions, ſhe lay moveleſs on her back. All the flies, after ſome ſtay in the artificial air, being taken out, and expoſed to the common air, grew well in a ſhort time.

I made the ſame experiments in another receiver, half full of artificial air, and in a manner with the ſame ſucceſs; but the flies in that receiver, into which only common air was admitted, ſoon recovered their ſtrength and motion.

June 27. I found one of the receivers, which was wholly evacuated of common air, to be full of artificial air; but it being caſually thrown down upon the ground, entrance was thereby given to the external air; yet I put a frog into it, which ſeemed not to be very ſick therein.

June 30. The fourth receiver, by the power of the produced air, ſeem'd at length forc'd away from its cover. I put a frog into it, and ſhe fell into high convulſions for 5 minutes ſpace; and then lay without motion. In 4 minutes after I opened the receiver, and taking out the frog, ſhe remain'd for 46 minutes without motion; but afterwards, in 4 or 5 minutes more, ſhe grew very well.

Hence it is evident, that artificial air is very hurtful to the life of animals; but that, if mixed with common air, it doth not ſo readily produce its effects.

(59.) *June* 28. 1677. I put paſte into four receivers, three of which I cauſed to be wholly exhauſted of common air; but the fourth was left half full of air.

June 29. One of the receivers that were wholly exhauſted, was found full of air, newly produced; and a frog being put into it for 4 or 5 minutes, had ſtrong convulſions; then for one minute ſhe lay without motion; whereupon I took her out, and in 5 minutes ſhe began to move, and a while after became well again.

I took another receiver, fill'd with artificial air, and putting a frog into it, in 7 minutes ſhe ceaſed to be convulſive. And afterwards, when ſhe had lain one minute there without motion, I opened the receiver, and taking her out, found that ſhe began to ſtruggle and move; tho' I judg'd thoſe motions to be the remains of her convulſions; for after that, ſhe continued moveleſs for half an hour and more; yet at laſt ſhe grew well again.

The receiver, from which I exhauſted only half its air, had ſo long regurgitated with produced air, that, very probably, much common air had got out together with it. A frog being put into it, ſeem'd to be vehemently moved and convulſed for 10 minutes, as the reſt did, and then ſhe ſeemed quite dead; but after a full minute I opened the receiver, when the frog, being expoſed to the open air, within a quarter of an hour began to move.

I put a frog into a receiver, full of common air, to try whether, the paſte being now taken out, ſhe would live any longer there.

July 1. In the afternoon I found her dead, tho' ſhe breathed in the morning; ſo that ſhe liv'd about 48 hours.

June 30. I put a frog into the fourth receiver, which was wholly fill'd with artificial air; for 7 minutes and an half ſhe was vehemently convulſive, and at laſt died; then, after two minutes, ſhe was taken out, but recovered no motion at all.

July 1. Perceiving the frog to remain in the ſame poſture, I threw her away.

Hence we have a confirmation, that artificial air is the more hurtful to animals, the freer it is from common air.

A ſhrew-mouſe, ſnail, and flies, in artificial air of paſte.

(60.) *June* 30. I included paſte in two receivers, and then exhauſted the air.

July 4. I put a ſhrew-mouſe into one of the receivers, filled with artificial air, where he ſuffered vehement convulſions, and in one minute died. I preſently took him out, and expoſed him to the common air; but no power of motion could be thereby recovered.

Then I took the other receiver, and putting a ſnail into it, with ſome wonder obſerved, that he continued to move very ſtrongly for a whole quarter of an hour; but afterwards his motion was ſlower, till in about another quarter of an hour he lay ſtill, as if he were dead; but then being taken out of the receiver, and expoſed to the air, he ſoon grew well.

I put flies into the ſame receiver; but now it had admitted too great a quantity of external air, for the flies received no hurt.

Hence we gather, that artificial air kills animals by ſome venomous quality, and not only by the defect of common air; for the ſnails liv'd longer *in vacuo*.

A frog in air produced from cherries.

(61.) *July* 5. 1677. I took a receiver filled with air produced from cherries, and tranſmitted it out of that into another receiver, full of common air, in which a frog was kept; matters were ſo order'd, that the water gave place only to the artificial air entring in; and the water itſelf flowed out. And thus the frog, being included in pure artificial air for a quarter of an hour and more, ſuffered convulſions, and at laſt lay ſtill, without motion; yet being afterwards taken out, and expoſed to the open air, ſhe grew quickly well.

Hence it ſeems probable, that air produced from cherries, is leſs hurtful to frogs than that produced from paſte.

Flies, and a ſhrew-mouſe, in artificial air of gooſeberries.

(62.) *July* 9. 1677. I put gooſeberries into three exhauſted receivers.

July 20. I found one of them ſevered from its cover, by the force of the produced air; I caſt a fly into it, which died inſtantly; a ſecond fly being likewiſe caſt into the receiver, preſently died; a third put into the ſame, ſeemed for a little while to be convulſive, but leſs than a fourth fly that I included therein; which yet in one quarter of a minute lay moveleſs. Afterwards I diſpelled the artificial air out of the receiver, by blowing, and in a little time the flies grew well.

July 24. I took another receiver, filled with air produced from gooſeberries; and putting a ſhrew-mouſe into it, found that he died there in half a minute.

Probably therefore the air produced from fruit, is leſs hurtful to animals than that produced from minerals; for on the 20th day of *July* I found that a mouſe lived not above a quarter of a minute in air produced from gun-powder.

A ſhrew-mouſe, a bird, and an adder, in artificial air of paſte.

(63.) *July* 5. 1677. I included paſte in four exhauſted receivers.

July 6. One of them being filled with factitious air, was forced from its cover, which I again ſtopped; yet not ſo ſoon, but ſome common air might mix with the artificial; I put a ſhrew-mouſe into it, which was preſently highly convulſed, and after one minute and an half remained moveleſs; and being preſently taken out, he ſeemed to have ſome convulſive motions, and died.

July 7. I took a ſecond receiver, filled with artificial air; and having included a little bird therein, ſuddenly ſtopt it; ſhe preſently fell into convulſions, and within about a quarter of a minute died; I took her out, but it was too late, for ſhe never ſtirred after.

I blew out the artificial air from the receiver, and then another bird of the ſame kind being put in, was very well, tho' ſhe ſtaid there four minutes.

July 9. I took a third receiver full of artificial air, and put that bird into it, which in the laſt experiment had continued well, and yet ſeemed to be lively and ſound; before ſhe had been there a full quarter of a minute, ſhe lay without motion; and being preſently taken out, there appeared no ſign of life in her.

In the afternoon I put an adder into my fourth receiver, and within two minutes he began to be ſick, to gape and pant; yet he was not wholly deprived of motion till after 24 minutes. Then in 6 minutes more I took him out of the receiver, motionleſs as he was, and expoſed him to the free air, but he did not recover.

July 10. The adder remained in the ſame ſtate, and gave no ſigns of recovery.

A bird and a ſhrew-mouſe, in artificial air of raiſins.

(64.) *July* 12. 1678. I put a bird into a receiver full of air produced from raiſins of the ſun; ſhe died in a quarter of a minute; and tho' I took her out preſently, yet ſhe never ſtirred more.

July 18. I likewiſe put a ſhrew-mouſe into a receiver full of air, produced from raiſins of the ſun; but a thread, left on the edge of the receiver, hinder'd me from ſtopping it cloſe; yet the mouſe preſently began to be very ill, and after two minutes he lay, as it were, without any motion; yet being taken out, he was well again in 2 or 3 minutes time.

Shrew-mice included in common air.

(65.) *October* 1. 1678. About 10 in the morning I included a ſhrew-mouſe with common air in a receiver, fortified againſt the external air; about 11 the mouſe could hardly breathe; I threw another ſtrong luſty mouſe into the ſame receiver, and preſently put on the cover; but the firſt mouſe having conſumed ſome of the air, the external air was forcibly impelled into the receiver, and ſo diſpelled a great part of the air ſtagnant there; upon which the firſt mouſe ſeemed to be much better; neither did he die much ſooner than the other, but both of them died about noon. About 4 in the afternoon I put another ſtrong mouſe into the ſame receiver, and leſt the external air might again expel the included air, I put him in very leiſurely; this third mouſe lived not 3 minutes entire.

Whence we may conjecture, that the portion of air which hath once ſerved for the reſpiration of animals as much as it could, is no longer uſeful for the reſpiration of another animal, at leaſt of the ſame kind.

Snails in factitious air of paſte.

(66.) *April* 28. In the morning I put ſo great a quantity of paſte into an exhauſted receiver, that in the afternoon I found the receiver full of factitious air; whereupon I put a ſnail into it, which preſently frothed very much, and often expanded and contracted itſelf; but in four minutes he ceaſed to move; yet I took him not out till he had ſtaid in the receiver for a quarter of an hour; and then being releas'd, he ſeem'd as if quite dead; for tho' he were prick'd with a pin, yet he diſcovered no ſign of life; tho' after another quarter of an hour, being prick'd in the ſame manner, he moved a little.

I blew out the factitious air, and put in another ſnail; he remained very well in the receiver, and did not froth at all.

Hence we have a confirmation, that factitious air is a greater enemy to animals, than a *vacuum*.

Snails in the factitious air of peaſe.

(67.) *June* 22. 1678. In the morning I put green peaſe into an exhauſted receiver, and towards evening the mercury had almoſt attain'd the height of 10 inches.

June 23. The height of the mercury was almoſt 30 inches.

June 24. The mercury did not yet exceed 30 inches in height. The cover no longer ſtuck to the receiver, yet nothing hitherto had eſcap'd.

June 26. I included the ſame peaſe in the ſame evacuated receiver.

July 29. When I now found that the receiver was fill'd with factitious air, I thruſt a ſnail into it, which froth'd much, and very often ſhot out and contracted his horns; but in 6 minutes time he lay ſtill, as if he had been dead, and continu'd thus for 2 or 3 minutes; then the receiver being opened, and the ſnail taken out, moved himſelf a little if he were prick'd. Whence it ſeems to follow, that air produced from peaſe is leſs prejudicial to ſnails, than air from paſte. I blew new air into the receiver, and a ſnail then put into it, did very well.

In this experiment it ſeems obſervable, that peaſe quickly produce air *in vacuo*; but, that in the uſual compreſſure of air, they generate little.

Animals in vacuo, *and firſt, a butter-fly.*

(68.) *June* 22. 1676. I put a butter-fly into an exhauſted receiver, and it was almoſt three hours before he was wholly deprived of motion; at length, perceiving him to lie unmov'd, I let the air into the receiver, and preſently the butter-fly moved. Then I bound him by one of his horns, with a thread, and ſuſpended him in the receiver; when, he was carried very freely from one part of it to the other, by the motion of his wings; but, after the air was extracted, the clapping of his wings was in vain, for he could not, in the leaſt, move the thread from its perpendicular poſture.

Flies in a receiver partially evacuated.

(69.) *July* 12. 1676. Yeſterday I put two flies into a receiver, in which I left $\frac{1}{4}$ of air, *(i. e.)* as much as would ſuſtain ten inches of mercury; the biggeſt of the flies ſeem'd to die preſently; but the other, which was a ſmall-bodied one, lived almoſt twenty-four hours.

When both the flies lay, as if they were dead, I ſuffer'd ſome air to enter in; the mercury was fifteen inches high, when the leſſer fly began to move her feet, but the other continued ſtill without motion.

Hence it appears, that air, highly rarified, may ſerve for inſects to breathe in, and that it doth not kill them ſo ſoon as artificial air.

Snails in vacuo.

(70.) *May* 1. I put two ſnails into an exhauſted receiver, and, for a whole hour, they ſeemed to be well enough, and crept up to the top of the veſſel; but, in two hours time, they fell down from thence, and lay without motion.

After they had remain'd *in vacuo* for ſix hours, I took them out, and within half an hour, they began to move a little. During the time they were included, they produced near as much air as ſuſtain'd the mercury at the height of a quarter of an inch.

Theſe ſnails liv'd longer *in vacuo*, than did others included in artificial air.

Flies eggs in vacuo.

(71.) *Auguſt* 12. 1676. I put the eggs of flies into an exhauſted receiver, to try if they would there produce worms.

Aug. 14. Worms were formed, but the air had crept into the receiver, ſo as to ſuſtain fifteen inches of mercury.

Hence it appears, that inſects may be produced, and live, if not *in vacuo*, yet, at leaſt, in air very highly rarified.

Auguſt 16. 1677. I put flies eggs into an exhauſted receiver.

No worms being produced, I admitted the air into the receiver, and left all things in the ſame poſture, to try whether the eggs had loſt their faculty of producing worms.

Sept. 9. The eggs produced nothing.

This experiment ſeems to ſhew, that inſects may be generated, and live in air highly rarified; but not at all *in vacuo*.

(72.) *March* 17. 1677. I put two equal quantities of frog-ſpawn into two glaſs veſſels of equal bigneſs; I left the one in an exhauſted receiver, expoſed to the ſun; but the other, being in a receiver full of common air,

Frog-ſpawn included in vacuo, *and in common air.*

I fortified againſt the acceſs of the external air. The frog-ſpawn *in vacuo*, all ſwell'd into bubbles.

May 2. No frogs were produced in either receiver; and the ſpawn, kept *in vacuo*, remain'd ſtill full of bubbles: but, about three days ago, all the bubbles vaniſh'd, and the ſpawn was changed to a green liquor.

July 2. The receivers remained in a window, expoſed to the noon-day ſun; ſo that ſome water mix'd with frog-ſpawn *in vacuo*, and the very ſpawn itſelf was elevated into vapours; and afterwards, ſticking to the ſides of the receiver, out of its own veſſel, was there condenſed: but the veſſel, kept in the common air, ſtill contained all its water, together with the ſpawn.

A frog in vacuo.

(73.) *June* 15. I ſhut a frog in an exhauſted receiver, at about ſeven of the clock in the evening, and about nine the frog died.

June 16. I repeated the experiment, and again perceiv'd, that the dead frog, in two hours ſpace, had produced ſome air, rather than conſumed it.

June 18. The frog, hitherto left *in vacuo*, was ſwollen very much; but the air, now entring, made her far more flaccid and lank than before.

Hence it appears, that a receiver, void of artificial air, is leſs hurtful to ſuch kind of animals.

Fly-blowings in vacuo.

(74.) *Auguſt* 3. 1678. I put fly-blowings upon fleſh, into an exhauſted receiver.

Aug. 12. No worms were generated.

Aug. 15. Perceiving no change in the eggs, I open'd the receiver, to try whether they would generate in the free air.

Sept. 15. Nothing was produced from them.

Hence we ſee, that animals, which may be generated, and live in highly rarified air, are killed *in vacuo*.

Vinegar-eels in vacuo.

(75.) *Aug.* 22. 1678. I included vinegar full of ſmall eels, or vinegar-worms, in an exhauſted receiver.

Aug. 29. The worms ſtill moved, but were fewer than at firſt.

September 6. Yeſterday ſome of the worms ſtill moved, but now I could not ſee one; and uſing a microſcope, I found them all dead: but, in the vinegar, which I had left in the open air, the eels had as briſk a motion as at the beginning.

Hence it appears, that very minute animals are alſo affected by the preſence and abſence of the air.

Fire in compreſſed air, and firſt perfumed cones, included, and fired, in condenſed and common air.

(76.) *May* 14. I took a perfumed cone, which being once kindled in the free air, will, by degrees, wholly conſume; and put it into a receiver firmly ſtopt with a ſcrew: then I intruded air into it, till the mercury roſe one hundred and twenty inches above its wonted height; when, by a burning-glaſs, I kindled the cone, which preſently darken'd all its receiver with ſmoke, and, after ſome time, ſeven eighths of an inch thereof, in length, were reduced to aſhes; but taking out the cone, and blowing away the aſhes, I found only the ſuperficies thereof conſumed; the inner parts remaining entire.

I included another cone of the ſame ſort in a much greater receiver, but did not compreſs the air therein: the cone, fir'd by the ſame burning-glaſs, was not taken out, till all the fumes abated and fell down; yet, much leſs of this cone was burnt, than of the former.

(77.) May 19. I weighed a perfumed cone exactly, and then firmly included it in a receiver with common air, and kindled it by means of a burning-glaſs; when the fumes were condenſed, I took the cone out of the receiver, and found, it had loſt of its weight, almoſt one grain.

Afterwards, the ſame cone, obſerving the ſame circumſtances, was again included and kindled; but firſt I had intruded as much air into its receiver as ſuſtain'd ninety inches of mercury; and by means of a pair of ſcales, found the loſs of weight, now, to be four times more than before.

Hence it ſeems to follow, that the conſumption of matter by fire is greater in proportion to the quantity of air contained in the receiver.

(78.) *May* 17. 1677. I included a perfumed cone in a receiver firmly ſtopt by the help of a ſcrew; and the air compreſſed to ſuſtain ſixty inches of mercury above its uſual height, I fir'd it with a burning-glaſs; the cone being afterwards taken out, had loſt three grains and an half in weight.

I repeated the ſame experiment in air, ſo compreſſed, that the mercury reached one hundred and twenty inches higher than uſual; then the cone was ſeven grains and three quarters lighter; and ſo, tho' the quantity of the air was not double, yet the conſumption of the matter by fire, was more than twice as much as in the former experiment.

May 17. I made the ſame experiment in air compreſſed, to ſuſtain ninety-ſeven inches of mercury; and then, the loſs of weight ſeemed to be ſix grains.

Hereby we are taught, that the matter is the more conſumed by fire, as the compreſſure of the air in the receiver is the greater; or rather, that the conſumption is made in a proportion greater than that of the compreſſure.

May 18. 1677. I intruded a perfumed cone, as before, in a receiver ſeven times larger than that uſed in the former experiments, and crowded no air at all into it. The cone kindled there, loſt three grains and a quarter of its weight; whereas, in the ſame quantity of air, if reduced to a fifth part of its ſpace, it would have loſt ten grains.

Hence it ſeems to follow, that the ſame quantity of air, reduced to leſs than its accuſtom'd ſpace, causeth a greater conſumption, than in its natural ſtate.

(79.) May 19. 1677. I repeated the laſt experiment, in the ſame receiver, cloſely ſtopt with a ſcrew, that nothing might get out or in. The cone loſt but one grain and a quarter of its weight; whence I ſuſpect, that it was not well kindled.

May 21. I made the ſame experiment in the ſame manner; and this day the cone was lighter by four grains; whence I more certainly collected, that it was not well ſet on fire in the preceding experiment.

May 23. I repeated the ſame experiment twice, but ſuſpect, that the cone was not well kindled; ſince, at one time, it loſt, only three quarters, and at another, one grain of its weight.

May 24. I made the ſame experiment again; and this day alſo the loſs of weight was found only one grain and a quarter. Then I open'd the receiver, and having wiped and cleans'd away the ſoot, repeated the experiment; when the cone took fire very well; for the loſs of its weight amounted to ſix grains and an half.

I tried the ſame experiment again, in an uncleanſed receiver, and the cone loſt only three grains.

May 25. I made the ſame experiment in a receiver well waſh'd, and the cone was lighter by ſix grains and an half.

I made the ſame experiment, in the like manner, in a well cleanſed receiver, and the cone loſt ſeven grains and an half.

I made the ſame experiment again, in an unwaſh'd receiver, and then I could not ſufficiently kindle the cone.

May 26. I made the ſame experiment in an unwaſh'd receiver, about the middle of the day; the ſun being clear, and bright; and remov'd not the burning-glaſs for a long time, ſo that it took fire very well, and became eight grains lighter.

Hence it is manifeſt, that the quantity of a cone to be conſumed in the ſame quantity of air is not fix'd and certain, but ſometimes greater, ſometimes leſs, as the cone ſhall be more or leſs kindled. Beſides, the imperfect mixture of the matter may cauſe ſome difference; yet it ſeems certain, that fire is more eaſily kindled in compreſſed air, than in common; and the conſumption will be the greater in a certain quantity of air, if that air be reduced into a narrower ſpace, than if it poſſeſs'd no more than uſual.

(80.) *May* 22. I put a perfum'd cone into a receiver made for compreſſing the air; and intruding the air till the mercury reſted thirty inches above its uſual height, I kindled the cone, and found its weight to be abated one grain and three quarters.

May 23. I made the ſame experiment again, after the ſame manner, and with the ſame ſucceſs.

I repeated the ſame experiment, but the cone did not kindle well. Whence we have a confirmation, that fire is more eaſily kindled in air much compreſſed, than in common air, or that which is but a little condenſed.

I repeated the ſame experiment, and after I had removed the burning-glaſs, whilſt I was intent to ſee, whether the cone would proceed to be conſumed, the receiver brake into an hundred pieces, ſome of which ſtruck my head, and wounded it: which I mention, that no man may be confident his glaſs will not break, whilſt he is about ſuch experiments, becauſe he has found, that at other times it reſiſted a greater preſſure. For this very glaſs had contain'd air four times more compreſs'd. It had alſo reſiſted air, ſuſtaining one hundred and ninety-eight inches of mercury above its wonted height; yet, now it was broken by a preſſure, more than ſix times leſs; and therefore while a man looks into ſuch receivers, his head ſhould be guarded.

(81.) *June* 4. 1676. I burnt paper, beſmeared with ſulphur *in vacuo*, and found that it produced ſome air; which was not at all diminiſh'd for two days.

This air muſt be aſcribed to the paper, for none is produced out of ſulphur alone.

Fire made uſe of to produce air, and firſt paper, beſprinkled with ſulphur burnt in vacuo.

(82.) *June* 15. I burnt harts-horn *in vacuo*, and found that the fumes iſſuing therefrom contained ſome air.

Harts-horn burnt in vacuo.

June 17. On theſe two laſt days I repeated the ſame experiment, and always obſerved, that air produced from harts-horn, was ſoon, in part, deſtroyed; but that which preſerved its elaſticity for an hour after the burning-glaſs was removed, ſeemed afterwards not to loſe it at all.

June 19. I took the harts-horn out of the receiver, and found no volatile ſalt, but only a fetid oil to be produced therefrom.

(83.) *June* 21. I burnt amber *in vacuo*, and at firſt could not find that the fumes aſcended above the height of one inch; yet in a receiver full of air they would be carried up to the top, and from thence return downwards; yet afterwards, even in the *vacuum* itſelf, the fumes reached almoſt to the top of the receiver, but the mercury varied not at all in its gage.

Amber burnt in vacuo.

June 22. This night a great deal of that water, in which I had immerſed the receiver, found a paſſage into it, tho' the cover was ſo well fitted to the aperture, that I never perceived any water to get in betwixt them before. Hence a ſuſpicion aroſe, that ſome volatile ſalt had probably attracted the aqueous parts, by reaſon of the congruity betwixt them.

July 8. I ſtill kept the receiver immerged in water, but no more water enter'd in; as if, the ſalts being waſhed away, the external water, deſtitute of aſſiſtance, could no longer inſinuate.

(84.) *Jan.* 18. 1677. I put two drams of camphire into an exhauſted receiver; and the juncture of the cover with the receiver, being fortified againſt external air, I put the camphire on a digeſting furnace.

Jan. 19. The camphire ſublimed into flowers, but no air was produced.

Camphire ſublimed in vacuo.

(85.) *May* 24. 1676. I included *Sulphur vivum* in an exhauſted receiver, and melted it by the help of a burning-glaſs; but found that the fumes produced therefrom contained no air, becauſe the mercury aſcended to the aperture of its gage, as is uſual while the receiver is evacuating; yet when that was cool'd, the mercury returned to its former height; and therefore that change was probably owing to the air included in the ſeal'd leg of the gage, being rarified, and driving the mercury into the other part.

Sulphur vivum *fuſed* in vacuo.

(86.) *July* 19. Having included paſte nine days ago *in vacuo*, and perceiving that it now contained no more air, I endeavoured to fire it with a burning-glaſs. The ſubſiding fumes had tinged the ſuperficies of the paſte of a curious yellow; and I conjectur'd that ſome air was produced, becauſe the receiver, which before was cloſely join'd to its cover, might now with eaſe be pluck'd therefrom.

Paſte expoſed to the rays of a burning-glaſs in vacuo.

The production of air from grapes in vacuo.

(87.) *September* 9. 1676. I exhauſted the air out of a receiver half full of dry'd grapes, and fortified it againſt the external air.

Sept. 10. In 24 hours time the height of the mercury was half.

Sept. 12. In two days time the aſcent of it was half.

	The aſcent of the mercury was
14.	$\frac{3}{8}$
17.	$\frac{3}{8}$
22.	$\frac{5}{8}$

27. The aſcent was $\frac{5}{8}$. The height 3 inches.

October 11. The height of the mercury was now about 6 inches.

From figs.

September 9. 1676. I put dry'd figs into a receiver, and fill'd about half of it with them; then I extracted the air, till the mercury reſted at the height of 3 inches.

Sept. 10. No air was produced.

Sept. 17. Perceiving no air to iſſue out of the figs, I open'd the receiver.

Hence we learn, that dry'd fruits put into an exhauſted receiver, produce very little air with regularity.

From pears and apricocks.

(88.) *Auguſt* 5. 1676. I included pears and apricocks *in vacuo.*

Aug. 6. In 18 hours time the mercury roſe two inches; in 10 hours more it reach'd to three.

	The height of it was
Aug. 7.	5
8.	6 $\frac{1}{2}$

9. In 14 hours ſpace the mercury mounted 3 quarters. Its height was 7 and a quarter.

	The height of it was		The height of it was
Aug. 10	8 $\frac{3}{4}$	*Aug.* 18	25
11	10 $\frac{3}{4}$	19	29
12	12 $\frac{1}{4}$	20	31 $\frac{1}{2}$
13	14 $\frac{1}{4}$	21	32 $\frac{1}{2}$
14	16	22	34
15	18	23	35
16	20	26	38 $\frac{1}{2}$

Aug. 29. The height of the mercury was 41.

Sept. 1. The height of it was 42 and a half.

4. The height of it was 44.

7. The 3 laſt days being hotter than the foregoing, the aſcent of the mercury was 2 and a quarter; its height 46 and a quarter.

Sept. 10. The height of the mercury was 47 and a half.

13. The mercury was depreſs'd, its height only 44 inches.

23. The mercury by degrees mounted again to 48 inches.

27. The height of the mercury was 50 and a half.

Nov. 5. The mercury aſcended gradually to 52 and a half.

Nov. 28. The apricocks were reduced to water; the ſkin being ſever'd from the pulp, yet no more air produced.

Jan. 10. 1677. Whilſt it froze very hard the mercury roſe to 57 inches, but when it thaw'd it ſunk to 23. Whether the ſtrength of the froſt opened ſome way for the air to get out, I know not.

March 3. The mercury could aſcend no higher, becauſe the air was got out. This day I found the receiver tumbled on the ground; and the apricocks, when the froſt broke, were putrified, and had loſt their colour.

Hence it ſeems to follow, that apricocks produce air almoſt as eaſily in their wonted preſſure, as *in vacuo*.

From cherries.

(89.) *June* 22. 1676. I put four cherries into two exhauſted receivers, and proceeded with both alike, except that in the one the cherries were whole, in the other cut aſunder; in two hours the whole cherries had impelled the mercury into the gage, to the height of 10 lines, and the cut ones to about 20.

June 21. In 24 hours the mercury, in the receiver containing the whole cherries, roſe to the height of three inches; but in the other the gage was ſpoiled.

June 26. The whole cherries had not yet produced ſo much air, as to ſuſtain 15 inches of mercury; but the cut cherries had wholly filled their receiver with air.

July 9. The receiver of the whole cherries was remov'd from its cover; I eat one of them, which taſted pleaſant enough. I included the reſt again *in vacuo*; many of them were broke, and in one hour's time they impell'd the mercury to the height of about 2 inches.

July 10. During theſe laſt 24 hours the mercury aſcended not; whether the gage was damaged, I am not certain.

July 15. I found the cover ſevered from the receiver, whence it was clear that the gage was hurt.

Hence it appears, that ſome cut fruit ſooner produce their air, than what is whole.

(90.) *June* 9. 1676. I put ſome cherries, that were not acid, into an exhauſted receiver; and within an hour found as much air produced from them, as ſuſtain'd a quarter of an inch of mercury.

June 10. In 18 hours the mercury roſe to 11 inches.

June 11. The fruits produced leſs air gradually, ſo that this day, towards the evening, the mercury came not up to 15 inches.

June 12. The mercury was a little higher than 15 inches.

13. The height of the mercury was 22 inches.

16. The mercury yet came not up to 30.

18. Perceiving no more air to be produced, I opened the receiver.

Such a ſmall production of air ſeems very remarkable, becauſe I had found fruit of the ſame kind in *France*, to fill their receivers in two days time. Probably fruits of the ſame kind, in ſeveral countries, differ much amongſt themſelves.

From cabbages.

(91.) *June* 12. 1676. I put cabbages, cut in pieces, into an exhauſted receiver, with a mercurial gage; and in an hour's time the mercury aſcended one line.

June 13. The mercury was now come almoſt to the height of 10 inches.

17. It was come almoſt to the top of its gage; and the receiver being opened, I found the cabbages little altered.

June 19. Being left for two days in the open air, they were wholly corrupted, and blackiſh. I put them again *in vacuo*, to try whether the putrefaction begun, would promote, or retard the production of air.

June 19. The mercury, in half an hour, ran up half an inch.

22. For three whole days, the mercury got higher, only by ten lines. Its height was one, and a third of an inch.

June 23. Finding the cabbages produce no more air, I took them out of the receiver; their ſmell was very bad.

Hence a ſuſpicion aroſe, that bodies, when they putrify, have produced almoſt all their air.

From oranges. (92.) *May* 29. 1676. I took pieces of orange, weighing 4 ounces, and put them into a receiver, capable of holding 10 ounces of water; and exhauſted the air.

June 10. The receiver was removed from its cover, by the force of the air produced; ſo that I took out the oranges, and preſently put them into another exhauſted receiver, capable of containing 8 ounces of water; when the mercury, within half an hour, was elevated to the height of half an inch.

June 13. This ſudden aſcent of the mercury was not durable; for it yet came not to the height of 2 inches.

June 16. The mercury, during the laſt 24 hours, aſcended about three lines.

June 21. The mercury, the laſt 24 hours, did not aſcend the ſpace of one line.

July 18. I perceived no more alteration in the height of the mercury, but ſome mouldineſs appear'd; tho' I am certain that no air from without had enter'd the receiver.

From a tulip. (93.) *April* 27. 1676. I put a tulip into an exhauſted receiver, with a mercurial gage; but before it was fortified againſt the external air, enough got in to ſuſtain 2 inches of mercury.

May 2. The tulip, which at firſt appeared ſtriped with various colours, was now wholly changed into a dark red, become moiſt, and produced very little air.

Half a lemon. (94.) *April* 22. 1676. I put half of a lemon into an exhauſted receiver, with a mercurial gage, ſo ſhort, that the mercury could not aſcend 3 inches.

April 24. In two days ſpace the mercury came to the height of an inch and a half.

April 25. The mercury was now two inches high.

April 27. Yeſterday the mercury aſcended 4 lines, but this day only one.

April 29. During the two laſt days, the mercury mounted higher by one line.

May 3. In four days ſpace the mercury aſcended one line, and a little more.

May 3. 1677. The mercury came to the top of the gage, yet no air got out; but the lemon was a little alter'd.

Jan. 1. 1678. Hitherto no air eſcaped out of the receiver; but the lemon had contracted a yellow colour, and a moiſture.

Apples. (95.) *March* 16. 1677. I put two apples of the ſame ſort, into two exhauſted receivers; one of the apples having begun to putrefy, but the other was only bruiſed by a few blows.

May 15. 1677. Hitherto they ſeemed in a very good caſe; but now the apple which was bruiſed, appear'd wholly rotten, and the receiver was forc'd from its cover: the other apple remain'd without change.

Aug. 20. 1677. The apple, which before began to be rotten, ſuffer'd no farther alteration; but, finding that the receiver was now parted from its cover, and fearing leſt the apple would be ſpeedily putrefied, I took it out: its taſte was grateful, inclining to acid, as if it had been fermented; but the pulp ſomewhat reſembled meal in conſiſtence.

Hereby it ſeems confirmed, that fruits have produced the greateſt part of their air, when putrefaction begins in them; ſince the putrid apple did not fill its receiver, but in a much longer time than the other.

Milk. (96.) *May* 17. 1676. I pour'd two equal quantities of milk into two glaſs-receivers, of equal bigneſs; the one I left in the free air, and the other I included in an exhauſted veſſel, with a mercurial gage.

May 18. The cream floated on the top of the milk, left in the free air; but that *in vacuo*, was only cover'd with bubbles, and the gage not alter'd at all.

May 19. The bubbles gradually ſwelled; and the mercury, in the gage, was a little higher.

May 20. The bubbles, *in vacuo*, ſwell'd yet more, and that milk ſeem'd curdled; but the other, in the free air, was, manifeſtly, curdled. The mercury, *in vacuo*, came almoſt to the top of its gage.

May 22. The milk, *in vacuo*, proceeded to generate more air; and now it evidently appear'd to be curdled. Whence, it is manifeſt, that the coagulation of milk, when the air is taken away, is retarded. Almoſt all the bubbles were now broke,

June 20. The milk, *in vacuo*, was no longer cover'd with bubbles, and remain'd ſtill coagulated in the ſame ſtate. But the milk, in the free air, became very fetid, and was full of worms. When it was put on the engine, and the air extracted, it emitted many very large bubbles, for a long time; and the worms mov'd very vehemently, but not one of them died in four hours time.

Whey. *May* 19. 1677. Three or four months ago, ſome whey, *in vacuo*, was poured out of a veſſel into a receiver, and it ſeem'd clear and limpid, like water; yet there was whey enough left in the veſſel, to ſeparate the butyrous from the caſeous part, at a ſufficient diſtance.

This day the milk, ſtagnant in the receiver, ſeem'd to have got out of it; ſo that it is clear, the air, in the receiver, had a greater force than the external air; for the cover, alſo, was ſeparated from the receiver. Towards night, I took that milk out of the receiver, and found it to be acid, both in ſmell and taſte, yet it was not ungrateful to the palate; but after a ſhort time, the whey, which hitherto had remained limpid between the caſeous and butyrous part, began to diſappear, and to be blended with the reſt.

May 24. The butyrous part wholly vaniſhed; though, as yet, it had ſuffered no ſenſible mutation: but the milk began to ſmell ill.

June 1. Our milk had not, yet, contracted the worſt of ſcents; neither had it produced any worms: but it grew dry by degrees, and, this night, the mice eat it up, as, perhaps, they had the butyrous part before.

Here we ſee, that the coagulation of milk, when air is extracted therefrom, is ſomewhat retarded; that the weight of the butter, of whey, and of cheeſe, is not the ſame in the air, as *in vacuo*; for, in the air, they are confuſedly mixed; but, *in vacuo*, one ſwims on the top of the other; that the putrefaction of milk, when air is extracted, is hindered, or very much retarded; and, laſtly, that milk, by continuing long *in vacuo*, is made unfit to generate worms, even in common air.

Urine.

(97.) *September* 5. 1677. I took the ſame receiver, and the ſame veſſel, uſed before to preſerve milk *in vacuo*, and included urine therein, as I had done milk before. The quantity of urine was about three ounces, and three drams, and the receiver capable of holding ten ounces of water.

Sept. 7. The mercury reached to the height of almoſt two inches.

Sept. 8. The mercury was ſomewhat higher than yeſterday.

December 5. The mercury aſcended not above three inches; and, for the whole month paſt, was not changed. The urine ſeemed not to be at all altered.

Decemb. 6. I ſet other urine under a receiver, not defended againſt the external air.

Decemb. 16. The urine, *in vacuo*, ſtill kept unaltered; but the other in ten days time, ſeem'd turbid, and to have contracted ſome mouldineſs on its ſuperficies.

This experiment, compared with the former, makes it probable, that urine contains leſs air than milk.

But the power of the air to corrupt urine ſeems very obſervable.

Paſte.

(98.) *May* 19. I took paſte, very much diluted, and without leaven, and putting it into a glaſs-veſſel, included it in an exhauſted receiver: and tho' the veſſel which contained it, were not half full, before all the air was exhauſted; yet the paſte had ſwollen above the brim of the veſſel.

May 20. The paſte continued to ſwell, and was interſperſed with many cavities.

May 22. The paſte was much more tumid than before, and much air was generated therefrom.

May 23. In the morning I found the cover ſever'd from the receiver, by the force of the produced air, and ſome of the paſte ſpread above the edges of the receiver; yet its ſwelling was ſomewhat abated. In the afternoon, its ſwelling was much more abated, yet it took up twice more ſpace than before it was put into the receiver. The taſte of it was not acid; and, therefore, I think, that bread, thus made, is very light.

(99.) *July* 20. 1676. I put a quantity of beef into an exhauſted receiver, defended againſt the external air; and another equal quantity into a receiver, neither exhauſted, nor cloſely ſtopp'd. *Beef.*

July 21. In thirty hours time, the exhauſted receiver was fill'd with air, ſo that I ſuſpected ſome air had got in: and, therefore, included the ſame beef again, and ſo cloſed it; there was no fear any external air ſhould enter.

July 22. In fourteen hours time, the mercury roſe to the height of fifteen inches.

July 25. For three whole days and more, the beef did not produce air enough to fill one half of the receiver.

July 26. The receiver was ſever'd from the cover; and in one hour's time, I perceived the beef, being again included *in vacuo*, had produced air enough to ſuſtain ten inches of mercury.

July 28. I found the receiver again filled with air, and re-exhauſting it, much air was in a ſhort time again produc'd from the beef.

July 30. The receiver being again fill'd, I again included the beef *in vacuo*, and found, that the air produced from it in one hour, ſuſtain'd ten inches of mercury.

Aug. 1. The receiver being this day fill'd again, the beef ſtunk abominably, and we threw it away.

Hence it appears, that fleſh, whilſt it putrefies, produces much more air, than before it putrefies; but it is otherwiſe in fruit.

(100.) *July* 18. 1676. I put ſome gooſberries, which I had long kept in receivers, to produce air, into one that was exhauſted. *Gooſberries.*

Within half an hour the mercury aſcended to the height of one inch.

In an hour and a half, the mercury mounted another inch.

July 19. In twenty-four hours time, the receiver was almoſt all filled with air.

July 20. The cover was forced from the receiver, and much juice run out.

July 29. I left the ſame gooſberries in a receiver, not defended againſt the external air; but this day I included them again *in vacuo*, to try, whether they could produce any more air.

July 30. In ſixteen hours time, the gooſberries drove up the mercury an inch and a half into the gage.

July 30. 1677. The gooſberries could not wholly fill the receiver; and they always remain'd in the ſame ſtate; but a while ſince they had almoſt loſt their red colour, and inclined to white.

From hence it ſeems to follow, that this fruit, after it has produced all its air, ſuffers very little alteration; as if that air itſelf were the cauſe of corruption.

(101.) *Aug.* 23. I put pears into an exhauſted receiver with a mercurial gage; and before the receiver could be well defended againſt the external air, the mercury was riſen one inch and a half. *Pears.*

In two hours time it aſcended four inches; its height being almoſt ſix.

Auguſt 24. The height of the mercury was twelve inches.

25. The height thereof was ſixteen.

Aug. 26	The height of it was	18	*Aug.* 28	The height of it was	23
27		21	31		30
Sept. 1	The height of it was	32	*Sept.* 4	The height of it was	44
2		35	5		45 ½
3		38 ½	6		50

Sept. 7. The height of it was the ſame, becauſe ſome air had eſcaped; but I prevented that for the future.

Sept. 8	The height of it was	53 ½
9		54 ½
10		58

Sept. 12. Yeſterday the mercury remained at the ſame height; but now it ſeem'd to be depreſſed: whence I conjecture, that ſome air had got out. The height of it was fifty-three and a half.

Sept. 13. I tranſmitted the air into another receiver: the height of it was thirty-two and a half.

Sept. 16. I perceived that the air had got out; and opening the receiver, found the pears very rotten.

Theſe pears produced their air irregularly, ſometimes quicker, ſometimes ſlower.

Dried plumbs.

(102.) *Sept.* 17. I put dried plumbs into an evacuated receiver.

Sept. 19. The mercury ſeem'd to have aſcended a little.

Sept. 22. I perceived not, that the height of the mercury was alter'd.

Novemb. 9. When the plumbs produced no more air, I open'd the receiver.

By this experiment, 'tis confirmed, that dried fruit is very unfit to produce air.

And nut-kernels.

(103.) *Sept.* 28. I put freſh nut-kernels, cut to pieces, into an evacuated receiver, with a mercurial gage.

Sept. 29. The mercury aſcended a little.

Sept. 30. The height of it was two inches.

Octob. 5. The mercury continued to aſcend by degrees: the height of it exceeded ſix inches.

Octob. 15. The height thereof was ten inches.

Octob. 22. The height of it was fifteen.

Nov. 28. The mercury was come to twenty inches, or more; but now the receiver was thrown down and broken, and the nut-kernels ſcattered: they were preſerv'd very well, both as to colour and taſte.

Hence we may conjecture, that air, without ſenſible putrefaction, is producible from fruits, even of a hard conſiſtence.

The production of air above its uſual preſſure, in peaſe, raiſins, and water, in common air.

(104.) *June* 22. I included new peaſe in a receiver, with a glaſs full of raiſins of the ſun bruiſed, and mixed with water; and did not exhauſt the air.

Towards evening, the mercury had mounted to twelve inches; but a great part of that air was produced from the raiſins, not from the peaſe.

June 23. The height of the mercury was forty-nine.

June

June 24	The height of it was	75	*June* 26	The height of it was	90
25		90	28		100

The peaſe ſweat, as it were, and grew yellow.

June 30. The height of the mercury was one hundred and ten.

July 1. The mercury aſcended not, yet no air eſcaped.

July 4. The height of the mercury was one hundred and twenty-four.

July 7. The height of it was one hundred and forty.

July 10. The height remain'd the ſame, but the liquor, which diſtill'd from the peaſe got out.

July 12. New liquor was produc'd from the peaſe; but the mercury continu'd at the ſame height.

July 13. The liquor got out of the receiver, and ſome air beſides; whereupon I ſet the ſcrew, and new liquor being in a ſhort time collected, fortify'd the cover within.

July 15. The receiver was broken in pieces; but the peaſe, being ſofter than ordinary, were eaſily ſtript of their husks, as if they had been parboil'd; but they kept their ordinary taſte.

In plumbs in vacuo.

(105.) *Sept.* 15. 1676. I put unripe plumbs into an exhauſted receiver; and before the receiver could be guarded againſt the external air, the mercury aſcended an inch.

Sept. 16. In twenty-four hours time, the mercury ran up five inches, and its height was ſix.

Sept. 17. The height of the mercury was eight.

Sept. 18	The height of it was	10	*Sept.* 23	The height of it was	18
19		12	24		19
20		14	26		23
22		18	28		26

Octob. 1. The height of the mercury was thirty.

Octob. 4. The height of it was thirty-one, the weather ſomewhat cold.

Octob. 5	The height of it was	32	*Octob.* 9	The height was 33½
7		33	11	

Octob. 15. For theſe two laſt days, the cold being abated, the mercury aſcended ſwifter; its height was thirty-ſeven.

Octob. 17	The height of it was	38	*Octob.* 29	The height of it was	45
19		39½	*Nov.* 2		46
22		41	5		47
26		43	20		53

In this experiment, the air ſeems to be produced ſometimes regularly, and at others irregularly.

In Gooſberries in vacuo.

(106.) *July* 6. 1676. I put gooſberries into an exhauſted receiver, but before we could prevent the entrance of the external air, it had impell'd the mercury half an inch; and afterwards, in half an hour, the air produced from the gooſberries, impell'd it another half inch.

In ſeven hours time, the mercury aſcended four inches higher, and reſted at five.

July 7. In fourteen hours, the aſcent of the mercury was two inches and a half.

In 10 hours, the aſcent of it was 2 $\frac{1}{2}$.

July 8. In 14 hours, the aſcent of the mercury was 1 $\frac{1}{2}$.

In 10 hours, the aſcent was 2.

July 9. In 14 hours, the aſcent of the mercury was 2 $\frac{1}{2}$.

In 10 hours, its aſcent was 1 $\frac{3}{4}$.

July 10. In 14 hours, the aſcent of it was 1 $\frac{3}{4}$.

In 10 hours, the aſcent of it was 3.

July 11. In 24 hours, the aſcent of the mercury was 4.

July 12. In 24 hours, the aſcent was 4.

Now the mercury was brought to its wonted preſſure.

July 13. In the morning I found the cover broken; and becauſe it was faſtened by a ſcrew, to prevent its being ſever'd from the receiver, I ſuſpected this happen'd from the internal air. I ſubſtituted another cover in its ſtead.

July 14, 15, 16, 17, 18. I perceived no change in the height of the mercury, becauſe the cover was not exactly cloſed; and therefore I took out the fruit, and put part into another evacuated receiver, and the reſt I ſtopp'd up cloſely with common air, that nothing might get out.

In 4 hours, the mercury aſcended 4 inches.

July 19. In 14 hours, the aſcent of the mercury was 1 $\frac{1}{2}$; but ſuſpecting the air to have eſcaped, I ſet the ſcrew.

In 9 hours the aſcent of the mercury was 11 inches.

The cover broke and the air eſcaped.

This experiment ſeems to prove that gooſberries contain much air, which, as ſoon as it is freed from the wonted preſſure of the air, more readily breaks out, than when reſtrained by ſome ambient air, till the gooſberries begin to ferment; for then air is produced in a far larger quantity, tho' the compreſſure be greater.

In paſte in vacuo.

(107.) *July* 8. 1676. I included paſte in an exhauſted receiver, and, before it was guarded againſt the external air, the mercury was come to the height of three inches; the air making an irruption from without; whence the paſte, which was much ſwollen, loſt about the third part of its tumidity.

A little while after, it ſwell'd again; and within half an hour, the mercury mounted higher by two inches.

In one hour's time, the aſcent of the mercury was two and a half; and the paſte continued to ſwell.

In another hour the aſcent of the mercury was three inches and a half.

In an hour more, the aſcent of it was four inches and a half; and it reſted at ſixteen.

July 9. In fourteen hours ſpace, the aſcent of it was twenty-one inches, and the height of the mercury thirty-ſeven. I ſuſpected that ſome air had got out. When I ſet the ſcrew, the cover broke; and upon admiſſion of the external air, the paſte, which always roſe, now abated about two inches of its tumidity; though it was leſs compreſs'd than before.

In five hours, the aſcent of the mercury was fifteen inches.

But, when I again endeavour'd to ſet the ſcrew, the cover broke, ſo that the air eſcaped ; and the paſte was preſently ſomewhat depreſs'd.

In four hours, the aſcent of the mercury was ten inches: the paſte again ſwell'd, as before ; but, being willing to ſubſtitute a better ſcrew, I permitted the air to enter ; yet the paſte did not now ſubſide, as before.

July 10. This night the paſte roſe again ; yet it ſeemed to have produced no air.

In four hours there was no aſcent of the mercury.

In ſeven hours, the aſcent of it was four inches.

July 12. I perceived no aſcent of the mercury.
13. It ſeemed to have aſcended a little.
17. Seeing no more air produced, I took out the paſte, and found it to have a ſouriſh ſmell.

This experiment ſeems to prove, that air might be produced out of paſte, in compreſs'd air, as well as *in vacuo.*

But the paſte was twice depreſſed, becauſe the compreſs'd air, ſuddenly finding a way to eſcape, was dilated ; as happens in ſprings, when carried beyond their point of reſt ; but, when that air was immediately repell'd by the external air, the paſte pitch'd, and was depreſs'd.

In beans in vacuo.

(108.) *July* 13. 1677. I included ſome horſe-beans *in vacuo*, with water ; when thoſe which were bruiſed, ſeemed to ſwell much ; but thoſe which were left whole, ſuffer'd no ſenſible alteration.

In two hours ſpace, I ſaw no air produced, tho' the beans continued to ſwell.

July 14. In twenty-four hours, the aſcent of the mercury was ſeven inches.

July 15. In ſixteen hours, the aſcent of the mercury was three inches and a half.

In eight-hours, the aſcent of it was one and a half, the height of it twelve.

July 16. In fourteen-hours, the aſcent of it was three.
17. In twenty-ſix hours, the aſcent of it was ſix.
18. In twenty-four hours, the aſcent of the mercury was almoſt 9.
19. I ſtopp'd the receiver firmly with a ſcrew, becauſe the air had got out. In nine hours the aſcent was one inch.
20. In twenty-four hours the aſcent was three and a half.
21. In twenty-four hours the aſcent was five and a half.
22. In fourteen-hours, the aſcent of the mercury was two.
23. In twenty-four hours the aſcent of the mercury was eighteen.
24. In fourteen hours, the aſcent of the mercury was almoſt five. The height of it thirty-five above the wonted preſſure.
25. The receiver could not ſuſtain a greater preſſure. I found the beans of a fetid ſmell, not much unlike that of putrefied fleſh.

Hence it ſeems to follow, that beans contain much air, and that it is produc'd in a moderate preſſure, as well as *in vacuo*; ſometimes more ſuddenly, ly, ſometimes more ſlowly. But eſpecially, that great inequality, which happen'd *July* 23, is obſervable.

In gooſberries in vacuo.

(109.) *July* 23. I included gooſberries in an exhauſted receiver, and guarded them very well againſt the external air.

In two hours the mercury aſcended one inch.

July 24. The height of the mercury was ſeven inches and a half.

July 25	The height of it was	12	*July* 27	The height of it was	20
26		17	28		24½

July 29. The height of it was almoſt 30.

30. The height of it was almoſt 31. I tranſmitted ſome air out of this receiver, into another evacuated receiver, and the height of the mercury was 26.

31. The height of the mercury was 35.

Auguſt 1. The height of the mercury was 39. But ſome air had eſcaped; and going to ſtop the receiver cloſe, I ſuffer'd more air to get out.

The height of the mercury was 30.

Aug. 2. The height of the mercury was 39. I tranſmitted ſome air into another receiver.

The height of the mercury was 31.

Aug. 3. The height of the mercury was 39.

4. The height of the mercury was 41.

5. The height of the mercury was 43. I tranſmitted the air into another receiver.

The height of the mercury was thirty inches.

6. The height of the mercury was 43.

7. The height thereof was 47.

8. The height thereof was 48. But the air being tranſmitted into another receiver, the height of it was 36.

9. The height of the mercury was 41, in fourteen hours.

Aug. 10. The height of the mercury was 47; the air being tranſmitted into another receiver, the height of it was 35, in twenty-four hours.

11. The height of the mercury was 38 and a half, in fourteen hours.

12. The height of the mercury was 42, in twenty four hours. I extracted the air, and the height of the mercury was 26.

13. The height of the mercury was 33, in twenty-four hours.

14	The height of it was	36	24	17	The height was	44	24
15		39		18		47	
16		41½		19		50	

I tranſmitted the air into another receiver; and the mercurial gage was ſpoiled. I took out the gooſberries, and found they had loſt their colour and almoſt all their acidity.

From hence we may infer, that gooſberries produce their air regularly, unleſs ſomething be extracted out of the receiver; for then they acquire a power to produce new air more ſpeedily.

In grapes in vacuo.

(110.) *Sept.* 12. I put crude grapes into an exhauſted receiver; but before they could be fenced from the external air, as much had got in as ſuſtain'd three inches of mercury.

Sept.	The height of it was	*Sept.*	The height was
13	5	17	19
14	10	19	23
16	17	20	25

Sept. 22. The height of the mercury was 30. I ſtopped the receiver with a ſcrew.

23. The height of the mercury was about 30 and a half.

24. The height thereof was 32.

Sept.	The height of it was	*Octob.*	The height of it was
26	34 ½	2	39 ½
27	36 ¼	4	39 ½
28	36 ¼	5	40 ½
29	37 ¼	7	41 ½
30	37 ¾	9	41 ½

Octob. 15. The height of the mercury was 46. It aſcended chiefly on theſe two laſt days, when the froſt was diſſolved.

Nov. 2. The height of the mercury was 54.

5. The height was 58.

Jan. 10. 1677. The mercury was come to the height of 70 inches; yet I perceived no ſenſible change in the mercurial gage, even when the cold was ſharpeſt; tho' the grapes and their juice were turn'd to ice.

Sept. 21. Hitherto the grapes ſeem'd not altered; but the mercury had aſcended a little, becauſe the air found a paſſage out. I open'd the receiver, and when the air broke forth, many of the grapes ſeemed to be wrinkled. The grapes had kept their taſte, but it was much more pungent, the juice continued tinged of a curious red colour.

This experiment ſeems to inform us, that grapes produce not all their air, but in a long tract of time.

In pears in vacuo.

(111.) *Aug.* 10. 1677. I put pears, cut aſunder, into an exhauſted receiver. Towards evening the mercury was riſen ten inches.

Aug.	The height of it was	*Aug.*	The height was
11	20	15	55
13	38	16	60
14	48	17	68

The air being tranſmitted into another receiver, the height of the mercury remained at 53 and a half.

Aug.	The height of it was	*Aug.*	The height of it was
18	61	20	70
19	64	21	72

The air being tranſmitted into another receiver, the mercury remained at 61.

Aug.	The height of it was	*Aug.*	The height of it was
22	68	24	79
23	74	25	81

The air being tranſmitted into another receiver, the height of the mercury was 61.

Aug. 26. The height of the mercury was 56. Some air having got out, I tranſmitted the reſt into another receiver, and the mercury remain'd at 52.

Aug. 27	The height of it was	60	*Aug.* 30	The height of it was	83
28		68	31		88
29		75	*Sept.* 1		93

Sept. 2. The height of it was 100.

Sept. 3. The height of it was 89; ſome air having eſcaped, which made me cautious to prevent the like for the future.

Sept. 4. The height of the mercury was 100.

5. The ſame height continued.

7. The ſame height ſtill continued, tho' no air eſcaped.

9. The height of the mercury was 107.

10. The height of the mercury was the ſame.

The air being tranſmitted into another receiver, the mercury reſted at 99.

Sept. 11. The mercury moved not.

13. The height of the mercery was 105.

Octob. 8. I found that the air had got out.

This experiment ſeems to inform us, that pears produce their air by fits.

Miſcellaneous experiments, and firſt, melted lead and tin cooled in vacuo.

(112.) *March* 16. I melted down lead, with a fire in a braſs veſſel, whoſe diameter was an inch and half; but before the lead was concreted by cold, I put it into a receiver, out of which I ſuddenly exhauſted the air; whence the figure of the lead was concave, and the parts of it the more depreſſed, nearer the center; but lead congealed in common air, exhibits a convex figure, except in the middle, where there is a little cavity.

I made the ſame experiment on tin, with the ſame ſucceſs; and tho' both metals being fluid, and very hot, had remained long *in vacuo*, yet no bubbles ſeemed to riſe from either; but all other hot liquors ſeem to yield numerous bubbles *in vacuo*.

Salt and water in vacuo.

(113.) *Sept.* 2. I put water ſaturated with ſalt *in vacuo*, to try whether it would be there converted into cryſtals, and the ſalt be carried above the ſuperficies of the water, as happens in the free air.

Sept. 15. The water, with the diſſolved ſalt, abiding in the ſame ſtate, I open'd the receiver; and, as no vapours could eſcape, 'tis reaſonable to judge, that the ſalt could not there be converted into cryſtals.

The air of gooſeberries in vacuo.

(114.) *Auguſt* 8. 1676. I put air produced from gooſeberries, into an evacuated receiver, furniſhed with a mercurial gage.

March 1. 167$\frac{6}{7}$. I perceived no change in the height of the mercury, and therefore, open'd the receiver.

The weight of air to that of water.

(115.) *Auguſt* 8. I took a vial, able to hold ſeven ounces, five drams, and three grains of water, and exhauſted the air out of it; and when, in a balance, it was ſuſpended in equilibrium, with another weight, I pierced the bladder which cover'd the orifice, with a needle, and then the vial being fill'd with air, appear'd heavier by four grains and a half; which latter weight to the former, is as 1 to 814: whence it follows, that water is about at leaſt, 800 times more ponderous than air of an equal bulk. This day was hot and clear; and ſome air is always left in the receivers after exhauſtion.

Aqua fortis, *and fixed nitre* in vacuo.

(116) *Jan.* 16. 1677. I put *Aqua fortis*, with fixed nitre, into a receiver; and having exhauſted the air as much as I could, poured one of them on the other, and found much air produced. I marked the height of the mercury in the gage.

March 5. Finding the produced air was not deſtroy'd, and that the mercury perſiſted at the ſame height, I open'd the receiver, and found nitre produced *in vacuo* from the mixture.

Oil, water, and ſpirit of wine, in compreſs'd air.

(117.) *May* 12. 1677. I fill'd a long and very narrow-neck'd vial, with oil, up to the middle of the neck, and put it into a receiver, firmly ſtopp'd by the help of a ſcrew; into which I afterwards intruded air, till it ſuſtain'd 120 inches of mercury above its wonted height. The oil, in the neck of the vial, appear'd depreſſed about a quarter of an inch; the cauſe whereof I judg'd to be the compreſſure of the air; but having eaſed the ſcrew, and thereby ſuffer'd the air to break in, and being dilated, the oil did not aſcend at all; ſo that, I ſuppoſe, it was condenſed only by cold.

Auguſt 5. I made the ſame experiment, after the ſame manner, uſing water inſtead of oil; yet could perceive no change of the height of the water in the neck of the glaſs; tho' the heat, being moderate, might have produced a ſenſible effect.

Jan. 14. 1678. Finding, by ſome experiments, that compreſſed air enters into the pores of the water, and pierces even to the bottom, a ſuſpicion might ariſe, that the water was not condenſed by the compreſſed air, becauſe the air entring into the pores, made the preſſure within equal to that from without; I, therefore, filled the abovesaid glaſs with ſpirit of wine; leaving only the length of three inches in the top of the neck thereof, which was filled with air only. Then applying my hands to the glaſs, the ſpirit of wine being heated, ſoon filled the whole neck to the top. The glaſs being now inverted into a veſſel of mercury, I removed my hands, when the ſpirit of wine being ſoon cooled, ſuffer'd the mercury to poſſeſs three inches in height. I put the veſſel, and the glaſs, in that poſture into a receiver, and afterwards compreſs'd the air therein, till the mercury exceeded its wonted height 90 inches; yet there was no ſenſible condenſation of the ſpirit of wine, nor any aſcent of the mercury: however, it is certain, that no air had crept in, becauſe the mercury hinder'd it; and the receiver being open'd, when the air, that compreſs'd from without, was dilated, no bubbles appear'd in the ſpirit of wine.

Here it ſeems worth enquiring, how the ſpirit of wine was ſo ſenſibly condenſed by a moderate cold, and not at all by a great compreſſure of the air.

Spirit of wine and oil of turpentine in vacuo.

(118.) *May* 12. 1676. I poured ſpirit of wine into a glaſs veſſel, and added ſome drops of oil of turpentine thereto, which ſwimming upon the ſpirit of wine, began to be there whirl'd about. I put the glaſs veſſel on the pneumatic engine, and cover'd it with a receiver; yet the bubbles did not at all ceaſe to move up and down. Then I pump'd out ſome of the air; when the bubbles, emerging from the ſpirit of wine, adhered to the drops of oil, and carried them to the ſides of the veſſel, and there detained them; yet two drops, free from ſuch bubbles, proceeded to have a further motion. Afterwards, I wholly exhauſted the receiver, and ſome drops roſe to the top thereof, by the force of the bubbling ſpirit of wine; but the remaining drops continued to be moved a little, and ſoon after reſted. The air being admitted, the drops began again to renew their motion, but it was ſlow, and quickly ceaſed.

I repeated the experiment with ſpirit of wine, and oil of turpentine, purged from air; and no ebullition was then made, nor did any bubble appear; but the drops of the oil of turpentine were moved *in vacuo*, as in the open air.

Hence it ſeems to follow, that the cauſe of the motion of the drops, is not owing to the diſſolution; for all diſſolutions *in vacuo*, have hitherto, ſeemed to me, to produce bubbles.

Radiſhes and claret in vacuo.

(119.) *May* 19. 1676. Yeſterday I left two radiſhes *in vacuo*, one of them hanging with the root downwards, the other in a contrary poſture; and both cut tranſverſly, reſted over a veſſel, which contain'd red wine. Theſe remaining for a whole night *in vacuo*, ſeem'd well purged of their air. Opening the receiver, I added two other radiſhes to the former, cut after the ſame manner, having firſt taken off their thick ſkin. Then exhauſting the receiver, I immerg'd the cut part of all the radiſhes, at once, into the ſubjacent wine; upon which many bubbles ſeem'd to ariſe from them: and more bubbles proceeded from thoſe radiſhes which were purged of air, for a whole night, than from thoſe which had not remained above half an hour *in vacuo*, with their ſkins off.

Hence bubbles ſeem to be formed of particles of air, ſwimming in water; and becauſe in the ſkin there are ſome canals, fit to retain parts of air, the peeled radiſhes afforded no opportunity for the formation of ſo many bubbles.

The liquor aſcended no leſs into thoſe radiſhes which hung with their roots upwards, than into the others.

A ſmall glaſs tube, plunged in water, the infuſion of nephritic wood and ſpirit of wine, in vacuo.

(120.) *May* 4. 1676. I immerged one end of a ſmall open glaſs tube, into water ſtagnant *in vacuo*, and preſently the water aſcended up into it, as uſual in common air, and to the ſame height; but ſoon after, many bubbles being formed there, raiſed the water higher, and kept it ſuſpended in three different places, intercepted by many bubbles; and ſeveral other bubbles ſeemed to paſs out from the end immerſed in water.

Then ſealing the other end of the tube hermetically, and making the experiment in common air, the water aſcended not up into the tube at the open end. But *in vacuo*, it aſcended therein, as if it had been open at both ends; and many bubbles ſuddenly form'd, ſeparated the water, contained in the tube, to a great diſtance, as before: in the mean time, many other bubbles ſeemed inceſſantly to paſs out from the end of the tube immerſed, though they afterwards appeared leſs frequent.

But the water being ſuſpended higher in the tube, ſeem'd to contain no bubbles, whilſt the end only emitted ſo many.

Then I took out that end from the water, and no more bubbles appear'd, tho' it was wholly filled with a cylinder of water.

May 5. I repeated the experiment; but before I had immerſed the end of the tube in water, a drop, which ran over from the upper aperture of the receiver, flowed down to the open end of the tube, and penetrated into it to the height of two lines; and no bubble was formed there in a full half hour. I afterwards, plunged the end of the tube into the water of the veſſel, and bubbles ſoon began to be formed as before; ſome of which ſucceeded others within half a minute: but, afterwards, they were leſs frequent. Repeating this experiment, many times, I perceived, that when the water was extracted from the tube, no bubbles appear'd; but if it were immerged in water, ſome would adhere to the end of it, either ſooner or later.

May 6. I made the ſame experiment, with the infuſion of nephretic wood, with a like ſucceſs; excepting that the bubbles emerged, and penetrated the liquor, before they had acquired any conſiderable bigneſs: whence we may conjecture, that this liquor is very thin, and hath no viſcoſity to reſiſt a pervading body.

May 10. I repeated the ſame experiment with ſpirit of wine, mixed with a certain oil, made *per deliquium*, but found nothing new; only the liquor aſcended not ſo high into the tube.

Hence the bubbles ſeem to be formed, at the extremity of the tube, of aerial particles, ſwimming in the water; which finding ſome impediment at that end, cannot paſs by, and ſo, new ones coming upon them, they ſwell into bubbles.

Horſe-beans and water included in an iron tube.

(121.) *July* 18. 1676. Two days ago, I took ſome horſe-beans, and included them in an iron tube, cloſely ſtopped; firſt pouring water on the compreſſed beans, till the tube ſeem'd wholly full; to try whether the expanſive force of the beans would break the tube. This day the tube ſeem'd not to be altered, but, the ſtopple being looſen'd, ſome air broke out, and much water, which was not imbibed by the beans, fell upon the ground: then we heard a noiſe, as it were, of bubbling water, for above an hour.

July 25. The tube remained in the ſame poſture; but now one of the ends of it being unſtopped, and ſome beans taken out, the murmur of the bubbling water was heard as before.

From hence it ſeems to follow, that beans contain air, which, in a great compreſſure, cannot eſcape; but breaks out, if freed from the compreſſing force.

Spirit of ſal-armoniac, and copper filings in vacuo.

(122.) *March* 4. 1677. I put a glaſs, half full of ſpirit of ſal armoniac, and copper filings, into a well exhauſted receiver, and ſtopp'd it up: in 15 minutes, the liquor had contracted a blue colour, very much diluted; bnt, the air being admitted, in three minutes the blue colour appeared vivid and thick. I put the liquor ſo tinged, again *in vacuo*, to try whether that colour would, in time, vaniſh.

April 4. The blue colour almoſt quite diſappear'd, but quickly return'd upon admiſſion of the air.

A certain oil, and ſpirit of wine in vacuo.

(123.) *May* 8. I put a certain oil, made *per diliquium*, and ſpirit of wine, into an exhauſted receiver: the ſpirit always ſwam on the top; and, leſt the ſpirit ſhould bubble over the edges of the veſſel, I extracted the air, by degrees; when, at firſt, great bubbles aroſe from the ſpirit, and but very ſmall ones from the oil; after one hour, the oil afforded large bubbles, which, from being ſmall at the bottom, fill'd, in their aſcent, the whole breadth of their veſſel: and, after another hour, ſome bubbles broke out with ſo great force, that they hit againſt the top of the receiver.

May 9. I repeated the experiment in a glaſs ſomewhat long and narrow, that I might the better perceive the motion of the bubbles; and I ſaw the bubbles paſſing out of the oil into the ſpirit of wine, without any great increaſe of their quantity: but being diſtant only one quarter of an inch from the ſuperficies, they were ſuddenly expanded.

Aqua fortis, *ſpirit of wine, and iron* in vacuo, *and common air.*

(124.) *May* 3. 1676. I mixed a quantity of *Aqua fortis*, with a larger of ſpirit of wine; then diſtributed the mixture equally into three glaſs veſſels, and put three equal pieces of iron into them, to each veſſel one. This done, I included one of the three veſſels *in vacuo*; and there many great ebullitions were made. In a quarter of an hour I took out the veſſel, and found the liquor black and turbid; whilſt the other two veſſels had their liquor not alter'd in colour; only ſome black powder appear'd at the bottom.

One of theſe two veſſels I put *in vacuo*, and there aroſe ebullitions, great indeed, but much leſs than the former: in one quarter of an hour, I took out the veſſel, and found the liquor black, yet leſs ſo than the former; but that which was left always in the air, remain'd, in a manner, unchanged.

May 4. In the morning, the liquors in the two veſſels, put *in vacuo*, appear'd clear and green.

But that in the open air bubbled more ſtrongly, than it did yeſterday, and was of a red colour. I put the three veſſels together *in vacuo*, and perceiv'd no remarkable ebullition, only ſome bubbles appear'd larger in the red liquor, than in the other two.

From hence it ſeems to follow, that ſpirit of wine accelerates ebullition *in vacuo*.

Spirit of ſal-armoniac and copper filings in artificial air of paſte.

(125.) *Jan.* 21. 1678. I had a glaſs half full of ſpirit of ſal armoniac, and filings of copper, the mouth whereof was ſo exactly ſtopp'd, that the blue colour, induced by the external air, now wholly diſappear'd. The ſtopple was made of leather, prepar'd after a particular manner.

This glaſs I ſet *in vacuo*, with unfermented paſte, that the receiver being full of air, from the paſte, I might perforate the leather that ſtopp'd the glaſs; and try whether the contact of the air, generated from the paſte, would alſo communicate a colour to the liquor.

Jan. 22. There was no need to perforate the leather; for I found the liquor already tinged; whence it is probable, that air produc'd from paſte is endu'd with ſuch minute particles, as to penetrate leather, which is impervious to common air.

Jan. 25. The liquor became almoſt colourleſs; whence it appears that common air is too thick to penetrate all paſſages, which are pervious to air, produc'd from paſte.

Feb. 2. I put the ſame vial *in vacuo*, but did not cement the receiver to the cover; ſo that the air, gradually entring, in twenty-four hours, fill'd the receiver, as it was leiſurely fill'd with the air produc'd from paſte; yet the liquor ſtill remain'd colourleſs.

Feb. 15. I put the ſame glaſs again *in vacuo*, with ſome quantity of paſte; but this time, the air produc'd from thence, did not pervade the leather, as it had done before, and the liquor was not at all tinged.

A ſhrew-mouſe in an engine that filtres air thro' water.

(126.) *April* 2. 1678. I put a ſhrew-mouſe into the filtrating engine; and when I perceiv'd him reduc'd to extremity, I began to ſtir the pump that the air might be, as it were, filtred thro' the water. The mouſe, a while after, ſeem'd to be better, yet not wholly reſtor'd; and having been long kept faſting, I am uncertain, whether he died for want of aliment, or of new air.

April 12. I repeated the experiment with a ſmall weakly mouſe, that had been kept a long time without food. And finding the ſame ſucceſs as before, I took out the mouſe before he was dead, but he recover'd not: ſo that more experiments are requir'd, to ſhew the effect of this filtration.

Frog-ſpawn in vacuo, *common air, and compreſs'd air.*

(127.) *May* 2. 1678. Six weeks ago, I included frog-ſpawn in three receivers, the firſt of which was exhauſted; the ſecond contain'd common air; and into the third, I intruded ſo much air, that the mercury reſted ſixty inches above its uſual height,

In fifteen days, the mercury in the evacuated receiver roſe an inch. The ſpawn in the common air ſeem'd corrupted, and of a blackiſh colour; but that in the compreſs'd air, remain'd unalter'd in colour; tho' no frogs were generated.

In a month's time, the ſperm *in vacuo* had not changed its colour, excepting the black round ſpots; but ſeem'd reduc'd into water: the colour of that in the common air was very black, but in the compreſs'd air the ſpawn began to be reddiſh.

As yet, no change was perceiv'd, either in the ſpawn *in vacuo*, or that in the common air; but in the compreſs'd air it appear'd redder.

May 22. The ſperm *in vacuo* was not chang'd; in the compreſs'd air it remain'd red; but in the common air it again became colourleſs.

June 23. The ſperm *in vacuo*, and in common air was not tinged, but in the compreſs'd air it inclin'd to green.

Octob. 15. I took the ſpawn from all the veſſels; that kept *in vacuo* was almoſt exhaled out of its veſſel, and appear'd ſtagnant in the receiver, like clear water; that in the common air remain'd colourleſs; but that in the compreſs'd air ſtill kept its red colour.

Oranges in receivers, with and without water.

(128.) *May* 9. 1678. Six days ago, I included two pieces of the ſame orange in two receivers, not quite of equal bigneſs; in the greater, there was left ſome quantity of water, ſo that the ſame ſpace remain'd for the air in that, as in the leſs. The orange included with water, tho' it were not touch'd by it, was four times more mouldy, than that kept without water.

And therefore, in repeating this experiment, I put two pieces of the ſame orange into two receivers, but fill'd the third part of one of them with water, yet ſo, that it did not reach the orange.

June 15. Neither of the pieces had contracted any mouldineſs.

May 16. I repeated the experiment with the ſame ſucceſs, only neither orange had acquired any mouldineſs in the ſpace of more than a month; tho' in former experiments, all ſuch oranges grew mouldy.

The cauſe of the difference, ſeems to be ſome particular diſpoſition of the air.

Turpentine included in a wind-gun.

(129.) *June* 1. 1678. I put a ſmall glaſs tube, half full of *Venice* turpentine, into our wind-gun; and had ſcarce reduc'd the air to the tenth part of its wonted ſpace, but the leather ſpread over the elliptic valve, was driven out; ſo that, the air having eſcap'd, I drew the glaſs-tube out of the engine, and found many bubbles formed in the ſuperficies of the turpentine. I therefore, ſuſpected that the air had pervaded the turpentine; and that it would have penetrated deeper into it, if they had remain'd longer thus inclos'd together. I plac'd the ſame tube in the ſame gun, and there left it in air reduc'd to about the fifteenth part of its natural ſpace.

June 3. I open'd the engine, and taking out the tube, found the turpentine almoſt free from bubbles; yet by degrees, many were formed therein, in the parts remote from the ſuperficies.

June 4. I put new turpentine into the ſame tube, and included it *in vacuo*, that it might be the better purged of air; then I pour'd the water upon it, and ſhut up all in the wind-gun.

June 8. I open'd the engine, and at firſt ſight, both the water and the turpentine in the tube, ſeem'd to be very free from bubbles; but ſoon after I perceiv'd, that bubbles were form'd in the turpentine, and that they aſcended by degrees; ſome of them ſeem'd to be made, almoſt at the very bottom, about half an inch below the ſuperficies of the turpentine. Whence we may conjecture, that all the water, and ſo great an height of the turpentine, were pervaded by the air, which formed thoſe bubbles.

Spirit of ſal-armoniac, and copper-filings in vacuo.

(130.) *Auguſt* 11. 1678. I included ſpirit of ſal-armoniac, with a mercurial gage, *in vacuo*; and after the ſpirit ceas'd to emit any bubbles, I mix'd copper-filings therewith, which caus'd many bubbles to riſe again; but they were ſo far from producing any air, that they conſum'd what was there before. But the liquor became greeniſh and turbid.

Decemb. 5. The ſpirit was almoſt all exhaled out of the containing veſſel, and being condens'd in the receiver, remain'd ſtill turbid, by reaſon of much filth, which was included there: but that which was not exhaled out of the veſſel, appear'd clear like water. The mercury alſo, was wholly expelled out of the gage. Whence I conjecture, that the air in the receiver was gradually more conſumed.

Cylinders of tin and lead immerſed in mercury, in vacuo, *and in common air.*

(131.) *Sept.* 2. 1678. I put two cylinders, one of tin, the other of lead, *in vacuo*; their loweſt parts were immerſed in mercury; and at the ſame time I immerſed two other cylinders, like the former, after the ſame manner in mercury; but theſe latter were left in the free air.

Sept. 6. I opened the exhauſted receiver, and the mercury in the tin cylinder was riſen 4 inches and a half above the ſuperficies of the ſtagnant mercury; and cutting the cylinder tranſverſly, in the middle of that height, the amalgam ſeem'd to have penetrated into the cylinder about half a line. And cutting the cylinder tranſverſly again in that part, which was diſtant only one inch from the ſuperficies of the ſtagnant mercury, I found the thickneſs of the amalgam equal to one line.

In the lead-cylinder, the mercury roſe two inches and a half; but only as far as the ſuperficies; and the very part immerſed in the mercury, was not penetrated by it to any ſenſible thickneſs.

Sept. 7. I took the tin cylinder left in the air, out of the mercury, in which it was immerſed, and found the mercury to have aſcended to the height of five inches.

Sept. 10. The ſame cylinder being left in the mercury, ſeemed to be beſmeared therewith to the very top, ſix inches and more above the ſuperficies of the ſtagnant mercury. When the cylinder was tranſverſly cut in ſeveral places, the mercury appeared to have pierced the deeper into the tin, the nearer it came to the ſtagnant mercury; ſo that in the part adjacent to the mercury, almoſt the whole diameter of the cylinder, three lines broad, was penetrated thereby.

In the lead cylinder, the mercury exceeded not the height of 3 inches and a half; neither had it penetrated to any ſenſible thickneſs. Whence it appears, that the weight of the air contributes little or nothing to the aſcent of mercury into metals.

A whiting included in vacuo, *in common air, in air compreſs'd, in artificial air, and left in the open air.*

(132.) *Decemb.* 12. 1678. I took a ſmall whiting, and having cut off his head, divided him tranſverſly into five pieces; the firſt whereof I included *in vacuo.* The ſecond in common air. The third in air ſo compreſſed, as to ſuſtain mercury 50 inches above its wonted height. Theſe three receivers were cloſed with ſcrews. The fourth piece was put into a receiver full of air, produced from paſte, which was preſently ſtopt. The fifth was left in the free air.

Decemb. 15. In the morning, that part of the whiting which was left in the free air, began to ſhine; and towards evening it gave a more vivid light.

Decemb. 16. In the morning, the whiting left in the free air, ceaſed to ſhine; but towards evening ſhone again.

Decemb. 17. This morning the ſame part of the whiting ſhone a little, yet leſs than yeſterday in the evening.

Decemb. 18. In the morning there appeared no light, tho' I long fixed my eyes upon the receiver in a dark place; but the night coming on, the light appeared again.

Decemb. 20. Hitherto the ſame part of the whiting left in the air continued to ſhine; but all the other parts did not yet begin to do ſo.

Decemb. 22. Yeſterday, the light of the whiting, left in the air, had not quite ceaſed; but this day it appeared no more.

Decemb. 24. The part of the whiting in the free air, entirely gave over ſhining; that included in common air, did yeſterday yield a faint light; but this day it ſhone no more.

Decemb. 26. No more light appeared in that in the common air; but the three other pieces did not begin to ſhine.

Jan. 26. 1679. I perceived no more ſhining in any one of the receivers.

Artificial air deſtroyed; and firſt that of cherries tranſmitted into a receiver full of common air.

(133.) *Aug.* 3. 1677. I tranſmitted air, produc'd from cherries, into a receiver full of common air, but ſo ſtopt with a ſcrew, that the mercury aſcended to 25 inches above its uſual height.

Aug. 4. The mercury was depreſſed about two inches. The height of it this day was only 23.

Aug. 6. The height thereof was reduced to 20.

Aug. 7. The height thereof the ſame.

Aug. 8. The mercury was ſomewhat depreſſed.

Aug. 10. The height of it was 19 and a half, above its uſual ſtandard; and perceiving little or no alteration, I opened the receiver.

Hence we have a confirmation, that air produced from fruits, at the beginning, is in part deſtroy'd; but that the reſt can very long retain the form of air.

That of ſal-armoniac, and oil of vitriol, in vacuo.

(134.) *May* 26. 1676. I put ſix grains of ſal-armoniac into a receiver, with a ſufficient quantity of oil of vitriol; then the air being exhauſted, I forced down the ſalt into the oil; whereupon a great ebullition preſently follow'd, and the mercury aſcended in the gage almoſt to its wonted height; but preſently after it ſunk again, and return'd to its former ſtate.

May 27. I repeated the experiment, the ſalt remaining ten hours *in vacuo* before it was put into the oil; but the ebullition proceeded as before; yet the air was produced much more ſlowly, nor could it wholly be deſtroyed in 7 or 8 hours time; yet at laſt the mercury deſcended to the very bottom.

May 29. I made the ſame experiment again, leaving the materials for 24 hours *in vacuo*; the ebullition ſeemed much leſs, and the air was produced, both in a leſs quantity, and more ſlowly than before. I obſerved alſo, that whilſt the materials remained *in vacuo*, before their mixture, the mercury came nearer to the open end of the gage, as if ſome air had been either extracted or deſtroyed.

And of oil of vitriol with a fifth part of common air.

June 8. I put oil of vitriol alone into a receiver, in which I left only a fifth part of common air, to try whether this oil, without ſal-armoniac, would diminiſh the elaſtic force of the air; but the force of the air was increaſed, and the mercury in one hour's time ſeemed to have aſcended a little into the gage; tho' afterwards for 24 hours no change happened.

This experiment ſhews, that ſome artificial air may be deſtroyed; but why this deſtruction happens, ſometimes ſooner, ſometimes later, deſerves a further enquiry.

The different celerity wherewith air is produced in vacuo, *and in common air, ſhewn from paſte in common air.*

(135.) *July* 10. 1676. I put paſte, made two days before, and now grown ſouriſh, into a receiver, and ſtopt it firmly with a ſcrew.

In one hour the height of the mercury was one inch.

In ſeven hours the height of it was ſix.

July 11. The height of it was eleven.

July 12. The height of the mercury was 24.

July 13. The height thereof was 30.

July 14. The height of the mercury was ſenſibly greater.

July 15. The mercury aſcended a little. Meaſuring its height exactly, I found it 38 inches.

July 19. No more air was produced from the paſte.

Paſte in vacuo.

July 10. 1676. I put another quantity of the ſame paſte, much leſs than the former, into an exhauſted receiver.

Tho' the quantity of the paſte was leſs, yet in one hour's time the height of the mercury was two inches.

In ſeven hours the mercury came almoſt to the top of the gage; but it was a ſhort one.

July 19. The paſte was not able to move the receiver from its cover; tho' at the beginning, it had produced a greater quantity of air, than the paſte in common air. I endeavoured to fire it with a burning-glaſs, and the fumes elevated therefrom, afterwards falling upon the paſte, tinged the ſuperficies thereof with a pleaſant yellow colour; and that air was thus produced, I conjectur'd, becauſe the cover was afterwards eaſily ſevered from its receiver.

Hence we learn, that air is ſometimes generated much more eaſily *in vacuo*, than in common air.

(136.) *Auguſt* 20. 1676. I put paſte, kept for 24 hours, in a receiver full of common air; to which I added new air, ſo that the mercury exceeded its wonted height 4 inches and a half.

In 6 hours the mercury gain'd almoſt 4 inches, and its height was 8.

Aug. 21. The aſcent of the mercury was 4 and $\frac{2}{3}$.

Aug. 22. The aſcent of it was about 1.

23. The aſcent of it was half an inch.

26. For three whole days the aſcent of the mercury was only half an inch.

27. There was no aſcent of it at all.

29. The paſte, taken out of the receiver, ſmelt acid.

Auguſt 20. I put another quantity of the ſame paſte into an exhauſted receiver, and obſerved the ſame proportion between the quantity of the paſte, and the capacity of the veſſel, as in the former experiment.

The mercury preſently ſeemed to have aſcended. Its height was two inches.

Aug. 21. The aſcent of the mercury was 5.
22. The aſcent of it was 3.
23. The aſcent of the mercury was 1.
26. For three whole days the aſcent of it was 2.
27. There was no aſcent of the mercury.
29. I took out the paſte, exhauſted of its air, from the receiver.

This experiment farther confirms, that air is ſometimes more eaſily produced *in vacuo*, than in common air.

Filberd kernels in vacuo.

(137.) *Sept.* 4. 1677. I put the kernels of filberds into an exhauſted receiver.

Sept. 5. The height of the mercury was 5 inches.

Sept. 6	The height of it was	10	*Sept.* 11	The height of it was	18
7		10	12		23
8		12	13		27
9		15	14		29

Sept. 15. The height of it was almoſt the ſame.
17. The height of it was 30.
18. This day the air began to get out of the receiver; for ſome bubbles appeared in the turpentine, which cloſed the juncture of the receiver and cover.

And filberd-kernels in common air.

(138.) *Sept.* 4. I put kernels of filberds into a receiver with common air.

In the afternoon the quantity of air ſeemed to be leſſened.

Sept. 5. The height of the mercury was leſs than half an inch.
6. The height of it was the ſame.
7. The height of it was one inch.
8. The ſame height continued.
18. The ſame height continued.

This experiment confirms, that ſometimes air is produced much more eaſily *in vacuo*, than in common air.

Raiſins with water in vacuo.

(139.) *September* 15. 1677. I included 8 ounces of raiſins of the ſun, bruiſed and diluted with a little water, in an exhauſted receiver, able to hold 22 ounces of that fluid.

Sept. 16. The height of the mercury was ſix inches.

Sept. 17	The height of it was	10	*Sept.* 19	The height of it was	29
18		15	20		29

Sept. 21. This day I found the receiver forced from its cover.

Sept. 24. I took out ſome of the raiſins, but thoſe that remained I incloſed in the ſame evacuated receiver.

Sept. 25. The raiſins forced the receiver, now full of air, from its cover.

And raiſins with water in common air.

September 15. 1677. I put 8 ounces of raiſins of the ſun, bruiſed and diluted with a little water, into a receiver, able to hold 22 ounces of water; but did not exhauſt the air at all.

Sept. 16. The mercury was 3 quarters of an inch above its uſual height.
Sept. 17. The height of the mercury was 1 and a half.
18. The height of it was 3.

Sept. 19	The height of it was	5	*Sept.* 22	The height of it was	11
20		7	23		12
21		9	24		15

Permitting the air to break out, many bubbles emerged from the raiſins.

This experiment further teaches, that air is ſometimes much more eaſily produced *in vacuo*, than in common air.

Onions in vacuo.

(140.) *February* 17. 1677. I put three onions into an exhauſted receiver.
Feb. 19. The height of the mercury was one inch.
21. The aſcent thereof was again 1. The onions were not alter'd.
25. The whole aſcent of the mercury was 9. The onions not alter'd.
May 4. The onions had yet ſuffered no alteration.
18. Neither were they yet alter'd.
June 19. I found the receiver forced from its cover, and the onions rotten.

Onions in rarified air.

Feb. 17. I incloſed three onions in air, ſo rarified, that it could ſuſtain only ten inches of mercury.
Feb. 19. There was no aſcent of the mercury.
21. There was yet no aſcent thereof. The onions did not ſprout, but contracted a mouldineſs.
25. The aſcent of the mercury was about 7 inches. The onions received no further alteration.
May 4. The onions were not alter'd.
18. The onions were not yet alter'd; but the receiver, by the force of the produced air, was removed from its cover.

And onions in common air.

Feb. 17. I put three onions in a receiver not exactly ſhut.
21. They contracted no mouldineſs, but ſprouted.
25. They gradually took root.
May 4. The onions began to be mouldy.

This experiment makes it probable, that ſome bodies produce their air not much more eaſily *in vacuo*, than in rarified air.

It hence alſo appears, that vegetation is hinder'd, not only by the evacuation, but alſo by the rarifaction of the air.

It likewiſe deſerves our obſervation, that the onions, as long as their roots ſprouted, contracted no mouldineſs.

The difference betwixt whole, and bruiſed fruits, ſhewn in bruiſed pears in vacuo.

(141.) *Auguſt* 23. 1677. I put bruiſed pears into an exhauſted receiver, with a mercurial gage.
Auguſt 25. The height of the mercury was five inches.

Aug. 26	The height of it was	10	*Aug.* 29	The height was	21
27		14	30		25
28		18	31		28

Sept. 1. The height of it was 30.
2. The receiver was forced from the cover.

And whole pears in vacuo.

Auguſt 23. I put whole pears into an exhauſted receiver; the quantity of the pears, and the capacity of the receiver, being the ſame with thoſe juſt mentioned.
Aug. 25. The height of the mercury was 11.

Aug.

Aug. 26	The height of it was	17	*Aug.* 28	The height of it was	28
27		25	29		30

Aug. 30. The mercury aſcended no higher; the receiver being forced from the cover.

This experiment ſeems to prove, that bruiſed fruits do not produce air ſo ſoon as entire ones.

In whole apples in vacuo.

(142.) *Auguſt* 24. I encloſed whole apples *in vacuo*, with a mercurial gage.

Aug. 25. The height of the mercury was five inches.

Aug. 26	The height of it was	9	*Aug.* 29	The height was	19
27		12	30		25
28		15	31		28

September 1. The height of it was 29.
2. The height of it was 30.
3. The receiver was forced from the cover.

And bruiſed apples in vacuo.

Auguſt 24. I put an equal quantity of bruiſed apples into an evacuated receiver, of the ſame capacity with the former.

Aug. 25. The height of the mercury was one inch.
26. The height of it was three.
27. The height of it was four.

Sept. 3. The mercury continued at the ſame height.
25. The mercury aſcended not.

This experiment ſeems to inform us, that bruiſed fruits produce air, ſlower than whole ones.

In bruiſed grapes in vacuo.

(143.) *Auguſt* 25. 1677. I put unripe grapes, bruiſed, into an evacuated receiver.

Aug. 26. The height of the mercury was one inch.
27. The height of it was two inches.
28. The height of it was two and a half.
29. The height of the mercury was the ſame.

Sept. 15. The mercury did not aſcend, but its height remain'd at 2 ½.

And whole grapes in vacuo.

Aug. 25. 1677. I put whole unripe grapes into an evacuated receiver.

Aug. 26. The height of the mercury was three inches.
27. The height of the mercury was five.

Aug. 28	The height was	7	*Aug.* 30	The height was	12
29		10	31		13

Sept. 1. The height of the mercury was 15.
2. The height of it was 16.
3. The height of it was 18.
4. The height of it was the ſame.

Sept. 5. The height of the mercury continued the ſame; but almoſt all the grapes had contracted a yellow colour.

Sept. 7. The mercury reſted at the ſame height; and all the grapes were yellow.

Sept. 15. The height of the mercury was 20.

This experiment ſhews, that whole fruits produce air more readily than bruis'd.

In whole grapes.

(144.) *Sept.* 10. 1677. I put two ounces of grapes, not bruis'd, into a receiver able to hold ten ounces of water.

Sept. 11. The height of the mercury was ſix inches:

Sept. 12	The height was	9	*Sept.* 15	The height was	20
13		12	16		25
14		15	17		28

Sept. 18. The height of the mercury was thirty. The grapes were not at all alter'd.

Sept. 19. The height of the mercury was the ſame.

Sept. 20. The receiver was not yet forced from the cover. The grapes were not alter'd, but appear'd only a little riper.

Sept. 21. The receiver was forced from the cover, though nothing had eſcap'd.

Sept. 22. In the morning the grapes began to rot; I therefore included them again *in vacuo.*

Sept. 23. The height of the mercury was five inches.

Sept. 24	The height was	9	*Sept.* 27	The height was	20
25		14	29		27
26		17	30		28

Octob. 10. The receiver was not forced from the cover, till to-day: the grapes, by their colour, ſeem'd rotten, yet kept their firmneſs.

And bruis'd grapes.

Sept. 10. 1677. I included two ounces of ripe, bruis'd grapes in a receiver capable of holding ten ounces of water.

Sept. 11	The height of the mercury was	4	*Sept.* 15	The height of it was	15
12		7	16		18
13		10	17		20
14		12	18		25

Sept. 19. The grapes had ſever'd the receiver from the cover, and much juice was ſpilt.

Sept. 20. I again put the ſame grapes into the ſame receiver; but, becauſe they had ſpilt their juice by ebullition, I did not exhauſt all the air: the mercury reſted at the height of five inches.

Sept. 21. In the morning, the receiver, being now full of air, no longer adher'd to the cover; ſo that I took out the grapes, and tranſmitted them into another receiver, which I ſtopt cloſe with a ſcrew, but extracted no air from it.

Sept. 22. The height of the mercury was eleven inches; tho' the receiver was able to hold twenty-ſix ounces of water.

Sept. 23. The height of the mercury was 19.
24. The height of it was the ſame.
30. The height of it was 20.

Octob. 3. When the grapes produced no more air, I took them out, and found them of a bitter taſte; being not yet perfectly ripe.

This experiment, compared with that before related, of unripe grapes, ſeems to intimate, that unripe grapes produce leſs leſs air when they are bruis'd, than when whole; but that ripe grapes do the contrary.

And in ſound and bruis'd apples, in vacuo.

(145.) *Nov.* 19. 1678. I put apples into three evacuated receivers. In the firſt was a ſound apple; in the ſecond an apple bruis'd, and laid looſe in the open veſſel; in the third was alſo a bruis'd apple: and the cover of this ſo fitted the included veſſel, that it ſtraitly compreſs'd the parts of the apple; but in exhauſting the receiver, the air, form'd between the parts of the apple, expell'd all the juice.

Nov. 21. In the firſt receiver, the height of the mercury was 5 inches; in the ſecond, three; in the third, none.

Nov. 23. In the firſt receiver, the height of the mercury was ſeven; in the two others there was no change.

Decemb. 7. In the firſt receiver, the height of the mercury was eleven. There was no alteration in the other two.

Jan. 23. The firſt receiver was now ſever'd from its cover, by the force of the air produced a-new. In the two others there was no air generated.

May 20. 1679. The third receiver was forced from its cover; but the ſecond had produced no air.

This experiment informs us, that bruis'd fruits produce leſs air *in vacuo*, than ſound ones; contrary to what happens in common air. The reaſon whereof may, perhaps, be this, that fruits bruis'd are very much rarify'd *in vacuo*; whence the ſeveral principles, of which they conſiſt, cannot act upon one another: but unbruis'd fruits, by reaſon of the entireneſs of their ambient ſkin, ſuffer leſs rarifaction.

That air is ſometimes unfit to produce mouldineſs, ſhewn by roſes in common and compreſs'd air.

(146.) *July* 12. 1678. I put roſes into 2 receivers, to be ſtopt with ſcrews. One of them contain'd common air uncompreſs'd; but I intruded ſo much air into the other, as ſuſtain'd the mercury 60 inches above its wonted height.

Aug. 2. The roſes in the common air, were four days ago turn'd yellow, as if they had been wither'd; but thoſe in the compreſs'd air, kept their colour very well.

Feb. 10. 1679. Thoſe in the compreſſed air, retain'd their freſh colour.

This experiment, compar'd with that made laſt year with roſes, informs us, that the air, at different times, is differently affected; ſo that ſometimes it hath a power to hinder corruption, and ſometimes to promote it.

And by tulips and lark-ſpurs.

(147.) *May* 22. Fifteen days ago, I included two equal quantities of flowers, in two receivers: into one of them, I thruſt ſo much air as ſuſtained the mercury ſixty inches above its wonted height; but in the other, I left common air uncompreſs'd. The flowers were tulips and lark-ſpurs.

Since that time, no mouldineſs appeared, except, only, that ten days ago, one half of the tulip, in the common air, being cut aſunder, ſeemed ſomewhat mouldy; and now the other half of the ſame tulip in compreſs'd air, ſeem'd alſo a little mouldy.

Some of the flowers ſeemed as freſh as when firſt put in; eſpecially thoſe in the common air; but in the compreſs'd air, they ſeem'd moiſter.

June 22. No more mouldineſs appear'd; whence we have it confirm'd, that the air is, ſometimes, unfit to produce mouldineſs; ſince laſt year, all this kind of flowers, contracted a great mouldineſs.

(148.) *Sept.* 4. 1678. I expoſed one dram of minium, in an open glaſs, to the ſun-beams, concentrated by a burning-glaſs; and found that it loſt $\frac{3}{4}$ grain of its weight, though much of the minium had not been touch'd by the rays.

The change of weight made by the ſun's rays, in veſſels hermetically ſealed, ſhewn by expoſing red-lead thereto in an open glaſs.

(149.) *Sept.* 6. I took calcined coral, and endeavour'd to calcine it further, by the rays of the ſun, in a ſealed glaſs; and the whiteneſs of the calx was ſomewhat increaſed hereby.

Calcin'd coral in a ſealed one.

Sept. 10. I expoſed the ſame coral again to the ſun-beams, in the ſame glaſs hermetically ſealed, for two whole hours; and then weighing the glaſs, found it had loſt about $\frac{1}{16}$ part of a grain, ſince it was firſt ſealed.

(150.) *May* 23. I put calx of tin in a light glaſs vial, hermetically ſealed, and weigh'd it exactly; afterwards I expoſed it to the beams of the ſun for a long time, by the help of a large lens; then the glaſs, being again weigh'd, ſeemed to have loſt $\frac{1}{64}$ part of a grain of its weight.

And the calx of tin, minium and ſulphur.

May 29. I repeated the experiment with minium, inſtead of calx of tin, and the loſs of weight came to $\frac{1}{32}$ part of a grain.

May 30. I endeavour'd to calcine the ſame minium again, but ſuch plenty of air was produced, that the glaſs broke, with a great noiſe, into an hundred pieces.

June 6. I made the ſame experiment again with minium; and then $\frac{1}{64}$ part of a grain was wanting of the weight.

Attempting again to burn minium, the glaſs alſo broke.

July 15. I uſed wood-coals for the ſame experiment, but the ſun did not at all affect them.

July 20. I expoſed *Sulphur vivum*, to the beams of the ſun, in the ſame manner; and tho' it was eaſily melted, and emitted many fumes, yet I found no change at all in the weight.

Aug. 1. I kept the ſame vial ſtill, with the flower of ſulphur; and often expoſed it to the fire of the burning-glaſs, without danger of being broken; becauſe ſulphur produces no air; the fumes roſe, and at firſt, the ſulphur bubbled: but the weight remain'd the ſame.

(151.) *Nov.* 6. A piece of roaſted rabbet, being exactly cloſed up, in an exhauſted receiver, was two months, and ſome few days after, taken out without appearing to be corrupted, or ſenſibly alter'd, in colour, taſte, or ſmell.

Bodies preſerved chiefly in vacuo, *and firſt ſome roaſted rabbet.*

(152.) *March* 11. A ſmall glaſs receiver, being half fill'd with pieces of white bread, was exhauſted, and ſecured.

Bread.

April 1. The receiver being open'd, part of the bread was taken out, and appear'd not to have been impair'd in that time; only the outſide, of ſome pieces of crumb, ſeem'd to be a very little leſs ſoft and white, than before. There appear'd no drops, or the leaſt dew, on the inſide of the glaſs. The remaining bread was, again, ſecured ſoon after.

April 18. The bread was taken out again, and taſted much as it did the laſt time; the cruſt being alſo ſoft, and no drops of water appearing on the inſide of the glaſs.

Milk. (153.) *March* 9. I open'd a ſmall exhauſted, and ſecured receiver, wherein about three months ago, we had included ſome milk, which was well-colour'd, and turn'd partly, into a kind of whey, and partly into a kind of ſoft curd. The taſte was not offenſive, only a little ſouriſh, like whey, nor the ſcent fetid, but ſomewhat like that of ſouriſh milk.

Violets. (154.) *March* 5. Violet-leaves, put up, freed and ſecured from air, being open'd, *April* 7. appear'd not to have chang'd their ſhape, colour, or conſiſtence, but their odour could not be well judg'd of; becauſe he who included them, had cruſh'd many of them together, in thruſting them down; ſince by ſuch a violation of their texture, 'tis natural for violets to loſe their fragrancy, and acquire an earthy ſmell.

(155.) Having carefully placed ſome violets in an exhauſted receiver, of a convenient ſize and bigneſs, and ſecur'd it from immediate commerce with the external air; after ſeven months, we looked upon them again, and found they were not putrefied, or reſolved into any mucilaginous ſubſtance, but kept their ſhape entire; ſome of them retaining their colour, but more of them having ſo loſt it, as to look like white violets.

Sheep's blood. (156.) *Nov.* 5. We convey'd into a conveniently ſhaped receiver, ſome ounces of ſheep's blood, taken from the animal, kill'd that afternoon. After the exhauſtion of the air, during which, numerous bubbles were generated, that made the liquor ſwell conſiderably; the included blood was kept in a warm place for twenty days; and during one or two of the firſt, the blood ſeemed to continue fluid, and of a florid colour; but afterwards degenerated into one, that tended more to blackneſs.

Nov. 25. We let in the external air; and the glaſs, containing the blood, being held in a light place, the greateſt part of the bottom of it ſeem'd to be thinly overlaid with a coagulated ſubſtance, of a higher colour than what ſwam above it; which though it appear'd dark, and almoſt blackiſh in the glaſs, whilſt view'd in the bulk, yet, if it was ſhook, thoſe parts of it that fell down along the inſide of the glaſs, appear'd of a deep fair colour. But, whilſt the blood continu'd in the glaſs, it was ſuppos'd not to ſtink; ſince even when it was pour'd out, tho' its ſcent ſeem'd, to me ſomewhat offenſive, yet to others, it ſeem'd to ſmell like the blood of a dog, newly kill'd.

Cream. (157.) *March* 17. Some cream being put up, and ſecur'd in an exhauſted receiver, appear'd a year after, to be more thick, and almoſt like butter, at the top, than in other parts; and afterwards, by being well ſhaken together in the glaſs, it was eaſily enough reduced to butter, whoſe butter-milk, by the judgment of thoſe who were more uſed thereto than I, appear'd not different from ordinary butter-milk; and, I found it had, like that, a grateful ſourneſs. The butter was judg'd to be a little ſourer than ordinary, but was not, as they ſpeak, made.

Beef, &c.

(158.) *Feb.* 18. We look'd upon three vials, that had been exhauſted, and ſecured *Sept.* 15. laſt; the one of theſe had in it ſome ſlices of roaſted beef, the other ſome ſhivers of white bread, and the laſt ſome thin pieces of cheeſe; all which, ſeem'd to be free from putrefaction, and look'd, much, as when they were firſt put in; we therefore, let not the air into the receiver, but left them, as they were, to prolong the experiment.

Flowers.

(159.) *Feb.* 18. There was a fourth vial, wherein, about ſix months before, had been incloſ'd, and ſecured ſome july-flowers, and a roſe; yet, theſe being kept in the ſame place with the reſt, tho' they ſeem'd a little moiſt, retain'd their ſhape and colour, eſpecially the roſe, which look'd, as if it had been lately gather'd. We obſerv'd, in none of theſe four receivers any great drops, or ſo much as dew in the parts ſituated above the included matter.

Straw-berries.

(160.) *June* 4. We left ſome ſtrawberries in an exhauſted receiver, and coming to look upon them after the beginning of *November*, we found them to be diſcolour'd, but not alter'd in ſhape, nor mouldy; we therefore, left them ſtill in the receiver for further trial.

Cheeſe, &c.

(161.) *May* 2. 1669. A piece of roaſt beef, ſecur'd *September* 15. laſt, appear'd to be not at all alter'd; no more did a piece of cheeſe, ſecured in another receiver, and ſome pieces of a *French* roll, ſecured on the ſame day, in a third.

Flowers, ſeal'd up *Auguſt* 12. 1668, being this day look'd upon, appear'd freſh.

Small-beer.

(162.) *June* 17. A pint of ſmall beer, being put into a conveniently ſhaped glaſs, afterwards exhauſted, and ſecured from the air; the moſt part of *Auguſt* proving extraordinarily hot; towards the latter end, there was, at ſeveral times, great thunder, which turn'd the beer in our cellar, and in moſt of thoſe of the neighbourhood, ſour. *Sept.* 1. The beer was open'd but did not ſeem to be ſour.

Ale.

(163.) To try, whether the thunder would have ſuch an effect upon ale, exactly ſtopt in glaſs veſſels, as it often has on it in ordinary caſks; I caus'd ſome ale, moderately ſtrong, to be put into a conveniently ſhaped receiver, and having exhauſted the air, and ſecur'd the glaſs veſſel, 'twas put into a quiet, but not a cool place. About ſix weeks after the liquor had been incloſ'd, there happen'd ſome very loud thunder, and our beer upon this, tho' the caſk was kept in a good cellar, being generally noted to have been turn'd ſour, I ſtay'd yet a day or two longer, that the operation upon our included liquor might be the more certain and manifeſt; and then taking out the ale, found it good, and not at all ſour'd.

Black-berries.

(164.) Some black-berries, included in an exhauſted receiver, *Sept.* 21. 1670. were open'd *June* 20. 1673. and found free from all mouldineſs, and ill ſcent; only there was ſome ſour liquor, which being taken out, the berries were ſecured again. At the ſame time, another parcel of the ſame berries was exactly cloſ'd up in a receiver, whence the air was not pump'd; but coming, *Octob.* 11. 1673. to look upon the glaſs, we found it crack'd, and the fruit all cover'd with a thick mould. Nor was this the only veſſel, wherein trials made to preſerve fruits without any exhauſtion of the air, miſcarried.

Octob. 11. 1674. The former berries *in vacuo*, being look'd upon, appear'd much leſs black than before; but did not ſeem putrefy'd, either by loſs of ſhape, or by any ſtinking ſmell; nor was the leaſt mouldineſs obſerv'd upon them, tho' they had been kept in the ſame receiver for above four years.

Ale. (165.) *June* 14. We put a convenient quantity of ale into a bolt-head, and ſeal'd it up hermetically; the next year, *July* 5. we broke off the ſeal, and found the liquor very good, and without any ſenſible ſourneſs. The next day, it was ſeal'd up again, and ſet by for thirteen months, when the neck of the glaſs being broken, the ale was found ſour. We ſee however, that a ſmall quantity of ale was preſerv'd good, at leaſt, above a year; which is much longer than that liquor uſually keeps.

Claret. (166.) *June* 14. 1670. In a large bolt-head, we hermetically ſeal'd up above a pint of *French* claret, which when we came to look upon *July* 5. 1671. appear'd very clear and high colour'd, and had depoſited a large ſediment at the bottom of the glaſs, but faſten'd no tartar, that we could perceive to the ſides. Upon breaking the ſeal'd end of the glaſs, we thought there was an eruption of included air, or ſteams; and high above the ſurface of the wine, there appear'd a certain white ſmoke, almoſt like a miſt, and then gradually vaniſh'd: the wine continu'd well taſted, and was a little rough upon the tongue, but not at all ſour.

The bolt-head was ſeal'd up again *July* 6. 1671. and ſet by, till *Auguſt* 5. 1672. at which time it was open'd again, and the wine ſtill taſted very well.

June 26. 1673. The bolt-head, with the ſame claret, being open'd, was found very good, and ſeal'd up again. *Octob.* 11. 1674. the ſame wine was open'd again, and appear'd of a good colour; it was not ſour, but ſeem'd ſomewhat leſs ſpirituous, than other good claret; perhaps, becauſe of the cold weather.

Bodies preſerved in compreſs'd liquors, and firſt apricocks with raiſins and water. (167.) *Aug.* 3. 1678. I included two apricocks in two receivers, one of which was exactly fill'd with bruis'd raiſins of the ſun, and with water; but in the other, there were only lodg'd a few raiſins, ſo that the apricock was not touch'd by them, or their moiſture.

Sept. 10. I took out the apricock, inclos'd with the water; and whilſt the air broke out, the fruit bubbled very much: the raiſins had loſt, almoſt all their taſte, but the apricocks preſerv'd a pleaſant reliſh; and ſeem'd more pleaſant than ſuch fruit uſually is at that ſeaſon of the year.

Feb. 10. 1678. The apricock, inclos'd without water, kept its colour and figure, only ſeem'd to have loſt its firmneſs.

This experiment informs us, that the taſte of ſome fruits may be preſerv'd in an infuſion of raiſins of the ſun; at leaſt in veſſels able to reſiſt a great compreſſure of the air.

Peaches in an infuſion of raiſins. (168.) *Sept.* 17. 1678. I included peaches, with an infuſion of raiſins, in two receivers, ſhut with a ſcrew.

Sept. 21. Too great a quantity of air produced in one of the receivers, expell'd ſome part of the liquor. The other receiver retain'd its liquor.

Sept. 25. The receiver, out of which the liquor was expell'd, loſt ſome more of it; ſo that a fifth, or ſixth part, now ſeem'd empty: but, ſetting the ſcrew, the liquor was then preſerv'd. The other receiver remain'd unalter'd.

Sept. 26. The ſame receiver began again to leak, and run over: I ſet the ſcrew again.

Nov. 27. Our receiver, hitherto, ſeem'd to be exactly ſhut; but now I open'd it; and, whilſt the air was getting out, the peaches bubbled very much: one of them, which was of that ſort whereto the ſtone uſually adheres, preſerv'd its firmneſs, and a pleaſant taſte; but the other, being of the yellow-colour'd kind, was very ſoft; yet the taſte thereof ſeem'd to be more pleaſant than of the other. The liquor was very grateful.

Decemb. 28. The other receiver ſeem'd unalter'd; but, when I open'd it, innumerable bubbles immerg'd from the liquor, and from the peach. The peach, on one ſide, had preſerv'd its firmneſs; on the other, it had loſt it: but the whole was grateful to the palate, tho' ſomewhat ſharp.

This experiment ſeems to teach, that liquors may grow ſour, though no ſpirits have evaporated from them.

Peaches with grapes, apples, and an infuſion of raiſins.

(169.) *September* 20. I included peaches, with unripe grapes, in two receivers, and fill'd them exactly; the one with apples bruiſed to the conſiſtence of a pultice; and the other, with an infuſion of raiſins of the ſun.

Sept. 25. The receiver fill'd with pulp of apples, hitherto ſeem'd unalter'd; but, in the other, the air, which was generated, had thruſt out half of the contain'd liquor, and impell'd the mercury into the gage, to the height of 100 inches; wherefore, I open'd the receiver, and the peach, whilſt the air got out, was almoſt reduced to the conſiſtence of a pultice: the taſte of it was pleaſant.

I put another peach into the ſame receiver, and ſubſtituted a new infuſion of raiſins of the ſun, inſtead of that which was loſt.

Sept. 26. The mercury roſe to 30 inches above its uſual height.

27. The height of the mercury was 72.

28. The height of it was 90. The liquor work'd out.

30. The ſame height remain'd; but the liquor was all eſcaped.

October 1. All the air had, alſo, eſcaped; wherefore, opening the receiver, I found the peaches very ſoft, but of a pleaſant taſte.

Octob. 3. The receiver, filled with the pulp of apples, had loſt nothing; but now I perceiv'd, that almoſt all the juice of the apples had run out: I open'd the receiver, and found its contents very much fermented. The peach was very ſoft, but not unpleaſant in taſte.

This experiment informs us, that fruits cannot be long kept in pulp of apples, becauſe of the great production of air; tho' that happens a little later in the infuſion of raiſins.

Peaches with grapes, and the pulp of apples.

(170.) *Sept.* 23. 1678. I included peaches, with crude grapes, in two receivers; one of which was exactly fill'd with pulp of apples, the other with unripe grapes, bruiſed.

Octob. 1. The receiver, fill'd with pulp of apples, ſeem'd to have ſuffer'd no alteration; but the other was empty of liquor: this, therefore, I open'd, and found one of the peaches to have retain'd its firmneſs and taſte; but the other had loſt its firmneſs, yet retained a grateful taſte.

Feb. 5. 1679. The receiver, containing the pulp of apples, ſeem'd unalter'd: I open'd it, and the great ebullition which roſe thereupon, manifeſted, that a great compreſſion of the air was made. The pulp of apples, and the peach, retain'd a grateful taſte, but ſomewhat more pungent than ordinary.

This experiment ſhews, that juice of crude grapes cannot, conveniently, be uſed for the preſervation of fruits, by reaſon of the too great production of air.

Pears included with the pulp of apples.

(171.) *Sept.* 25. 1678. I included two pears, called butter-pears, in a receiver exactly fill'd with pulp of apples.

Sept. 28. I perceiv'd no alteration in the height of the mercury.

Octob. 5. The mercury was now riſen 15 inches.

6. The height of the mercury was above 16.

12. The mercury was not changed.

20. Three days ago, the mercury was depreſſed, though nothing had eſcaped.

Octob. 26. This day the receiver was crack'd; though I did not find that the air was compreſſed within it; but, perhaps, the ſcrew was ſet too high. The pulp of the apples was of a very grateful taſte; ſo were the pears, tho' ſoft, and one of them inclin'd to rottenneſs.

Perhaps, the crack in the receiver, was the cauſe of ſo little air being produced in this experiment.

Peaches incloſed with the pulp of apples, and unripe grapes.

(172.) *Octob.* 1. 1678. I incloſed peaches in two receivers; one of which was fill'd with pulp of apples, and the other, with unripe grapes, bruiſed.

Octob. 5. Much air was produced in the ſecond receiver, and ſome of the juice ran out. The height of the mercury was 64 inches.

Oct. 6. The juice continu'd to run out: the height of the mercury was 70.

Octob. 8. Now the juice ſeemed to be all run out of the receiver; and the height of the mercury was 86.

Octob. 12. The mercury remain'd at 86.

18. The receiver, emptied of its juice, held the air very well; and the mercury in it reſted at 86. The other receiver, fill'd with pulp of apples, had, for theſe five laſt days, ſuffer'd ſome juice to flow out.

Decemb. 4. I open'd the receiver, fill'd with pulp of apples; and tho' all the juice was gone, yet it ſtill retain'd the air, very much compreſſed; and many bubbles broke out, not without noiſe, after the receiver was quite open'd. The peach was very ſoft, and of a pungent taſte, like to that of ſtrong wine.

Jan. 22. 1679. After the effuſion of the juice in the other receiver, the mercury reſted at the ſame height. I open'd the receiver; the peaches emitted many bubbles, and were wrinkled, but their colour was little changed: their taſte was moſt pungent, and inclining to acid.

This

This experiment confirms the concluſions drawn from the former.

Peaches included with ale, beer, and wine.

(173.) *Octob.* 4. 1678. I put peaches into three receivers; the firſt of which was filled with ale; the ſecond, with hopp'd beer; the third, with wine.

Octob. 5. The height of the mercury in the firſt receiver, was 15; in the ſecond, 10; in the third, 9.

Octob. 6. The height of it, in the firſt receiver, was 25; in the ſecond, 15; in the third, 20.

Octob. 8. The height of the mercury, in the firſt receiver, was 35; in the ſecond, 15; in the third, 20.

Octob. 12. The height in the firſt receiver, was 63; in the ſecond, 15; in the third, 28.

15. The height of the mercury, in the firſt receiver, was 81; in in the ſecond, 15; in the third, 30.

16. There was no more change perceived in any of the three receivers.

18. The mercury rather deſcended, than aſcended, in all the three.

22. In the wine, only, the mercury aſcended, or deſcended, according to the degrees of heat and cold.

24. The height of the mercury, in the firſt receiver, was 96; in the ſecond, 15; in the third, 30.

30. The height, in the firſt receiver, was 115; in the ſecond, 20; in the third, 30.

Nov. 3. The height, in the firſt receiver, was 117; in the ſecond, 20; in the third, 30.

6. The height, in the firſt receiver, was 120; in the ſecond, 31; in the third, 31.

11. The height of the mercury, in the firſt receiver, was 105; in the ſecond, 31; in the third, 28. The weather was cold.

Nov. 16. The height of the mercury was the ſame. The peach, which hitherto lay at the bottom, now mounted to the upper part of the liquor, in the ſecond receiver; the reſt ſtaid at the bottom.

Nov. 25. The height, in the firſt receiver, was 140 inches; in the ſecond, 47; in the third, 32.

Nov. 28. The height, in the firſt receiver, was 96; in the ſecond, 36; in the third, 28. It was very cold weather.

Decemb. 13. The height, in the firſt receiver, was 96; in the ſecond, 47; in the third, 33. I open'd the third receiver, and found the peach firm, and of a laudable colour; but it had contracted much of its taſte from the wine, and might yet be improved by ſugar. The wine, alſo, was grateful to the palate.

Decemb. 30. The height of the mercury, in the firſt receiver, was 96 inches; in the ſecond 47. I open'd the firſt receiver; when the peaches, which had lain, till then, at the bottom of the liquor, preſently emerged to the upper part, and emitted many bubbles: the taſte of the ale, of which they had greatly partook, became pleaſant, with ſugar.

Hence fermented liquors may be uſeful for the preſervation of fruits, as being unfit to produce air.

(174.) *Sept.* 5. 1678. I included one whole peach, with another cut to pieces, in a receiver; into which, I afterwards poured old wine, till it was exactly fill'd, and then ſhut it with a ſcrew.

Nov. 20. Nothing, hitherto, ſeem'd to be alter'd; but, this day, I perceiv'd ſome of the wine run out.

Nov. 30. A third part of the wine was loſt.

Decemb. 8. The wine beginning again to run out, and there being but little of it left, I open'd the receiver, and found the peaches very much fermented, yet of a grateful, but moſt pungent taſte. The wine, alſo, was pleaſant.

From this experiment, compared with the third receiver, in the former, we may conjecture, that wine hinders the fermentation of peaches, if uſed in a ſufficient quantity; but here the quantity was not ſufficient, becauſe the pieces of the cut peach fill'd the whole receiver, ſo that no room was left for the wine, but in the interſtices.

(175.) *Octob.* 11. 1678. I put two unripe peaches, one whole, the other cut to pieces, into a receiver filled with hopp'd and fermented beer.

Octob. 12. In one night's time, the mercury aſcended three inches.
15. The height of the mercury was 15.
16. The height of it was 15.
18. The height of it was 12. It was very cold.
20. The height of it remained at 12.
22. The mercury aſcended again. The cold abated.
Nov. 2. The height of the mercury was 20.
3. The mercury aſcended a little. It was cold weather.
6. The height of the mercury was 28. The weather grew hotter.
8. The height of it was 33.
11. The height of the mercury was 40.
12. The height remained at 40. Some of the beer worked out.
16. The height of it was 46.
19. The height of it was 43. But much of the beer was loſt.
21. The mercury aſcended not, but the beer continued to work out.
23. When the beer was almoſt all work'd out, I open'd the receiver, and found the peaches very ſoft, yet of a grateful taſte; tho' they were kept for nine hours in the free air, after the receiver was opened.

From this experiment, compared with the ſecond receiver, we may infer, that beer hinders the fermentation of peaches, and the production of air, if uſed in a ſufficient quantity: but here there was only a little beer, contained in the interſtices, which was unable to hinder the fermentation of the peaches.

Raw beef included with ſtale beer, and common air.

(176.) *Octob.* 19. 1678. I included raw beef in three receivers; the firſt of which was exactly filled with ſtale beer, forcibly intruded; ſo that the mercury exceeded its wonted height by ſixty inches. The ſecond was alſo exactly fill'd with ſtale beer, but here there was no compreſſure made. The third was fill'd, partly with the beef, and partly with common air.

Octob. 20. In the firſt receiver, the mercury was depreſs'd to twenty inches below its uſual height; tho' nothing at all had eſcaped out. In the ſecond, alſo, it deſcended; but in the third, it aſcended a little.

Octob. 26. In the firſt receiver, the mercury ſometimes aſcended, and then deſcended very irregularly; in the ſecond, it began to aſcend ſlowly, two days ago; in the third, it was not mov'd at all.

Octob. 27. A piece of the ſame beef, which was left in the air, began to ſmell ill; and the mercury in the third receiver, began to aſcend; in the ſecond, it continu'd to aſcend gradually; but in the firſt, it ſeemed rather to deſcend.

Nov. 3. The mercury in the firſt receiver aſcended not; in the ſecond, the height of it was twenty inches; in the third, ten.

Nov. 5. I open'd all the receivers, and the two firſt had no offenſive ſmell, only contracted a ſcent from the beer. The fleſh boil'd in the ſame beer was very tender, but its taſte was bitter; perhaps, by reaſon of the too great quantity of beer. The beef included with common air, preſently ſmelt fetid, upon being open'd; yet, when taken out, and applied to the noſe, it ſcarce ſeem'd to ſtink. I included the ſame fleſh in the ſame receiver, to try whether new air being admitted, would promote corruption.

Nov. 6. The height of the mercury was 3 inches.
11. The height of it was 9.
25. The height of it was 20.

I open'd the receiver, and found the fleſh ſo fetid, that I was forced to throw it away.

From hence it ſeems to follow, that beer may help to preſerve fleſh, eſpecially if it be forcibly intruded into the receiver; but this compreſſure is ſoon abated, becauſe the air, compreſſed in the ſame receiver, is apt to enter into, and gradually pervade the pores of the beer.

Beef included with ſalt-water, and common air.

(177.) *Nov.* 12. I included beef, preſs'd together as cloſe as I was able, in three receivers: into the firſt of them I pour'd water, mix'd with one fortieth part of ſalt, which fill'd up all the interſtices, left betwixt the parts of the fleſh; the ſecond, in like manner, contain'd ſome ſalt-water; but it was ſo forcibly intruded, that the mercury in the gage aſcended fifteen inches above its wonted height: into the third receiver, I pour'd no water, and therefore thoſe few interſtices, which could not be poſſeſs'd by the fleſh, were left for the air.

Nov. 13. The mercury deſcended in all the receivers, eſpecially in the ſecond, wherein was the compreſs'd liquor.

Nov. 18. The two receivers, which were uncompreſſed, did not drive the depreſs'd mercury upward; but that, whoſe mercury had been impell'd to fifteen inches, and afterwards had deſcended moſt, now return'd almoſt to its former height. A piece of the ſame beef, being left in the air, began to ſmell ill.

Nov. 23. In all three receivers, air was produced a-new; but to-day the mercury, in the ſecond, deſcended three inches, and the height of it was twenty; in the other two, 'twas about ſixteen. I open'd the firſt receiver, and the fleſh was not at all corrupted.

Nov. 30. I took that fleſh out of the receiver, which was put in without ſalt, and it did not ſtink at all; but, being boil'd, was very tender, and of a pleaſant taſte.

December 6. I open'd the receiver, into which I had forcibly introduced ſalt-water. The mercury exceeded its wonted height by twenty-five inches. The fleſh ſmelt ſtrong, yet did not ſtink: that *in vacuo* yielded many bubbles, which ceas'd not, till a pretty while after the receiver, in which it was included, was taken from the pneumatic engine; then the mercury, in one hour's time, came to the height of three or four inches. I, afterwards, immers'd the ſame receiver ſo exhauſted, in hot water; and the liquor, contain'd therein, bubbled very much, tho' the water, from which it borrow'd all its height, did not boil; but ſo great a quantity of air was produced, or had enter'd from without, that the receiver was quickly full. The liquor, contain'd therein, did not afterwards bubble or boil, tho' it were immerg'd in boiling water. I took out the fleſh, and found it pleaſant and tender, yet leſs ſo, than I expected; perhaps, becauſe it was not boil'd enough.

Hence, water, as well as beer, may conduce to the preſervation of fleſh.

Oiſters with their ſhells, and without, included in ſalt-water, common air, and in vacuo.

(178.) *Nov.* 29. 1678. I inclos'd oiſters in four receivers: in the firſt, the oiſters were without their ſhells, and exactly fill'd the whole ſpace; in the ſecond, the oiſters, with their ſhells, were included with common air; in the third, the oiſters alſo were included in their ſhells; the remaining ſpace of the receiver, being exactly fill'd with ſalt-water. Theſe three veſſels were firmly clos'd with ſcrews. The fourth receiver was exhauſted of air, and contain'd three oiſters in their ſhells, and eight taken out of their ſhells. When the air was pump'd out of this receiver, the oiſters, freed from their ſhells, emitted many large bubbles; but the three others ſuffered no ſenſible change, only one of them gaped.

Nov. 30. In the three receivers, ſtopt with ſcrews, air ſeem'd to be conſumed, rather than produced; but the mercury *in vacuo* aſcended a little.

Decemb. 4. Whilſt the weather was cold, the mercury aſcended not; now, when the cold began to abate, the height of it in the firſt receiver was ſeven inches; in the ſecond, none; in the third, three; and in the fourth, three.

Decemb. 5. The height of the mercury in the firſt receiver was twenty inches; in the ſecond, one; in the third, three; in the fourth, five.

Decemb. 7. The height of the mercury in the firſt receiver was thirty inches; in the ſecond, one; in the third, three; in the fourth, eight. Other oiſters, left at the ſame time in the air, ſmelt ill.

Decemb. 9. In the firſt receiver, the height was thirty; in the fourth, eleven. The reſt were not chang'd.

Decemb. 13. There was no change in the three firſt receivers; but in the fourth, the height was fourteen inches.

Decemb. 20. In the firſt receiver, the height was 46; in the fourth, 24; the reſt were not chang'd.

Decemb. 21. In the firſt receiver, the height was 52; in the fourth, 25; in the reſt, no change.

Decemb. 22. The height of the mercury, in the firſt receiver, was 60; in the fourth, 27; no change in the reſt.

Decemb. 27. In the fourth receiver, the height was 29; the reſt were not chang'd.

Jan. 1. 1679. The oiſters in the third receiver, had ting'd the water black.

Jan. 25. The mercury *in vacuo* ſeem'd ſtill to remain, almoſt, at the ſame height. But this day, ſome bubbles were form'd in the turpentine, by the internal air, about the juncture of the cover with the receiver. I, therefore, open'd the receiver, and found the oiſters very fetid. I, likewiſe, open'd the other receivers, and found the oiſters of an ill ſcent, and turn'd to a kind of viſcid gelly.

This experiment ſeems to inform us, that fiſh produce leſs air than fleſh; yet will be corrupted, tho' defended againſt the air.

Butter included in a receiver.

(179.) *Nov.* 29. 1678. I exactly fill'd a glaſs veſſel, with freſh and unſalted butter; then ſtopt it with a ſcrew. A mercurial gage was included in the ſame veſſel.

Nov. 30. In the night, the cold being very ſharp, the butter was condens'd; for the mercury approach'd nearer to the aperture of its gage.

Decemb. 2. The mercury came ſtill nearer to the aperture of its gage; perhaps, becauſe the cold daily increas'd.

Decemb. 5. The cold being abated, the mercury return'd almoſt to its former height.

Part of the ſame butter, being left in the air, began to have a very bad ſmell.

Decemb. 7. The cold returning, the mercury, again, came to the top of its gage. The butter left in the air, ſmelt worſe than before, tho' it was ſtill edible.

Decemb. 24. The butter had produced no air; being taken out of the receiver, it was of a grateful taſte, except, only, a little of the ſuperficies, which lay contiguous to the leather ſpread over the cover.

It follows, that butter may be kept a great while, if it be defended from the external air.

Whitings and wine, and whitings and oiſters, included in receivers.

(180.) *Nov.* 30. 1678. I fill'd two receivers with whitings; and that no air might be left in the vacant ſpaces, into the one I pour'd wine; and into the other, oiſters, with their juice; ſo that both receivers were exactly fill'd. When I had afterwards clos'd their covers with ſcrews, the air, in the mercurial gages was compreſs'd; but in three hours ſpace the mercury again return'd to its former mark.

Decemb. 2. The cold increaſing, the mercury came nearer to the aperture of its gage in both receivers.

Decemb. 4. The cold ceaſing, the mercury aſcended very much in that receiver wherein the oiſters were; but in the other, it moved not.

Decemb 5. In the receiver, containing the oiſters, the height of the mercury was twenty inches; but, in the other, it was not yet return'd to its uſual height.

Decemb. 7. In the receiver with oiſters, the height of the mercury was 40; in the other, it continued ſtill below its ſtandard.

Decemb. 9. The mercury, in both receivers, was changed little or nothing.

Decemb. 20. When the mercury alter'd no more, I open'd the receivers, and both of them were very fetid. It here ſeem'd new to me, that the receiver, in which the wine was, had admitted of corruption, without producing air; for, hitherto, all bodies, whilſt they were corrupting, had produced ſome.

Beef with ſpice, included in receivers.

(181.) *Decemb.* 3. 1678. I put raw beef into two large receivers, with pepper and cloves; and that no air might be left in the interſtices, I poured beer upon them; and, in no long time after, found the preſſure of the air in the receivers to be abated; the mercury in the gages, coming to the open ends.

Decemb. 8. The mercury aſcended not in either of the receivers. I open'd the one, that I might boil the fleſh; which had contracted a ſweet ſcent from the cloves; and the liquor, contained in the ſame receiver, before it was boil'd, ſmell'd like hippocras.

Jan. 2. 1679. I open'd the other receiver, and found no air produced therein: the fleſh was not at all corrupted; and, when I boil'd it *in vacuo*, I obſerv'd, that if a more intenſe fire were made, the air, or ſome ſpirits, broke thro' the ſtop-cock, which was faſten'd to the top of the receiver. The receiver being cool'd all the night, was, the day after, found almoſt quite empty of air. The fleſh was very tender, and well-taſted, only it was a little over-boil'd; for it had been kept on the fire full ſix hours.

Hence we have a confirmation, that beer may be uſeful to preſerve fleſh, eſpecially if the bitter taſte thereof be corrected by aromatics.

Larks, with beef and ale, included in a receiver.

(182.) *Decemb.* 4. 1678. I included two larks, with ſome beef, in a receiver, and fill'd all the ſpaces, unpoſſeſs'd by the fleſh, with ale; at the ſame time, I filled another receiver, with the ſame ſort of beef, adding beer alſo, but no larks.

Decemb. 9. Some pieces cut off from the larks, and expoſed to the air, began to ſmell ill; but thoſe included in the receiver, had produced little air; for the mercury was not yet come five inches above its wonted height. In the other receiver it was not moved.

Decemb. 19. In the receiver, which contained the larks, the mercury aſcended no higher; for the cover being broken, ſuffer'd the liquor to run out. Wherefore, I open'd the receiver, and boil'd both the beef and the larks, which were not at all corrupted, but very grateful to the palate. The beef had contracted a pleaſant taſte; partly from the larks, and partly from the beer.

December 23. I opened the other receiver, and the fleſh being boiled, ſeemed pleaſant; yet not ſo pleaſant as that which received a veniſon-like taſte from the larks.

Hence birds may be long preſerved by the help of beer or ale.

Apples included in receivers.

(183.) *December* 14. I included apples in four receivers; in the firſt was a whole apple, and all the interſtices were filled with powdered ſugar; in the ſecond was an apple cut in pieces, and the ſpaces filled with ſugar, as before; in the third was alſo an apple cut; but the reſt of the receiver was filled with water, wherewith a tenth part of ſugar had been mixed; in the fourth the apple was alſo cut, and the ſpaces filled with a ſolution of one part ſugar, and five of water.

December 21. In the firſt receiver the mercury began to aſcend a little, yet the ſugar did not diſſolve; in the ſecond receiver all the ſugar was melted, and the pieces of apple were ſhrivel'd; they produced much air when firſt put into the receiver. In the two other receivers the mercury began alſo to aſcend; but in the third the pieces of apple were very much corrupted, their ſkin being taken off.

December 22. Air was produced in all the receivers; but the quantities did not bear the ſame proportion amongſt themſelves as the quantities of the ſugar; for in the ſecond receiver much air was produced; but in the fourth the mercury aſcended leſs than in the third. Some air was alſo generated in the firſt.

December 27. In the three firſt receivers the height of the mercury was ten inches; but in the fourth only ſix.

December 31. In the firſt and ſecond receiver the height of the mercury was 13; in the third 15; in the fourth only 9.

January 2. 1679. In the firſt and ſecond receiver the height of the mercury was almoſt 14; in the third 17; in the fourth 11.

Jan. 7. In the ſecond the height of the mercury was 16; in the third 36; in the fourth 15; but in the firſt the mercury had not aſcended, and ſomething had eſcaped out of the receiver; I therefore eaſed the ſcrew, that I might diſpoſe it the better, and then the air made an eſcape.

Jan. 9. In the firſt receiver the height was ſix inches; in the ſecond 16; in the third 39; in the fourth 15.

Jan. 17. In the firſt receiver the height was 13; in the ſecond 19; in the third 56; in the fourth 17.

Jan. 30. In the third receiver the height of the mercury was 76 inches, and the liquor got out; I therefore opened it, and found the fruit to have loſt much of its taſte; but the water had contracted it, and was pleaſant to the palate. In the ſecond receiver the mercury aſcended no more. I opened this alſo, and found the fruit much more pleaſant than the other; yet much of its taſte was imparted to the ſugar, which was turned into a very good ſyrup.

February 16. The height of the mercury, in the firſt receiver, was 22 inches; but in the fourth 33. This I opened, and found the fruit to have loſt much of its taſte; and that the ambient water had got it, and was thereby turned into a pleaſant drink.

February 27. In the firſt receiver the height of the mercury was 30 inches.

March 15. In the firſt receiver the height of the mercury was not changed; but now ſomething eſcaped out of the receiver; I opened it, and found the apple of a laudable colour; but the pulp was ſpongy, and had loſt much of its taſte.

This experiment ſeems to teach, that ſugar is not ſo fit to preſerve fruits as fermented liquors.

A lark included with milk.

(184.) *Decemb.* 23. I filled a glaſs veſſel with milk, then ſtopt it with a ſcrew; and into another receiver I put a lark with milk, and ſtopt it cloſe.

December 24. This evening I perceived that the caſeous part was ſeparated from the butyrous in the cloſed receivers, as well as in the milk, which at the ſame time I left expoſed to the air.

December 27. I found no air produced in the receiver which held the lark; but in the other the mercurial gage was ſpoiled.

December 31. The mercury aſcended in that receiver which contained the lark; but the milk left in the air, at the ſame time that I ſtopt the receivers, ſtunk three days ago.

Jan. 1. In the receiver which held the lark, the height of the mercury was ten inches.

Jan. 2. The height of the mercury was 14½. The milk ſtagnant below the butyrous part, appeared of a red colour.

Jan. 4. The height of the mercury was 19. Some white ſediment was concreted at the bottom of the milk.

Jan. 29. The height of the mercury was 29 inches.

Jan. 25. I opened both receivers; the lark ſmelt only ſtrong, tho' it had been kept 32 days; when boiled, it was of a pleaſant taſte. In the other receiver, the caſeous part of the milk was ſub-acid, and grateful; but the butyrous part was not ſour at all.

This experiment informs us, that ſometimes milk may be ſucceſsfully uſed to preſerve fleſh.

A lark included in a receiver with butter.

(185.) *December* 24. 1678. I put a lark into a ſmall receiver, and poured butter upon it, melted over a ſlow fire, till all the interſtices were exactly filled; then I cloſed the cover with a ſcrew.

Decemb. 27. The mercury approached nearer to the aperture of its gage. The butter ſeemed to be altered; for the loweſt part of it was yellower, and the middle whiter than before. The upper part was fluid.

Jan. 5. 1679. The mercury returned by degrees to its wonted height.

Jan. 9. The mercury was ſomewhat higher.

Jan. 28. The mercury was little changed. I opened the receiver, and found that part of the butter, contiguous to the leather, ſpread over the cover, to be white, and of a very unpleaſant taſte. The butter, more remote from the leather, was yellow, and ſomething fetid, tho' edible. But the lark being roaſted, was grateful to the palate, tho' it had been kept 34 days. This experiment ſeems to ſhew, that hot melted butter is not very ſucceſsfully uſed to preſerve fleſh.

Boiled fleſh in vacuo.

(186.) *Jan.* 4. 1679. I included boiled fleſh in an exhauſted receiver, ſtopt with a ſcrew; and filled the interſtices exactly with broth of the ſame fleſh, which ſeemed a little too ſalt. Whilſt I ſet the ſcrew, all things in the receiver were compreſſed; and the mercury aſcended to the height of ſix inches into the gage; but it ſoon returned to its wonted height.

Jan. 28. The air was gradually more conſumed, ſo that the mercury now deſcended 8 inches below its uſual ſtandard. I opened the receiver, and found the fleſh very ſweet and tender. The broth alſo had an acidiſh, but a very grateful taſte.

This experiment ſhews, that boiled fleſh may be long preſerved good; which is a great convenience at ſea, where perhaps there might be no occaſion for ſalt meat. For after raw fleſh hath been included in ſcrew'd veſſels, as long as experience ſhews there is no danger of its corrupting, it may be taken out, and being perfectly boiled, be again included in the ſame receivers; and ſo, doubtleſs, it may be kept for a great while without ſalt.

Raw fleſh included, with and without ſpice.

(187.) *Jan.* 30. 1679. I put raw fleſh into two receivers; to the firſt I added pepper and cloves; in the ſecond I mixed nothing.

February 11. The height of the mercury in the firſt receiver was three inches; in the ſecond below $1\frac{1}{2}$.

Feb. 12. The height of the mercury in the firſt receiver was $4\frac{1}{2}$; in the ſecond not above $1\frac{1}{2}$.

Feb. 13. In the firſt receiver the height of the mercury was above ſix inches; in the ſecond, three. I boiled the fleſh of the firſt receiver, and it was very pleaſant and tender.

Feb. 14. The height of the mercury, in the ſecond receiver, was 5.

Feb. 19. The height of the mercury, in the ſecond receiver, was 8.

Feb. 20. The height of the mercury, in the ſecond receiver, was 11. I boiled the fleſh, and found it very tender, tho' it remained over the fire *in Balneo Mariæ*, only for 3 quarters of an hour. I put ſome part of this fleſh, before it was boiled, into a receiver, and filled all the vacuities, as exactly as I could, with the ſame fleſh, to try how long the fleſh might be preſerved when the air was thus excluded.

Feb. 28. The mercury aſcended very little.

March 20. The height of the mercury was about 16 inches. I opened the receiver, and the fleſh ſeemed of a pleaſant taſte, yet inclining to corruption.

Raw beef included with ſpice, with ſalt-water, and apart.

(188.) *Feb.* 10. I put raw beef into three receivers; in the firſt, the beef was ſeaſon'd with pepper and cloves; in the ſecond, it was encompaſſed with ſalt-water; in the third, I put neither ſalt nor ſpice.

Feb. 19. Four days ago the mercury aſcended in the third receiver; in the firſt alſo it began to aſcend; but in the ſecond not at all.

Feb. 21. In the firſt receiver, the height of the mercury was four inches and a half; in the third, ten; but in the ſecond there was no aſcent at all.

Feb. 25. The height of the mercury in the firſt receiver was 6; in the third, 19; in the ſecond, half an inch.

Feb. 26. This night there was no aſcent of the mercury in any of the receivers. I opened the third, and the fleſh, after boiling, was very good.

By the former experiment, ſpices ſeem to hinder the production of air; but the preſent experiment proves the contrary. Whence this contrariety ſhould proceed, I know not; unleſs perhaps, becauſe I had left a ſpace large enough for the air in theſe receivers; but in the former experiment, filled all as exactly as I could with fleſh.

March 9. The height of the mercury in the firſt receiver was 8 inches, in the ſecond, none.

March 12. The height of the mercury in the firſt receiver was 12; in the ſecond, one.

April 3. The height of the mercury in the firſt receiver was 11; but in the ſecond it exceeded not 1. I opened the receiver, and boiling the fleſh, found it very tender, and of an excellent taſte.

Hence the ſaltneſs of water, included with fleſh, ſeems to hinder the production of air; but there being ſo ſmall a quantity of water, compared with the quantity of fleſh, I rather incline to think, that leſs air was produced in the ſecond receiver, becauſe it was more exactly filled. And indeed, freſh water being uſed inſtead of ſalt, has the ſame effect; but the chief art to preſerve fleſh without ſalt, conſiſts in excluding all air from it, and making a great compreſſure in the receiver.

Theſe experiments about the preſervation of aliment, may be very uſeful in tranſporting fruits, veniſon, *&c.* from remote places, and towards affording better nouriſhment to mariners.

Boiling and diſtillation practiſed in vacuo; *and firſt, beef boiled in an exhauſted receiver.*

(189.) *Decemb.* 12. 1678. I put two ounces and ſix drams of beef into an exhauſted receiver, able to hold 22 ounces of water; then I left it in boiling water for 3 hours; which done, I expoſed it to the air, to cool for a whole night; afterwards uſing my pneumatic engine, I perceived that the air formed in the receiver, could ſcarce ſuſtain 3 inches of mercury; whence fleſh in boiling, cannot form air enough to make an entire preſſure in a receiver, capable of holding a double weight of water; that is, if you include one pound of fleſh in an exhauſted receiver, able to hold two pounds of water, it will not generate air enough to remove the cover from the receiver, unleſs heat greatly contribute to produce the effect; but our fleſh, I confeſs, was not boiled enough.

(190.) *Decemb.* 23. I incloſed 3 ounces of raw beef in a receiver able to hold 32 ounces of water; and in boiling, after it had been long on the fire, the cover was forced from the receiver, and ſo ſuffered the vapours to paſs out; but being preſently ſhut again, and the fire removed, the receiver ſoon loſt its internal preſſure; ſo that being re-plac'd on the fire, it was a long time before it could force away the cover a ſecond time. I tried this again, and again; and unleſs the receiver had been expos'd to a very ſtrong fire, the cover would never have been removed; but if the fire burns well, ſweet exhalations continually paſs out.

Decemb. 24. The receiver having been cool'd, during the whole night, was, this day, by the uſe of the pneumatic engine, almoſt wholly evacuated. Whence we ſeem to have a confirmation, that the divulſion of the cover is not made by that air which can keep the form of air, but from the ſteams exhaling from the fleſh, and ſubſiding again therein; provided they be kept in, as they eaſily may, if we uſe not too fierce a fire to the evacuated receiver, whereby the loſs of thoſe ſweet vapours may be prevented.

Paſte boiled in vacuo, *and in common air.*

(191.) *Jan.* 21. 1679. I put paſte without leaven into an exhauſted receiver, and included another part of the ſame paſte in a ſecond receiver full of common air. I inclos'd theſe two receivers *in Balneo Mariæ*, ſtopt with a ſcrew; and when they had remain'd there for three hours, expoſed to a moderate fire, I opened the receivers: the paſte *in vacuo* I found reddiſh on the ſuperficies; but the other had admitted water; and the paſte was not boiled enough: and therefore, I put both receivers again *in Balneo Mariæ*, where they ſtaid a whole night.

Jan. 22. This morning I found the *Balneum Mariæ* quite cold; and the paſte, when taken out, was boil'd enough, but covered with no cruſt. That which I included *in vacuo*, was interſperſed with many cavities, but it ſeemed too inſipid; the other had no cavities, but a more pleaſant taſte. Both the receivers were found almoſt wholly empty'd of air.

Leavened paſte boiled in Balneo Mariæ, *after it had yielded its air* in vacuo.

(192.) *Feb.* 3. 1679. I incloſed leavened paſte *in vacuo*, and as ſoon as it had filled its receiver with factitious air, tranſmitted it into the receiver I uſed to boil fleſh *in Balneo Mariæ*; but when the paſte was thus remov'd, it pitch'd much; yet when it had remained for three hours in a hot *Balneum Mariæ*, the bread made of it was interſperſed with many cavities, but covered with no cruſt.

Feb. 5. I repeated the experiment, but now the paſte was included *in vacuo*, in the ſame receiver, which was afterwards put *in Balneo Mariæ*; and therefore, there was no need to remove the paſte, and expoſe it to the air. Hence the bread made thereof was much lighter than the former.

Roſemary and water diſtilled in vacuo.

(193.) *Feb.* 12. I included roſemary, with water, in the diſtilling veſſel; and when the air was pump'd out, I put the veſſel *in balneo Arenæ*, and there came over a water of a very ſweet ſmell, and ſome drops of eſſential oil of a very ſweet ſcent, and not empyreumatical. But when I opened the ſtop-cock to let in the air, the noiſe ſo ſoon ceaſed, that I judged much air was produced from the roſemary.

Feb. 13. I put the ſame roſemary into the ſame evacuated veſſel, and adminiſtred a more intenſe fire, yet could extract no oil, ſweet, or fetid; and the water was leſs fragrant than the former.

Fleſh boiled in vacuo.

(194.) *Feb.* 10. 1679. I boil'd one pound of fleſh *in vacuo*, in a veſſel deſcrib'd, which would contain almoſt four pounds of water: its upper part, which was made of glaſs, held the mercurial gage; by the help whereof, I perceiv'd, that the mercury aſcended not three inches, tho the fleſh had boiled for three hours, and more. It was not boiled enough, and its taſte was ungrateful: the liquor, form'd of the condens'd vapours, had alſo an unpleaſant taſte.

Feb. 11. I repeated the experiment, but now ſprinkled the fleſh with pepper and cloves: the mercury aſcended to the height of ſix inches, tho' the fleſh boil'd no longer than the other: it ſeemed very grateful to the palate: and the liquor, formed from the vapours, had a moſt pungent taſte of pepper; but contracted nothing ungrateful from the fleſh, as in the former experiment.

From theſe experiments made *in vacuo*, it ſeems, that ſuch veſſels may be very uſeful for diſtilling, and boiling of ſuch bodies, as contain thin, and very volatile ſpirits: for every thing will here be preſerved, and nothing be ſuffer'd to fly away.

Boiling in ſcrew'd veſſels or digeſtors; and firſt, beef and water boiled in Balneo Mariæ.

(195.) *Jan.* 29. Eight days ago, I filled a ſcrew veſſel, with beef and water together; and when it had continued over a moderate fire for eight or nine hours *in Balneo Mariæ*, ſtopt alſo with a ſcrew, I took the fleſh out; but it was boiled a great deal too much, and the taſte of it was very unpleaſant. I boiled other beef in the ſame veſſel, after the ſame manner; only this was ſeaſoned with pepper and cloves, and remained expoſed to the fire but for three hours. This fleſh preſerved a moſt pleaſant taſte. I boiled other fleſh without ſpices for three hours, in the ſame veſſel, and after the ſame manner: when the fleſh was taken out, it taſted well; whence I conjectur'd, that what ſpoiled the firſt fleſh, was over-boiling: yet the ſpices may be convenient to correct ſome part of the ungrateful taſte; for I left a place to condenſe the vapours, in the top of the veſſel, and found, that the liquor there formed, had an unpleaſant taſte; but not ſo when the fleſh was ſeaſon'd with pepper and cloves.

Apples boiled in a ſcrew-veſſel.

(196.) *Jan.* 29. I boil'd apples, after the ſame manner as I did the fleſh before-mention'd; but mix'd no water with them. They were ſet upon a moderate fire for almoſt two hours. They were very ſoft, and of a very good taſte; but ſome pieces, which lay in the upper part of the receiver, where the vapours aſcending from the lower part condenſed, were of an unpleaſant taſte; and the drops, form'd from the ſame vapours, had an ungrateful ſcent.

Fleſh ſeaſoned with ſpice, boiled in a ſcrew-veſſel.

(197.) *February* 4. I incloſed fleſh with pepper and cloves, in a receiver ſtopt with a ſcrew, but uſed no water to fill up the interſtices; only compreſſed the fleſh as much as I could, and then put the receiver *in Balneo Mariæ*, already hot, and ſtopt it with a ſcrew: when it had remained there, over a moderate fire, for an hour, the fleſh was rather over-boiled, than under; but when I opened the *Balneum Mariæ*, all the water burſt out of it with a great force; the liquor being hot, and now finding vent.

Feb. 5. I incloſed ſome part of this fleſh in a receiver, ſtopt with a ſcrew.

March 12. The fleſh included five weeks ago, was, this day, found very good. I do not doubt, but that perfect boiling contributed ſomething to its preſervation: for I find, by experiments made upon other bodies, that boiling, the more perfect it is, hinders fermentation the more.

A cow-heel boil'd till the bones were tender.

(198.) *February* 10. I boil'd a cow-heel, after the ſame manner as I had done the fleſh above-mention'd; but left it, for four hours, or more, upon a moderate fire: then the veſſels being unſtopt, we found the fleſh excellently well boiled, and the bones ſo ſoft, that they might be eaſily cut with a knife, and eaten.

Feb. 12. I repeated the experiment, and let the veſſels remain expoſed to the fire for twelve hours; and tho' the water of the *Balneum Mariæ* every where ſecured the veſſel immerſed in it, yet the fleſh had contracted a very empyreumatical taſte and ſmell; but the juice, which, in the former experiment, concreted into a very firm gelly, did not here congeal at all.

Hence it appears, that many bones, and hard tendons, which we daily throw away as unprofitable, may, by the help of a *Balneum Mariæ*, ſtopt with a ſcrew, be converted into good nouriſhment.

A fiſh boil'd in a ſcrew'd Balneum Mariæ.

(199.) *February* 10. I boil'd a fiſh after the ſame manner, in a ſcrew'd *Balneum Mariæ*, but mixed no water therewith. The fiſh remain'd upon the fire for two hours only; when the veſſel being cool'd and open'd, it was found of a very good taſte; and its bones were ſo ſoft, that they yielded to the preſſure of the finger; and the head of it might be eaten like its fleſh. The juice of it, in a ſhort time, concreted into a gelly of a hard conſiſtence.

This method is uſeful for boiling ſuch fiſh as are very bony.

Hart's-horn boil'd ſoft.

(200.) *February* 15. I put hart's-horn into a receiver, to be ſtopt with a ſcrew, and filled the interſtices with water; I included the receiver, thus ſtopt, in a ſcrew'd *Balneum Mariæ*, and ſo expoſed it for four hours, to a moderate fire: the veſſels being open'd, the hart's-horn was found ſoft, and the juice ſoon concreted into a very firm gelly.

Feb. 17. I repeated the experiment, but no water was included with the hart's-horn, and the fire laſted ſix hours under the *Balneum Mariæ*; after this, the hart's-horn was found very ſoft; but a little juice had ſweat out of it, and adhered to the external parts of the hart's-horn, like drops of gelly.

The excellency of ſuch a *Balneum Mariæ* appears from this experiment; for ſince even hart's-horn can be boil'd by means thereof, without water, all the freſh water, uſually conſumed in boiling fleſh at ſea, may be preſerved for other uſes.

A
DEFENCE
OF THE
Phyſico-Mechanical Experiments,
AGAINST
The Objections of FRANC. LINUS; his Hypotheſis examined, and his Anſwers to particular Experiments conſider'd.

The objections againſt the air's ſpring examined.

OUR author confeſſes, that the air hath both a ſpring and weight; but denies that ſpring to be great enough to perform what I aſcribe thereto; and, particularly, labours to prove it unable, in a cloſe place, to ſuſtain the mercury in the *Torricellian* experiment. For, ſays he, "if a tube, only twenty inches long, be not entirely fill'd with "quick-ſilver, but a ſmall ſpace be left betwixt it, and the finger that "cloſes the upper end, with nothing but air there; and the tube be opened at the bottom, the finger will not only be drawn downwards, but "the quick-ſilver will deſcend conſiderably; that is, as far as ſo ſmall a "parcel of air can be ſtretch'd by the deſcending weight; and therefore, "if, inſtead of air, any other liquor, not ſo eaſily extended, be here uſed, "the quick-ſilver will not fall: but if the external air cannot ſuſtain 20 "inches of mercury, how ſhould it ſupport 29 $\frac{1}{2}$?" But to this argument, he has himſelf furniſhed us with an anſwer in theſe words. "But, "you'll ſay that the mercury deſcends, becauſe 'tis impelled downwards, "by the air dilating itſelf by its own ſpring." Which I think ſufficient for the objection, notwithſtanding the two exceptions he makes to it.

For, firſt, when he ſays, that then "the finger ought rather to be repell'd "from, than fix'd to the tube, ſince the expanſion is made every way;" he conſiders not, that tho' the included air extends itſelf at firſt, every way, yet the expanſion, in our caſe, muſt neceſſarily be made downward; becauſe the finger that ſtops the tube, being expoſed on the upper parts, and the ſides, to the external air, has the whole weight and preſſure of the atmoſphere

ſphere upon it; and conſequently, cannot be thruſt away, but by a force, able to ſurmount that preſſure; whilſt on the lower ſide of the included air, there is the weight of the whole mercurical cylinder to aſſiſt the ſpring of the air to ſurmount the weight of the atmoſphere, that gravitates upon the ſtagnant mercury. So that the air included, endeavouring to expand itſelf, finding no reſiſtance upwards, and a conſiderable one downward, it is very natural, that it ſhould expand itſelf that way, where it finds the leaſt reſiſtance; as will happen, till the ſpring of the air be ſo far weaken'd by expanſion, that its preſſure, together with the weight of the mercury, that remains ſuſpended, will but balance the preſſure of the outward air upon the ſtagnant mercury. And, if inſtead of quick-ſilver, you employ water, and leave, as before, in the tube, an inch of air, and then inverting it, open it under water, the included inch of air will not dilate itſelf near half ſo far, as it did when the tube was almoſt fill'd with mercury; becauſe, the weight of ſo ſhort a cylinder of water does but equal that of between an inch and an inch and half of quick-ſilver; and conſequently the internal air is far leſs aſſiſted to dilate itſelf, and ſurmount the preſſure of the outward, by the cylinder of water, than by that of mercury.

As for what our author ſays, that, "if inſtead of air, or water, ſome "other liquor be left at the top of the tube, the quick-ſilver will not de-"ſcend;" we can readily ſolve that phenomenon, ſince water has either no ſpring at all, or but an exceeding weak one; and ſo ſcarce preſſes, but by its weight, which, in ſo ſhort a cylinder, is inconſiderable.

Hence we ſee, why the finger is ſo ſtrongly faſten'd to the upper orifice of the tube it ſtops: for the included air, being ſo far dilated, that an inch, for example, left at firſt, in the upper part, reaches twice or thrice as far, as before the deſcent of the quick-ſilver, its ſpring muſt be proportionably weaken'd; and conſequently, that part of the finger within the tube will ſuſtain much leſs preſſure upon it, from the dilated internal air, than the upper part of the ſame finger, from the unrarify'd air without. By which means, the pulp of the finger will be thruſt in.

Our author's ſecond objection runs thus. "If you take a tube, open at "both ends, of a conſiderable length, ſuppoſe forty inches, fill it with "mercury, place your finger on the top, and open the lower end; the "mercury will deſcend to its wonted ſtation, and your finger on the top, "be ſtrongly drawn within the tube, and ſtick cloſe to it. Whence again "it is evident, that the mercury at its own ſtation, is not there ſuſtain'd "by the external air, but by a certain internal cord, whoſe upper end, "being faſten'd to the finger, draws and fixes it, after this manner, in "the tube.

But this argument, being much of the ſame nature with the former, the anſwer made to that, may ſerve here alſo; eſpecially, becauſe in the preſent caſe, the pulp of the finger ſuſtains leſs preſſure on the inſide of the tube, than in the other; the preſſure of the atmoſphere being here kept off from it, by the ſubjacent mercury; whereas there is nothing of that preſſure abated againſt the other part of the finger, that kept it off from the deſerted cavity of the tube; only from the pulp, contiguous to the tube, there may be ſome taken off, by the weight of the glaſs itſelf. But as for that part of the finger, which immediately covers the orifice, whether there be any ſpring in its own fibres, or other conſtituent ſubſtance, which finding no reſiſtance in the place deſerted by the quickſilver, may contribute to its ſwelling; he, who duly conſiders the account already given of this intruſion, will find no need of our author's internal *Funiculus*, which ſeems more difficult to conceive, than to ſolve the phenomena, in controverſy, without it.

Our author propoſes this as a clear demonſtration; and it is, indeed, the principal thing in his book. "Take a tube of about 20 inches long, "with both ends open, let its orifice be immers'd in ſtagnant mercury, and "one finger being plac'd underneath, that the mercury to be pour'd in, "may not run thro', let it be fill'd with quick-ſilver, and then another "finger apply'd to cloſe its orifice. This done, if you take away the lower "finger, the upper will be ſtrongly drawn, and ſuck'd into the tube, and "adhere to it ſo firmly, that it will elevate the tube itſelf, with all the "quick-ſilver, and make it hang pendulous in the veſſel. Since then the "quick-ſilver in ſuch a tube muſt be thruſt upwards by the preponderating "air; it can never be hence explain'd, how the finger is ſo drawn down-"wards, and made ſo ſtrongly to adhere to the tube. For it cannot, by the "air forcing upwards, be drawn downwards." In anſwer hereto, I alledge that a good account may be given of this experiment, upon our hypotheſis, which is ſufficient to ſhew the argument not to be unanſwerable.

I deny then, that the finger is drawn downward or made, by ſuction, to adhere to the tube, otherwiſe than we have already explain'd.

He ſays indeed, that the air which thruſt up the quick-ſilver, cannot ſo ſtrongly draw down the finger: as if the air were not a fluid body, but a ſingle and entire pillar of ſome ſolid matter.

However, when the tube is fill'd with quick-ſilver, the finger that ſtops the upper orifice is almoſt equally preſs'd above, and at the ſides, by the contiguous air; and when the lower finger is remov'd, the cylinder of mercury, which before gravitated upon the finger, comes to gravitate upon the ſtagnant mercury, and by its intervention, preſſes againſt the outward air; ſo that, againſt thoſe parts of the finger, that are contiguous to the air, there is all the wonted preſſure of the external air; but againſt that pulp contiguous to the mercury, not ſo much preſſure, as againſt the other parts of the finger, by about two thirds; becauſe the mercurial cylinder, in this experiment, is ſuppos'd to be twenty inches high; and if it were but a little more than thirty inches high, the weight of the quick-ſilver would take off not two thirds only, but the whole preſſure of the outward air from the pulp of the finger. For in that caſe, the quick-ſilver would quite deſert it, and ſettle below it. Wherefore, ſince I have before ſhewn, that the preſſure of the outward air is taken off from the body that remains in the upper part of the tube, according to the weight of the liquor ſuſpended in it; and ſince, on our hypotheſis, the preſſure of the outward air is able to keep 30 inches of quick-ſilver, or 32, or 33 feet of water ſuſpended; it is no wonder if a preſſure of the ambient air, equal to the weight of a cylinder of water of near 22 feet long, ſhould be able to thruſt in the pulp of the finger, at the upper orifice of the tube, and make it ſtick cloſely to the top of it.

I know our author affirms, that no preſſure from without, can ever effect ſuch an adheſion of the finger to the tube; but this ſhould be proved. Nor could I, upon trial, find the adheſion of the finger to the tube, to be near ſo ſtrong as our author relates; but if you endeavour to thruſt the pulp of your finger into the orifice of the tube, you may thro' the glaſs, perceive it to be manifeſtly tumid in the cavity of the pipe. And if, by preſſing your finger againſt the orifice, you ſhould not make the pulp adhere quite ſo ſtrongly to the tube, nor ſwell quite ſo much within it, as may happen in ſome mercurial experiments; it is to be conſidered, that the air being a fluid, as well as a heavy body, does not preſs only againſt the upper part of the finger, but upon as much of it as is expoſed thereto, almoſt every way uniformly and ſtrongly; and ſo by its lateral preſſure, thruſts the pulp of the finger into the orifice, where there is leaſt reſiſtance.

Hence, we need not borrow the objection our author offers to lend, that in the experiment under conſideration, the quick-ſilver is preſſed downward by the ſpring of ſome air lurking betwixt it and the finger; (tho' ſuch a thing might eaſily happen) ſince we lately proved the contrary. And as for what he adds to confirm his argument, that "if the "preponderating air ſucceed in the place of the lower finger, which "was withdrawn; that is, if it ſuſtain the quick-ſilver after the ſame "manner, as by the lower finger apply'd under it; it is manifeſt, that "the finger on the top, ought to be no more drawn downwards, after "the lower finger is removed, than before: but experience teacheth the "contrary;" we muſt conſider, that the tube being ſuppoſed perfectly full of mercury, the finger which ſtops the lower orifice, is uſually kept ſtrongly preſſed againſt it, leſt any of that ponderous fluid ſhould get out; ſo that tho' the lower finger keeps up the mercury in the tube, and the preſſure of the outward air would do ſo too; yet there is this difference, that the preſſure of the atmoſphere, depending upon its weight, cannot be increaſed and weakened as we pleaſe, like the undermoſt finger. And therefore, whereas the atmoſpherical cylinder will not ſuſtain one of quick-ſilver above 30 inches high, thoſe who make the *Torricellian* experiment, often keep up with the finger a mercurial cylinder of perhaps 50 inches; ſo that in our caſe, before the removal of the under finger, the pulp of the uppermoſt muſt ſuſtain about the ſame preſſure, where it is contiguous to the mercury, as the other part of the ſame finger; after the removal of the under finger, there is as much preſſure of the atmoſphere taken off from the pulp, as balances a cylinder of quick-ſilver 20 inches high.

Our author's laſt experiment is thus propoſed: "This opinion is falſe, "becauſe thence it would follow, that quick-ſilver, thro' a like tube, "might be ſuck'd with the ſame eaſe out of a veſſel as water; which is "contrary to experience; for, according to this opinion, that the fluid "underneath, whether water or mercury, may ſo aſcend, no more is required, than that the air in the tube be drawn upwards by ſuction; when "the liquor below will immediately aſcend, being impelled by the external air, which now preponderates." But we formerly ſhew'd, that when the mercurial cylinder, which reſts upon the ſtagnant mercury, has, at the other end of it, air kept from any communication with the atmoſphere, that included air has ſo much of the preſſure of the external air taken off from it, as balances the mercurial cylinder. And the finger expoſed to the whole preſſure of the ambient air, in ſome of its parts, and in others but to the much fainter preſſure of the included air, ſuſtains an unuſual preſſure from the preponderating power of the atmoſphere. Thus the thorax, and the muſcles of the abdomen, which ſerve for reſpiration, ſuſtain the preſſure of the whole ambient air; tho' theſe muſcles are able, without any conſiderable reſiſtance, to dilate the thorax; becauſe as faſt as they open the cheſt, and by dilating it, weaken the ſpring of the included air, the external air ruſhing in, for want of the uſual reſiſtance there, keeps that within the thorax, in an equilibrium to that without. We ſay then, that if a cylinder of mercury be, by ſuction, raiſed in the tube to any conſiderable height, the preſſure of the air in the thorax, is leſſened by the whole weight of that mercurial cylinder; and conſequently, the reſpiratory muſcles are thereby diſabled from dilating the cheſt as freely as uſual. But if inſtead of mercury, you ſubſtitute water, ſo ſhort a cylinder of that takes off ſo little of the preſſure of the included air, that it comes into the lungs with almoſt its uſual ſtrength; and conſequently, with almoſt the ſame force wherewith the external air preſſes againſt the thorax.

And there is an experiment of M. *Paſchal*'s, which ſhews clearly, that if we could free the upper part of a tube from the preſſure of all internal air, the quick-ſilver (as our author ſays it ſhould) would, by the preſſure of the outward air, be impell'd up into the tube as well as water, till it had attain'd a height ſufficient to make its weight equal to that of the atmoſphere. The experiment itſelf is this: "If a glaſs ſyringe be made of a ſufficient "length, and after the ſucker is thruſt into the utmoſt orifice, it be plung'd "in the mercury, as ſoon as the ſucker is drawn out, the mercury follows, "and aſcends to the height of 2 feet, 3 inches and a half. And when afterwards, tho' no greater force be added, the ſucker is drawn higher, the "mercury ſtands, and follows no farther; whence that ſpace remains "empty which lies between the mercury and the ſucker." So that we may well explain our author's experiment, by ſaying, that in a more forcible reſpiration, the mercurial cylinder is raiſed higher than in a more languid one; becauſe in the former, the cheſt being more dilated, the included air is alſo more expanded, whereby its weaken'd ſpring cannot, as before, enable the mercurial cylinder to balance the preſſure of the ambient air. And the reaſon why the quick-ſilver is not, by reſpiration, rais'd as high as 'tis kept ſuſpended in the *Torricellian* experiment, is not the preſſure of the outward air being unable to raiſe it ſo high; but becauſe the free dilatation of the thorax, is oppoſed by the preſſure of the ambient air; which preſſure being againſt ſo great a ſuperficies, and but imperfectly reſiſted by the weakned ſpring of the air in the thorax, will be very conſiderable; ſince, in our engine, the preſſure of the external air againſt the ſucker of leſs than three inches diameter, was able to raiſe an hundred weight. And by the way, when we ſtrongly ſuck up quick-ſilver in a glaſs tube, tho' the elevation thereof proceeded from our author's *Funiculus*, contracting itſelf every way; and tho' there be a communication betwixt the internal ſurface of the lungs, and the cavity of the tube; yet we feel not, in our lungs, any endeavour of the ſhrinking cord to tear off that membrane they are lined with.

Our author further ſays, that "the ſpring of the air can perform neither more nor leſs, in a cloſe place, than its equilibrium in an open one." But I allow of this opinion, only in ſome caſes; for in others, we have performed much more by the ſpring of the air, which we can, within certain limits, increaſe at pleaſure, than can be perform'd by the bare weight, which for ought we know, remains always nearly the ſame. And of this difference, we formerly gave an inſtance; when by compreſſing the air, in the receiver, we impell'd the mercurial cylinder higher than the ſtation at which the balance of the air ſuſtains it.

Our author adds, that "ſince the experiments of the adheſion of the finger, *&c.* ſucceed alike in a cloſe and open place, the arguments produced againſt the equilibrium, make alſo againſt the ſpring of the air." This has already been anſwer'd; but ſince he ſays, that the experiments, concerning the adheſion of the finger, *&c.* ſucceed equally in a cloſe and open place, I wiſh he had told us what way he took to make them; for in ordinary rooms, there ſcarce ever wants a communication betwixt the internal and external air, by means whereof, the weight of the atmoſphere has its effect within the room.

The funicular hypotheſis examin'd.

Our author ſuppoſes, that what we aſcribe to the ſpring, and weight of the air, is performed by a ſort of *Funiculus*, conſiſting of a thin ſubſtance, greatly expanded; which lying between two bodies, endeavours to contract itſelf, and to bring theſe bodies together, to avoid a vacuum; by nature's abhorrence whereof he at length, ſolves all phenomena.

His firſt argument for this is, that the finger would not be drawn down, by the deſcent of the mercury in the *Torricellian* tube, were there not a *Funiculus*; and that, were no thin ſubſtance there extended, a vacuum muſt enſue.

But this argument being deduced from the ſuction of the pulp of the finger, upon the deſcent of the mercury, has been anſwer'd already. Another argument, which he alledges againſt a vacuum, is the tranſparency of that part of the tube, where 'tis ſaid to be: for, were there a vacuum, he ſays, it would be like a black pillar, neither able to afford any thing viſible, or to permit objects to appear thro' it.

But the invalidity of this aſſertion appears from the doctrine of the atomiſts, who teach, that light is made up of ſuch ſubtile effluvia, as are able to penetrate glaſs, and therefore, may leave many vacuities, tho' the cavity of the cylinder ſeems full of it; and no doubt, were the parts of the lucid matter contracted, they would not fill one tenth of that ſpace, ſince the ſmoke which fill'd our receiver, ſo as to make it appear opake, poſſeſs'd when condens'd, only a ſmall part thereof.

Thus a room may appear full of the ſmoke of a perfume, tho' if all the corpuſcles that compoſe the ſmoke were re-united, they would make up but a ſmall paſtil. A little camphire alſo, will fill a room with its odour; but having, in well clos'd glaſſes, caught the fumes of it driven over by heat, and again reduced them into true camphire, I found its bulk very inconſiderable, in compariſon of the ſpace it poſſeſſes, when its ſcented corpuſcles are ſcatter'd thro' the air.

I might add, that if the *Torricellian* experiment ſucceed in the dark, it may well be doubted, whether our author's argument will hold. For if he endeavours to prove, that the place in queſtion was full in the dark, becauſe upon letting in the light, a light appears within it; we may reply, that this light is a new one, flowing from the lucid body, that darts its corporeal rays thro' the glaſs and ſpace in diſpute, which for want of ſuch corpuſcles, were not, juſt before, viſible.

And ſuppoſing light to be made by a propagation of the impulſe of lucid bodies thro' tranſparent ones, yet it will not thence follow, that the deſerted part of the tube muſt be full; in one of our experiments, tho' many of thoſe groſs aerial particles, that appear neceſſary to convey a languid ſound, were ſoon drawn out of the receiver, yet there remain'd ſo many, that the others were not miſs'd, till a far greater number was extracted; and thus there may remain matter enough to tranſmit the impulſe of light, tho' betwixt the particles of that matter there ſhould be numberleſs vacuities; yet our author pretends to prove abſolutely, that there is no vacuity in the diſputed ſpace. And ſhould a *Carteſian* ſay, the deſerted part of the tube is filled with *Materia ſubtilis*, he muſt allow the preſſure of the outward air to be the cauſe of the ſuſpenſion of the quick-ſilver; for tho' the *Materia ſubtilis* may readily fill the ſpaces deſerted by the mercury; yet that within the tube cannot hinder ſo ponderous a liquor from ſubſiding as low as the ſtagnant mercury; ſince the whole tube, being pervious to that ſubtile matter, it may, with like facility ſucceed, in whatever part of it ſhall be forſaken by the quick-ſilver.

Our author's next argument is, that the mercurial cylinder, reſting at its wonted ſtation, does not gravitate; as appears by applying the finger to the immerſed, or lower orifice of the tube: whence he infers, that it muſt, of neceſſity, be ſuſpended from within the tube. And indeed, if the finger be applied to the open end of the tube, before 'tis quite lifted out of the ſtagnant mercury, the experiment will ſucceed;

the finger, however, will feel a gravitation, or preſſure, of the glaſs-tube, and the contained mercury, as of one body; but no ſenſible preſſure of the mercury apart, as if it endeavoured to thruſt away the finger from the tube. Now, according to our hypotheſis, the mercurial cylinder, and the air, balancing one another, the finger ſuſtains not any preſſure, ſenſibly differing from the ambient air, that preſſes againſt the nail, and ſides of it, and from the included quick-ſilver that preſſes againſt the pulp. But if the mercurial cylinder ſhould exceed the uſual length, then the finger would feel ſome preſſure from that additional quick-ſilver, which the air does not aſſiſt the finger to ſuſtain: ſo that this phenomenon may as well be ſolved on our hypotheſis, as on our author's. But how comes the mercury in the tube, when of a due altitude, to run out, upon removal of the finger beneath, if it be ſuſtained only by an internal cord; and, when that ſuſtains it, to reſemble a ſolid body, if the preſſure of the external air has no ſhare in it?

If it be here ſaid, that the finger muſt feel great pain, by being ſqueezed betwixt a pillar of thirty inches of quick-ſilver, and an equivalent preſſure from the atmoſphere, we muſt obſerve, that in fluids, a ſolid has not that ſenſe of preſſure from ſurrounding bodies which men are apt to imagine; as appears from divers: and I am inform'd, that the learned *Maignan*, tho' he purpoſely thruſt his hands, three or four palms deep, into quick-ſilver, his fingers were not ſenſible of any weight or preſſure.

Laſtly, our author tells us, that "thoſe remarkable vibrations the "quick-ſilver makes, in its deſcent, favours his hypotheſis." But this phenomenon, alſo, is eaſily ſolved on our hypotheſis: for when the experiment is made in a cloſe place, as our receiver is, mercury, by its ſudden deſcent, acquires an impetus, beſides the preſſure it has upon account of its gravity: whence it, for a while, falls below its ſtation, and thereby compreſſes the air that reſts upon the ſtagnant mercury; which air, by its own ſpring, again forcibly dilating itſelf, to recover its former extenſion, expands beyond it, and thereby impels up the quick-ſilver ſomewhat above its wonted ſtation; in its fall from whence, it again acquires a power to compreſs the air: and this reciprocation of preſſure, betwixt the quick-ſilver, and the external air, decreaſing by degrees, at length wholly ceaſes, as the mercury loſes that additional preſſure it acquired by falling from parts of the tube, higher than its due ſtation. But this way of explicating theſe vibrations, is not neceſſary in the free air; for if we conſider the atmoſphere only as a weight, and allow an impetus acquired by deſcent, the phenomenon will be eaſily explained by a balance, wherein one of the ſcales chancing to be depreſs'd, they do not, till after many vibrations, regain their equilibrium.

I took a glaſs ſiphon, whoſe two legs, unequal in length, were parallel, and both perpendicular to that part of the pipe which joined them; and poured quick-ſilver into it, till 'twas ſome inches high, and equal in both legs; then the ſiphon being inclined, till moſt part of the quick-ſilver was fallen into one of the legs, I ſtopped the orifice of the other with my finger: and, erecting the ſiphon again, tho' the quick-ſilver were forced to aſcend a little in that ſtopped leg; yet, becauſe my finger prevented the air from getting away, the quick-ſilver was kept much lower in the ſtopped leg than in the other: but if, by ſuddenly removing my finger, I gave vent to the included compreſs'd air, the preponderant quick-ſilver in the other leg would, with the mercury in the open one, make ſeveral undulations before, in both legs, it reſted in an equilibrium. Now, in this caſe, there is no pretence for a *Funiculus* of violently diſtended air, to cauſe the vibrations of the mercury.

But there are many particulars which render the funicular hypotheſis improbable.

And, firſt, our author acknowledges, that quick-ſilver, water, wine, &c. as well one as another, will deſcend in tubes, exactly ſeal'd at the top, in caſe the cylinder of liquor exceed the weight of a mercurial cylinder of twenty-nine inches and a half, but ſubſide no longer than till it is a balance to a cylinder of quick-ſilver of that height. Now it's very ſtrange, that whatever the liquor be, there ſhould be juſt the ſame weight or ſtrength to extend them into a *Funiculus*. And this is the more ſurprizing, becauſe our author makes ſo great a difference betwixt the diſpoſition of bodies of various conſiſtences to be extenuated into a *Funiculus*, that he will not allow any human force able to produce one by the divulſion of two flat marbles, in caſe the contact of their ſurfaces were ſo exquiſite, as quite to exclude all air; tho' his reaſoning plainly agrees with experience, that adhering marbles may be forcibly ſever'd; and therefore, according to him, the ſuperficial parts may be diſtended into a *Funiculus*, that prevents a *Vacuum*. But our hypotheſis labours not under this difficulty; for the weight of the external air, being that which keeps liquors ſuſpended in ſealed tubes, it matters not of what nature or texture the ſuſpended liquor is, provided its weight be the ſame with that of a mercurial cylinder equiponderant to the aerial one.

In the next place I obſerve, that the account our author gives of his *Funiculus*, is much more ſtrange than ſatisfactory, and not made out by any unqueſtionable parallel operations of nature: whereas, the weight and ſpring of the air may be inferr'd from ſuch certain experiments as are not concern'd in the preſent controverſy. For the gravity of the air may be manifeſted by a pair of ſcales; and its ſpring is diſclos'd ſo clearly in wind-guns, and other inſtruments, that our author does not deny it. But in the explanation of his *Funiculus*, he would have us remark two things; firſt, "that the quick-ſilver which fills the whole tube, doth not only touch the "top of it, but firmly ſtick to it; and that the finger adheres to the mercury; "ſince, tho' the orifice of the tube be oil'd, that will not hinder it from "ſticking as firmly as before." But two bodies, by truſion, may eaſily be made to ſtick together, as much as the tube and finger do, tho' one of them be oil'd; beſides, this adheſion of the finger to the tube will happen, not only when the ſurface of the included quick-ſilver is contiguous to the finger, but many inches below it. Water and quick-ſilver, he ſays, aſcend by ſuction, "becauſe the parts of the air included in the tube, are now ſo "firmly glued to one another, that they make a ſtrong chain, whereby "the water and quick-ſilver are drawn up." Which way of wreathing a little rarified air into ſo ſtrong a rope, is highly improbable.

Secondly, he ſays, that "the rarifaction, or extenſion of a body, ſo as "to make it take up more ſpace, is not only cauſed by heat, but by diſtenſion, or a certain disjoining power; as condenſation is not only "made by cold, but alſo by compreſſion." And it is true and obvious, that the condenſation of bodies (taking that word in a large ſenſe) may be made as well by compreſſion as cold. But I wiſh he had more clearly expreſſed what he means in this place by that rarifaction, which, he ſays, is to be made by a disjoining power; whereof, he tells us, there are innumerable inſtances. For, as far as may be gathered from the three examples he ſubjoins, it is only the air that is capable of being ſo extended, as his hypotheſis requires quick-ſilver, and even ſtones to be. And how will he prove that even air may be thus extended, to fill 2000 times the ſpace it poſſeſſed before? For that the ſame air adequately fills more ſpace at one time than another, he proves but by the ruſhing of water into the evacuated glaſs, and almoſt filling it; which, he ſays, is done by the diſtended air, that contracting itſelf, draws up the water with it. The explanation he gives of his *Funiculus* is this: "Since it is manifeſt, that "the quick-ſilver ſticks to the top of the tube, and that rarifaction is "made by the mere diſtenſion of a body, it happens that the deſcending "quick-ſilver leaves its upper ſuperficies fixed to the top of the tube; "and by its weight ſo ſtretches and extenuates it, till it becomes eaſier "to leave another ſuperficies in like manner, than to extend that any further. It leaves therefore a ſecond; and by its deſcent extends that a "little further, till it becomes eaſier to ſeparate a third, than to extend "that any more; and ſo on, till at length it hath no power to ſeparate, "or extend any more ſurfaces, when it comes to the height of 29 inches "and a half, where it reſts." Hence it is eaſy to diſcern, that he is obliged to aſſign his *Funiculus* a ſtrange and unparallel'd way of production. Now I muſt demand, by what force, upon the bare ſeparation of the quick-ſilver and the top of the tube, the new body he mentions, comes to be produced? or how it appears, that the mercury leaves any ſuch thing as he ſpeaks of behind it? For the ſenſe perceives nothing of it at the top of the tube, nor is it neceſſary to explain the phenomena, as we have formerly ſeen. And how ſhould the bare weight of the deſcending mercury be able to extend a ſurface into a body? Beſides, the ſucceſſion of ſurfaces is a chimera; or ſuppoſing ſome of the quick-ſilver were turned into a thin, ſubtile ſubſtance, yet how comes that ſubſtance to be contrived into a *Funiculus* of ſo ſtrange a nature, that ſcarce any weight can break it; and that, contrary to all other ſtrings, it may be ſtretched, without becoming more ſlender, and obtains other very odd properties?

Our author ſays indeed, that "theſe ſurfaces ſeem to be ſeparated from "the quick-ſilver, and to be extended into a moſt ſlender ſtring, by the

“ falling weight, after the ſame manner that in a lighted candle, ſurfaces of like ſort are ſeparated from the wax or tallow underneath, by the heat above, and extenuated into a moſt ſubtile flame; which doubtleſs takes up above 1000 times more ſpace, than the part of the wax, of which the flame conſiſted, poſſeſſed; ſo our *Funiculus* takes up 1000 times more ſpace than the ſmall particle of mercury from whence it aroſe.” And this is the only example whereby our author endeavours to illuſtrate the generation of his *Funiculus*. But here intervenes a conſpicuous and powerful agent, actual fire, to ſever and agitate the parts of the candle; and beſides, there is a manifeſt waſting of the wax or tallow turned into flame; and we muſt not admit that the fewel, when turned to a flame, really fills ſo much as twice the genuine ſpace as the wax it was made of. For the flame is little leſs than an aggregate of thoſe corpuſcles, which before lay upon the upper ſuperficies of a candle, and were, by the violent heat, divided into minuter particles, vehemently agitated, and brought from lying flat, to beat off one another, and make up, about the wieck, ſuch a figure as is uſual in the flame of candles burning in the free air. Nor will it neceſſarily follow, that the ſpace which the flame ſeems to take up, ſhould contain neither air, nor æther, or any thing beſides the parts of that flame, becauſe the eye can diſcern no other body there; for even the ſmoke aſcending from the ſnuff of a candle newly extinguiſhed, appears a dark pillar, tho’ there are many aerial, and other inviſible corpuſcles mixed with it; ſo that if all thoſe parts of ſmoke, which ſhew large in the air, were collected, and contiguous, they would not perhaps amount to the bigneſs of a pin’s head; as may appear from the great quantity of ſteams that in chymical veſſels go to make up one drop of ſpirit. And therefore, as our author, to enforce his former example, alledges the turning of a particle of quick-ſilver into vapour by fire; if ſuch be the rarifaction of mercury, it is not at all likely to make ſuch a *Funiculus* as he talks of; ſince thoſe mercurial fumes appear, by various experiments, to be mercury divided, and thrown out into minute parts; whereby, tho’ the body obtain more of ſurface than it had before, yet it really fills no more of true and genuine ſpace; ſince, if all the particular little parts filled by theſe ſcattered corpuſcles, were reduced into one, as the corpuſcles themſelves often are in chymical operations, they would amount but to one whole equal to that of the mercury before rarifaction.

I farther demand, how the *Funiculus* comes by hooks, or parts proper to take faſt hold of all contiguous bodies; and even the ſmootheſt, ſuch as glaſs, the calm ſurface of quick-ſilver, water, oil, *&c.* and how theſe ſlender and inviſible hooks find innumerable loops in ſmooth bodies, to take hold on ſo ſtrongly, as to lift up a tall cylinder of quick-ſilver, and draw inwards the ſides of ſtrong glaſſes ſo forcibly, as to break them to pieces? It is alſo ſomewhat ſtrange, that water, and other fluid bodies, ſhould, when the *Funiculus* once lays hold on their ſuperficial corpuſcles, preſently, like conſiſtent bodies, be drawn up in one entire continued piece; tho’, even in the exhauſted receiver, they appear by many ſigns, to continue fluid.

I know, that by calling this extenuated ſubſtance, a *Funiculus*, he intimates, that it has its ſpring inwards, like lute-ſtrings, and ropes forcibly ſtretch'd ; but there is no ſmall diſparity betwixt them : for in ſtrings, there is requir'd either wreathing, or ſome peculiar, and artificial texture of the component parts ; but a rarifaction of air, does not infer any ſuch contrivance of parts, as is requiſite to make bodies elaſtic. And ſince lute-ſtrings, *&c.* muſt, when they ſhrink inwards, either fill up or leſſen their pores, and increaſe in thickneſs, as they diminiſh in length ; our author's *Funiculus* differs widely from them ; ſince it has no pores to receive the ſhrinking parts ; and contracts its length, without increaſing its thickneſs. Nor does it, to me, ſeem very probable, that when, for inſtance, part of a poliſh'd marble is extended into a *Funiculus*, that *Funiculus* ſtrongly aſpires to turn into marble again. And 'tis very unlikely, that the ſpace, our author would have repleniſh'd with his funicular ſubſtance, ſhould be full of little, highly-ſtretch'd ſtrings, that lay faſt hold on the ſurfaces of all contiguous bodies, and always violently endeavour to pull them inwards. For, a pendulum being ſet a moving, in our exhauſted receiver, vibrated as freely, and with the ſtring as much ſtretch'd as in the common air. Nay, the balance of a watch did there move freely ; which is hard to conceive, if the moving bodies were to break thro' a medium conſiſting of innumerable ſtrings, exceedingly ſtretch'd. And 'tis ſtrange, if theſe ſtrings, thus cut, or broken, by the paſſage of theſe bodies thro' them, could ſo readily have their parts re-united, and immediately be made entire again. And in this caſe, the two divided parts of each ſmall ſtring, do not, like thoſe of other broken ſtrings, fly back from one another, but meet, and unite again ; yet when in the *Torricellian* experiment, the tube, with the contain'd mercury, is ſuddenly lifted out of the ſtagnant quick-ſilver into the air, the *Funiculus* ſo ſtrangely contracts itſelf, that it quite vaniſhes ; ſo that the aſcending mercury may riſe to the very top of the tube.

But this is not all that renders our author's hypotheſis improbable ; for it neceſſarily ſuppoſes ſuch a rarifaction, and condenſation, as is unintelligible.

We muſt here premiſe, that a body is commonly ſaid to be rarified, or dilated, when it acquires greater dimenſions than it had before ; and to be condenſed, when it is reduced to leſs dimenſions, that is, into a leſs ſpace ; and that there are three ways of explaining rarifaction : for, either we muſt ſay, that the corpuſcles whereof the rarified body conſiſts, depart from each other, ſo that no other ſubſtance comes in between them, to fill up the deſerted ſpaces, or, that theſe new interſtices, are but dilated pores, repleniſh'd, as thoſe of a tumid ſpunge by water, with ſome ſubtile ethereal ſubſtance ; or laſtly, that the ſame body does not only obtain a greater ſpace in rarifaction, and a leſſer in condenſation ; but adequately, and exactly fills it : and ſo, when rarified, acquires larger dimenſions,

menſions, without leaving any vacuities betwixt its component corpuſcles, or admitting any new, and extraneous ſubſtance between them.

'Tis to this laſt way of rarifaction, that our author has recourſe, in this hypotheſis; tho', I confeſs, it appears to me ſo difficult to be conceiv'd, that I doubt whether any phenomenon can be explain'd by it. Let us ſuppoſe, that in the *Magdeburg* experiment, he ſo often urges to prove his hypotheſis, that the undilated air, which, as he tells us, poſſeſs'd about half an inch of ſpace, conſiſted of 100 parts, 'twill not be deny'd, that as the aggregate is adequate to the whole ſpace it fills, ſo each of the 100 parts is likewiſe, adequately commenſurate to its reſpective ſpace, which is 100th part of the whole. Now, our author ſays, that " if a body poſſeſſes twice " as much ſpace, each part of that body muſt do the ſame." Whence the whole capacity of the ſphere, which according to him, was 2000 times bigger than the ſpace poſſeſs'd by the unexpanded air, there muſt likewiſe, be 2000 parts of ſpace, commenſurate each of them, to one of the aforeſaid 100th parts of air; and conſequently, when he affirms, that half an inch of air poſſeſs'd the whole cavity of the globe, if we will not admit, as he does not, either vacuities, or ſome ſubtile ſubſtance in the interſtices of the aerial particles, each part of air muſt, adequately, fill 2000 parts of ſpace. Now that this ſhould be reſolutely taught, to be really, and regularly done, in the *Magdeburg* experiment, will queſtionleſs, appear very abſurd to the *Carteſians*, and thoſe other philoſophers, who take extenſion to be but notionally different from body; and conſequently, impoſſible to be acquir'd, or loſt, without the addition, or detraction of matter: and will, I doubt not, appear ſtrange to every one who conſiders how generally extenſion is allow'd inſeparable, and immediately to flow from matter; and bodies to have a neceſſary relation to a commenſurate ſpace. Nor do I ſee, if one portion of air may be ſo eaſily brought exactly to fill a ſpace 2000 times as great as that it did but fill before, without the addition of any new ſubſtance, why the matter contain'd in each of theſe 2000 parts of ſpace may not be farther brought to fill 2000 more, and ſo on; ſince each of theſe newly repleniſh'd ſpaces, is preſum'd to be exactly fill'd with body, and no ſpace, and conſequently, that which the un-rarified air repleniſh'd, can be more than adequately full. And ſince, according to our author, not only fluids, but even ſolids, as marble, are capable of ſuch a diſtenſion, why may not the world be made many thouſand times bigger than it is, without either admitting a vacuity betwixt its parts, or being increas'd with the addition of one atom of new matter?

He further alledges, that the phenomena of rarifaction cannot be explain'd, either by vacuities, or the ſub-ingreſſion of an ethereal ſubſtance; and that there are two ways of explaining that kind of it, which he contends for.

After our author's objections againſt the two ways of rarifaction propoſed; the one by the vacuiſts, and the other by the *Carteſians*, who admit the moſt ſolid bodies, and even glaſs itſelf, to be pervious to an ethereal, or ſubtile matter, he attempts to explain the manner by which his own rarifaction is perform'd; and having premis'd, that the explanation of the way how each part of the rarified body becomes extended, depends upon the quality of the parts into which the body is ultimately reſolv'd; and, having truly obſerv'd, that they muſt, neceſſarily, be either really indiviſible, or endleſly diviſible, he endeavours to explain the *Ariſtotelian* rarifaction, according to theſe two hypotheſes. But tho' he thus propoſes two ways of making out his rarifaction, yet they are irreconcilable; and he ſpeaks of them very doubtfully, and obſcurely.

And firſt, having told us how rarifaction may be explain'd, if we admit bodies to be diviſible *in infinitum*, he makes an objection againſt the infinity of parts in a continuum, whereto he gives ſo dark an anſwer, that, I confeſs, I do not underſtand it.

And 'tis not clear to me, that even ſuch a diviſibility of a continuum, as is here ſuppoſed, would make out the rarifaction he contends for: ſince, let the integrant parts of a continuum be more or leſs finite, or infinite in number, ſtill each part, being a corporeal ſubſtance, muſt have ſome particle of ſpace commenſurate to it; and if the whole body be rarified, for inſtance, to twice its former magnitude, then will each part be, likewiſe, extended to double its former dimenſions; and fill both the place it took up before, and another equal to it; and conſequently, two places. I will not, however, pretend to affirm which of the two ways by atoms, or by parts infinitely diviſible, our author declares himſelf for: but whichſoever of them it be, I think he has not intelligibly made it out; as himſelf ſeems willing to confeſs. So that, in his diſcourſe of rarifaction, to which our author frequently refers, as that which ſhould make good what ſeems the moſt improbable, he has, inſtead of a probable hypotheſis, ſubſtituted a doctrine which himſelf dares not pretend capable of being well freed from the difficulties with which it may be charged.

As for the other way of explaining rarifaction, by ſuppoſing that a body is made up of parts indiviſible, he is, upon this hypotheſis, reduced to allow, that " one and the ſame part muſt be in two places adequately, " for ſince it is indiviſible, and takes up a greater ſpace than before, it " muſt, of neceſſity, be alſo in every point of that ſpace; or be virtually " extended thro' all that ſpace." When, therefore, he preſently after, affirms, that by this virtual extenſion of the parts, the difficulties which have for ſo many ages, perplex'd philoſophers, may be eaſily ſolved, he muſt give me leave to deſire he would explain what this *extenſio virtualis* is; and how it will remove the difficulties charged upon the *Ariſtotelian* rarifaction. For the eaſier conſideration of this matter, let us reſume what we lately ſuppos'd, that in the *Magdeburgic* experiment, the half-inch of undilated air, conſiſted of a hundred corpuſcles; I demand how the indiviſibility of theſe corpuſcles will qualify them to make out ſuch a rarifaction, as our author imagines? For, what does their being indiviſible, in this caſe, but make it the leſs intelligible, how they can fill above

100 parts of ſpace? He will anſwer, they are virtually extended. But, not here to queſtion, how this indiviſibility makes them capable of being ſo; I demand, whether by an atom's being virtually extended, its corporeal ſubſtance does really fill more ſpace than it did before, or not? If it do, then 'tis a true, and real, and not barely a virtual extenſion: but ſuch an extenſion, we have ſhewn, will not ſerve the turn; and our author ſeems to confeſs as much, by deviſing this virtual extenſion, to avoid the inconveniencies to which he ſaw his doctrine of rarifaction would otherwiſe be expoſed. But if it be ſaid, that when an atom is virtually extended, its corporeal ſubſtance fills no more ſpace than before; I demand, how that which is not a ſubſtance, can fill a ſpace; and how this improper, and only metaphorical extenſion, will ſolve the phenomena of rarifaction? As how the half inch of air, at the top of the fore-mention'd ſphere, ſhall without a corporeal extenſion, fill the whole cavity 2000 times its bigneſs, when the water is ſuck'd out of it, and act at the lower-part of the ſphere: For, our author teaches, that the whole globe was fill'd with a certain thin ſubſtance, which, by its contraction violently ſnatch'd up the water wherein the neck of the glaſs was immers'd. And in a parallel caſe, he makes it his grand argument, to prove, there is no vacuum in the deſerted part of the tube, in the *Torricellian* experiment, that the attraction of the finger cannot be but from ſome real body.

Our author's *Funiculus*, alſo ſuppoſes a condenſation, that to me, appears incumber'd with no leſs difficulties. For, ſince he teaches, that a body may be condens'd, without either having any vacuities for the compreſs'd parts to retire into; or, having its pores fill'd with any ſubtile, and yielding matter, that may be ſqueezed out of them; it follows, that the parts of a body to be condens'd, immediately touch each other; which ſuppoſed, I demand, how bodies, that are already contiguous, can be brought cloſer, without penetrating each other? So that I ſee not how this condenſation can be perform'd, without penetration of dimenſions. In the *Magdeburgic* experiment, he tells us, that the whole capacity of the globe is fill'd with an extremely rare body; which, according to him, intercepts neither pores, nor any heterogeneous ſubſtance. Now let us conſider, that before the admiſſion of water into the exhauſted globe, there was, according to him, 2000 half inches of a true and real body; and that, after the admiſſion of the water, there remain'd in the ſame globe, no more than one half inch of body beſides. Since then, our author does not pretend, that the 1999 half-inches of matter, that now appear no more, travers'd the body of water; and ſince he will not allow, that it gets away thro' the pores of the glaſs; I demand, what becomes of ſo great a quantity of matter: For that 'tis annihilated, I ſuppoſe, he is too rational to pretend; and to ſay, that ſo many parts of matter, ſhould be retir'd into that one part of ſpace that contains the half-inch of air, is little leſs incredible: for, that ſpace was ſuppos'd perfectly full of body before; and how a thing can be more than perfectly full, who can conceive? In ſhort, according to our author's way of condenſation, two, or perhaps, two thouſand bodies, may be crowded into a ſpace that is adequately filled with one of them apart. And, if this be not penetration of dimenſions, I deſire to be inform'd what is.

The preſſure and ſpring of the air confirm'd.

But as the hypotheſis I am oppoſing, is a kind of inverſion of ours; ſuppoſing the ſpring, or motion of reſtitution in the air, to tend inwards, as, according to us, it tends outwards, many of the phenomena would, if it were true, be plauſibly explicable by it; the ſame motions, in an intermediate body, being, in many caſes, producible alike, whether we ſuppoſe it to be thruſt, or drawn; provided, both the endeavours tend the ſame way. But then we may be ſatisfied, whether the effect be to be aſcribed to pulſion, or to traction, if we can find out an experiment, wherein there is a reaſon that ſuch an effect ſhould follow, in caſe pulſion be the cauſe enquired after; and not, in caſe it be traction. And ſuch an *experimentum crucis* is afforded us by M. *Paſchal*, who obſerv'd that the *Torricellian* experiment, being made at the foot, and in different parts of a very high mountain, after he had aſcended an hundred and fifty fathom, the quick-ſilver was fallen two inches and a quarter below its ſtation at the foot of the mountain; and that at the very top of the hill, it had deſcended above three inches below the ſame ſtation. Whence it appears, that the quick-ſilver being carried up towards the top of the atmoſphere, falls down the lower, proportionably to the height of the place wherein the obſervation is made: the reaſon of which, on our hypotheſis, is, that the nearer we come to the top of the atmoſphere, the ſhorter, and lighter is the cylinder of air, incumbent upon the ſtagnant mercury; and conſequently, the leſs weight of mercury will that air be able to balance, and keep ſuſpended. And, ſince this noble phenomenon, thus clearly follows upon ours, and not upon our author's hypotheſis, it ſeems to determine the controverſy; becauſe, in this caſe, it cannot be pretended, that the deſcent of the quick-ſilver, in the tube, is caus'd from the preternatural rarifaction, or diſtenſion of the external air, when, by trying to reſtore itſelf, it endeavours to draw up the ſtagnant mercury: for, there appears no ſuch forcible dilatation of that air, as in many of the phenomena of our engine, he is here pleaſed to imagine.

To this experiment he replies but two things; which, neither ſingly, nor together, will amount to a ſatisfactory anſwer.

And firſt, he queſtions the truth of the obſervation itſelf, becauſe, having made trial on a low hill, the event did no ways anſwer his expectation. But *Gaſſendus* relates, that the obſervation was five times repeated, with circumſtances, which ſufficiently argue the diligence wherewith the experiment was made: and I can confirm theſe obſervations, by two more made on hills in *England*. But, however the proportion of the deſcent of quick-ſilver may vary according to the different conſiſtence, and other accidents of the air, in the particular places, and times of the experiments being made; yet all obſervations agree in this, that nearer the top of the atmoſphere the quick-ſilver falls lower, than when further from it. And, in one of theſe experiments, a determinate quantity of air being left in the tube, before the mouth of it was open'd, under the ſtagnant mercury, and notice taken how low ſuch a quantity of that air depreſs'd the mercurial cylinder, 'twas obſerv'd, that at the mountain's foot, the included air was not able to depreſs the quickſilver ſo much. Whence we infer, that the cylinder of air, at the top of the hill, being ſhorter and lighter, did not ſo ſtrongly preſs againſt the included air, as did the ambient air at the bottom of the hill, where the aerial cylinder was longer and heavier.

We alſo attempted a trial, wherein we hoped to find a ſenſible difference in the weight of the atmoſphere, in a far leſs height, than that of an ordinary hill. But inſtead of a common tube, we made uſe of a weather-glaſs, and inſtead of quick-ſilver, employed common water in the pipe belonging to the glaſs; that ſmall changes in the weight or reſiſtance of the atmoſphere, in oppoſition to the included air, might be the more diſcernible.

Fig. 83. The inſtrument we made uſe of conſiſted only of a glaſs AB, with a broad foot, a narrow neck, and a ſlender glaſs-pipe CD, open at both ends; the pipe ſo placed, that the bottom of it almoſt reached to the bottom of the bigger glaſs AB, within whoſe neck A, it was faſten'd with a cloſe cement, that both kept the pipe in its place, and hinder'd all communication betwixt the inward air II, and the outward air KK, except by the cavity of the pipe CD. Now we choſe this glaſs AB, more than ordinarily capacious, that the effect of the dilatation of the included air II, might be the more conſpicuous. Then conveying a convenient quantity of water HD, into this glaſs, we carry'd it to the leads of the abbey-church at *Weſtminſter*, and there blew in a little air, to raiſe the water to the upper part of the pipe, that, being above the veſſel AB, we might the more preciſely mark the ſeveral ſtations of the water. Afterward, having ſuffer'd the glaſs to reſt a pretty while upon the leads, that the air II, within, might be reduced to the ſame ſtate with KK, that without; having mark'd the ſtation of the water F, we gently let down the veſſel by a ſtring to the foot of the wall, where one attended to receive it, who having ſuffer'd it to reſt upon the ground, told us, that it was ſubſided about an inch below the mark F; whereupon, having ordered him to put a mark at this ſecond ſtation of it E; we drew up the veſſel again, and ſuffering it to reſt a while, obſerv'd the water to be re-aſcended to the firſt mark F, which was, indeed, about an inch above E: and this we did a ſecond time, with almoſt a like ſucceſs; tho', two or three days after, the wind blowing ſtrongly upon the leads, we found not the experiment to ſucceed quite ſo regularly; yet the water, always, manifeſtly, fell lower at the foot of the wall, than at the top. But, to avoid miſtakes, and prevent objections, we made the experiment within the church, at the ſame height with the leads; but the upper part of the pipe being accidentally broken off, we order'd the matter ſo, that the ſurface G, of the remaining water in the pipe, ſhould be about an inch higher than the ſurface of that in the veſſel. And then, letting down the glaſs, I found, that almoſt as ſoon as it was ſettled upon the pavement, it was not only fallen as low as the other water, but the outward air depreſs'd it ſo far, as, whilſt I was looking on, to break in below the bottom of the pipe, and aſcend thro' the water in bubbles; after which, the glaſs being drawn up again, the water was very manifeſtly re-aſcended. Hence 'tis evident, that the atmoſphere gravitates more, *cæteris paribus*, near the ſurface of the earth, than in the more elevated parts of the air: for the leads on which we made our trials, were found, in perpendicular height, but 75 feet.

But for an experiment of the ſame kind, made at a greater height, take the following, communicated by Dr. *Power*.

On the 15th of *October*, 1661, we took a weather-glaſs AB, about two feet in length, and carrying it to the bottom of *Hallifax-hill*, the water ſtood in the ſhank at 13 inches above that in the veſſel: thence carrying it, thus fill'd, with the whole frame, immediately to the top of the ſaid hill, the water fell down to the point D; that is, an inch and a quarter lower than it was at the bottom of the ſaid hill; which proves the elaſticity of the air: for the internal air AC, which was of the ſame power and extenſion with the external, at the bottom of the hill, manifeſted a greater elaſticity, than the mountain-air there manifeſted preſſure; and ſo extended itſelf further by CD. *Fig.* 84.

The like experiment, I hear the ſame ingenious perſon has lately repeated, and found the deſcent of the water to be greater than before. And tho' ſome have thought it ſtrange, that, on a hill, far inferior to the *Alps* and *Appennines*, ſo ſhort a cylinder of water ſhould fall ſo much; yet I ſee not any reaſon to diſtruſt, upon this ground, either this experiment, or ours made at *Weſtminſter*; but rather wonder the water fell no more, if the hill be conſiderably high: for their ſuſpicion ſeems grounded on a miſtake; as if becauſe the quick-ſilver in the *Torricellian* experiment, made without purpoſely leaving any air in the tube, would not at the top of the mentioned hill, have ſubſided above an inch, the water, that is near 14 times lighter, ſhould not fall above a 14th part of that ſpace; whereas, in the *Torricellian* experiment, the upper-part of the tube has little or no air left in it, while the correſpondent part of the weather-glaſs contained air, whoſe preſſure was little leſs than that of the atmoſphere at the bottom of the hill; and conſequently, muſt be much greater, than the preſſure of the atmoſphere at the top of the hill.

Another particular, which confirms our hypotheſis, is that experiment made by the ſame M. *Paſchal*, by carrying a ſlack-blown foot-ball from the bottom to the top of an high mountain; for, the foot-ball gradually ſwell'd, the higher it was carry'd: ſo that at the top of the mountain it appear'd as if it were full-blown; and became gradually lank again, as it was carry'd downwards; ſo that at the foot of the hill, it was flaccid as before. We have here an experiment to prove our hypotheſis, wherein recourſe cannot be had to any body forcibly, and preternaturally diſtended, ſuch as is pretended to remain in the deſerted ſpace of the tube in the *Torricellian* experiment.

But further, our author's hypotheſis is needleſs; for he denies not that the air has ſome weight and ſpring, but affirms it very inſufficient to counterpoiſe a mercurial cylinder of 29 inches. We ſhall, therefore, now endeavour to manifeſt by experiments purpoſely made, that the ſpring of the air is capable of performing far more than is neceſſary to ſolve the phenomena of the *Torricellian* experiment. We took a long glaſs-tube, ſo bent at the bottom, that the part turned up, was almoſt parallel to the reſt of the tube; and the orifice of this ſhorter leg being hermetically ſealed, the length of it was divided into inches, each of which was ſub-divided into eighths, by a liſt of paper carefully paſted along it: then putting in as much quick-ſilver as filled the bended part of the ſiphon, that the mercury ſtanding in a level, might reach, in the one leg, to the bottom of the divided paper, and juſt to the ſame height in the other; we took care, by frequently inclining the tube, that the air, at laſt included in the ſhorter cylinder, ſhould be of the ſame laxity with the reſt of the air about it. This done, we began to pour quick-ſilver into the longer leg of the ſiphon; which, by its weight, preſſing upon that in the ſhorter, gradually ſtraitned the included air; and continuing to pour in quick-ſilver, till the air in the ſhorter leg was, by condenſation, reduced to take up but half the ſpace it poſſeſs'd before, we obſerv'd, in the longer leg of the glaſs, on which was likewiſe paſted a liſt of paper, divided into inches and parts, that the quick-ſilver was 29 inches higher than in the other. Hence we ſee, that as, according to our hypotheſis, the air, in that degree of denſity, and correſpondent meaſure of reſiſtance, whereto the weight of the incumbent atmoſphere reduces it, is able to balance and reſiſt the preſſure of a mercurial cylinder of about 29 inches; ſo, here, the ſame air, brought to a degree of denſity, about twice as great as it had before, obtains a ſpring twice as ſtrong; being able to ſuſtain, or reſiſt a cylinder of 29 inches in the longer tube, together with the weight of the atmoſpherical cylinder, that reſted upon thoſe 29 inches of mercury.

After ſome other trials, one of which we made in a tube, whoſe longer leg was perpendicular; and the other, that contained the air parallel to the horizon; we at laſt procured a tube, which, tho' large, was ſo long, that the cylinder, whereof the ſhorter leg of it conſiſted, admitted a liſt of paper divided into 12 inches, and their quarters; and the longer leg another, ſeveral feet in length, and divided after the ſame manner: then quick-ſilver being poured in, to fill up the bended part of the glaſs, that the ſurface of it, in either leg, might reſt in the ſame horizontal line; more quick-ſilver was pour'd into the longer tube: and notice being taken, how far the mercury roſe therein, when it appeared to have aſcended to any of the diviſions in the ſhorter; the ſeveral obſervations that were thus ſucceſſively made, and ſet down, afforded us the following table.

Fig. 85.

A TABLE of the Condenſation of the AIR.

A	A	B	C	D	E
48	12	00	Added to 29 $\frac{1}{8}$ makes	29 $\frac{2}{16}$	29 $\frac{2}{16}$
46	11 $\frac{1}{2}$	01 $\frac{7}{16}$		30 $\frac{9}{16}$	30 $\frac{6}{16}$
44	11	02 $\frac{13}{16}$		31 $\frac{15}{16}$	31 $\frac{12}{16}$
42	10 $\frac{1}{2}$	04 $\frac{6}{16}$		33 $\frac{8}{16}$	33 $\frac{1}{7}$
40	10	06 $\frac{3}{16}$		35 $\frac{5}{16}$	35
38	9 $\frac{1}{2}$	07 $\frac{14}{16}$		37	36 $\frac{15}{19}$
36	9	10 $\frac{2}{16}$		39 $\frac{5}{16}$	38 $\frac{7}{8}$
34	8 $\frac{1}{2}$	12 $\frac{8}{16}$		41 $\frac{10}{16}$	41 $\frac{2}{17}$
32	8	15 $\frac{1}{16}$		44 $\frac{3}{16}$	43 $\frac{11}{16}$
30	7 $\frac{1}{2}$	17 $\frac{15}{16}$		47 $\frac{1}{16}$	46 $\frac{3}{5}$
28	7	21 $\frac{3}{16}$		50 $\frac{5}{16}$	50
26	6 $\frac{1}{2}$	25 $\frac{3}{16}$		54 $\frac{5}{16}$	53 $\frac{10}{13}$
24	6	29 $\frac{11}{16}$		58 $\frac{13}{16}$	58 $\frac{2}{8}$
23	5 $\frac{3}{4}$	32 $\frac{3}{16}$		61 $\frac{5}{16}$	60 $\frac{18}{23}$
22	5 $\frac{1}{2}$	34 $\frac{15}{16}$		64 $\frac{1}{16}$	63 $\frac{0}{11}$
21	5 $\frac{1}{4}$	37 $\frac{15}{16}$		67 $\frac{1}{16}$	66 $\frac{4}{7}$
20	5	41 $\frac{9}{16}$		70 $\frac{11}{16}$	70
19	4 $\frac{3}{4}$	45		74 $\frac{2}{16}$	73 $\frac{11}{19}$
18	4 $\frac{1}{2}$	48 $\frac{12}{16}$		77 $\frac{14}{16}$	77 $\frac{2}{3}$
17	4 $\frac{1}{4}$	53 $\frac{12}{16}$		82 $\frac{12}{16}$	82 $\frac{4}{17}$
16	4	58 $\frac{2}{16}$		87 $\frac{14}{16}$	87 $\frac{3}{8}$
15	3 $\frac{3}{4}$	63 $\frac{15}{16}$		93 $\frac{1}{16}$	93 $\frac{1}{5}$
14	3 $\frac{1}{2}$	71 $\frac{5}{16}$		100 $\frac{7}{16}$	99 $\frac{6}{7}$
13	3 $\frac{1}{4}$	78 $\frac{11}{16}$		107 $\frac{13}{16}$	107 $\frac{7}{13}$
12	3	88 $\frac{7}{16}$		117 $\frac{9}{16}$	116 $\frac{4}{8}$

AA	The number of equal ſpaces in the ſhorter leg, containing the ſame parcel of air, differently expanded.
B	The height of the mercurial cylinder in the longer leg, that compreſs'd the air into thoſe dimenſions.
C	The height of a mercurial cylinder, that balanced the preſſure of the atmoſphere.
D	The aggregate of the two laſt columns B and C, exhibiting the preſſure ſuſtained by the included air.
E	What that preſſure ſhould be, ſuppoſing it in reciprocal proportion to the expanſion.

For the better underſtanding of this experiment, it is proper to obſerve the following particulars. 1. The tube being very tall, we were obliged to uſe it on a pair of ſtairs, which were very well illumined; and for preſervation, it was ſuſpended by ſtrings. 2. The lower, and bent part of the pipe, was placed in a ſquare wooden box, large and deep, to prevent the loſs of the quick-ſilver. 3. We were two, to make the obſervation together; the one to take notice at the bottom, how the quick-ſilver roſe in the ſhorter cylinder; and the other, to pour it in at the top of the longer. 4. The quick-ſilver was poured in but ſlowly, according to the direction of him who obſerved below. 5. At the beginning of the operation, that we might the more truly diſcern where the quick-ſilver reſted from time to time, we made uſe of a ſmall looking-glaſs, held in a convenient poſture, to reflect to the eye what we deſired to ſee. 6. When the air was crouded into leſs than a quarter of the ſpace it poſſeſſed before, we try'd whether the cold of a linen-cloth, dipt in water, would condenſe it; and it ſometimes ſeemed a little to ſhrink, but not ſo manifeſtly, that we dare build upon it. We then try'd likewiſe whether heat would dilate it; and approaching the flame of a candle to that part where the air was pent up, it had a more ſenſible operation than the cold before; ſo that we ſcarce doubted the expanſion of the air would, notwithſtanding the weight that oppreſſed it, have been made conſpicuous, if the fear of breaking the glaſs had not kept us from increaſing the heat.

This ſufficiently proves the principal thing for which I here alledge it; ſince it is evident, that as common air, when reduced to half its natural extent, obtained a ſpring about twice as forcible as it had before; ſo the air thus compreſſed, being farther crouded into half this narrow room, thereby obtained a ſpring as ſtrong again as that it laſt had, and conſequently four times as ſtrong as that of common air. And there is no cauſe to doubt, that if we had been furniſhed with a greater quantity of quick-ſilver, and a very ſtrong tube, we might, by a further compreſſion of the included air, have made it balance the preſſure of a far taller and heavier cylinder of mercury. For no man perhaps yet knows how near to an infinite compreſſure the air may be reduced by a force competently increaſed. So that here our author may plainly ſee, the ſpring of the air can reſiſt, not only the weight of 29 inches, but in ſome caſes above 100 inches of quick-ſilver; and this without the aſſiſtance of his *Funiculus*, which, in our preſent caſe, has no pretence to be employ'd. And to ſhew that the weight of the incumbent atmoſphere, made a part of the weight reſiſted by the impriſon'd air; when the mercurial cylinder, in the longer leg of the pipe, was about 100 inches high, we cauſed a man to ſuck at the open orifice, whereupon the mercury in the tube conſiderably aſcended; which phenomenon cannot be aſcribed to our author's *Funiculus*; ſince, by his own confeſſion, that cannot pull up a mercurial cylinder of above 29 or 30 inches. And therefore the preſſure of the atmoſphere being in part taken off, by expanding itſelf into the man's dilated cheſt, the impriſoned air was thereby enabled manifeſtly to dilate and repel the mercury that compreſſed it, till there was an equality of force betwixt the ſtrong ſpring of the compreſſed air on the one part, and the tall mercurial cylinder, with the contiguous dilated air, on the other.

Now, if to what we have delivered concerning the compreſſure of the air, we add ſome obſervations of its ſpontaneous expanſion, it will the better appear, how much the phenomena of theſe mercurial experiments depend upon the different meaſures of ſtrength to be met with in the air's ſpring, according to its various degrees of compreſſion and laxity.

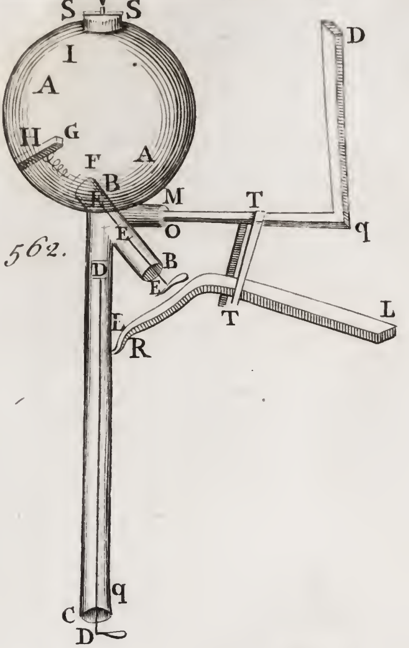


Fig. 80. p. 562.

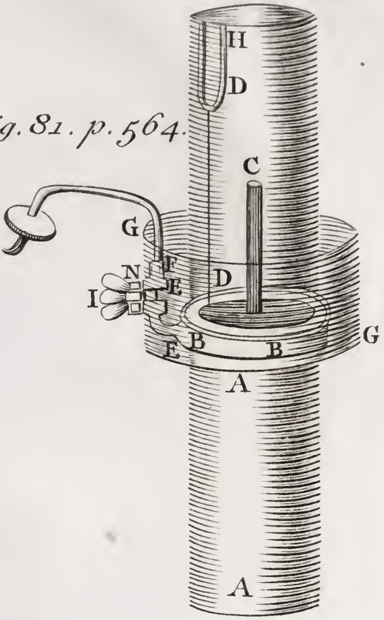


Fig. 81. p. 564.

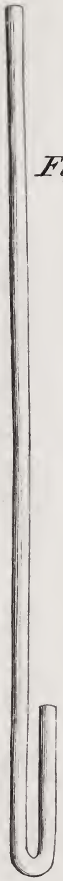
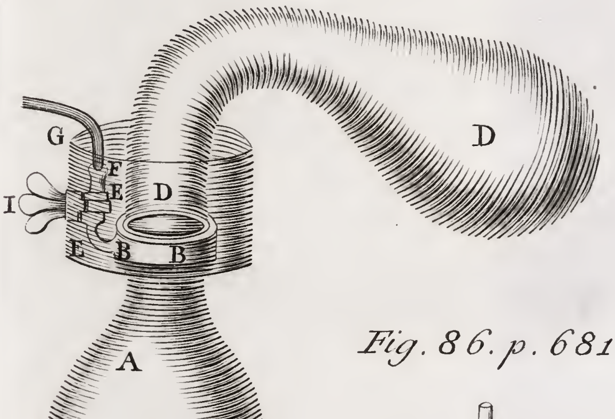


Fig. 85 p. 670.

Fig. 82. p. 565.



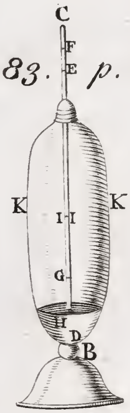
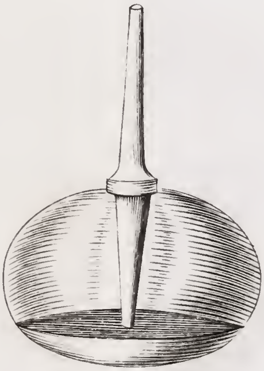


Fig. 83. p. 668.

Fig. 86. p. 681.



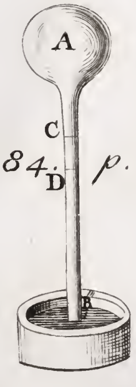


Fig. 84. p. 669.

J. Mynde.

A TABLE of the Rarifaction of the AIR.

A	B	C	D	E
1	00	Subſtracted from $29\frac{3}{4}$ leaves	$29\frac{3}{4}$	$29\frac{3}{4}$
$1\frac{1}{2}$	$10\frac{5}{8}$		$19\frac{1}{8}$	$19\frac{5}{6}$
2	$15\frac{3}{8}$		$14\frac{3}{8}$	$14\frac{7}{8}$
3	$20\frac{2}{8}$		$9\frac{4}{8}$	$9\frac{15}{12}$
4	$22\frac{5}{8}$		$7\frac{1}{8}$	$7\frac{7}{16}$
5	$24\frac{1}{8}$		$5\frac{5}{8}$	$5\frac{19}{20}$
6	$24\frac{7}{8}$		$4\frac{7}{8}$	$4\frac{23}{24}$
7	$25\frac{4}{8}$		$4\frac{7}{8}$	$4\frac{1}{4}$
8	26		$3\frac{6}{8}$	$3\frac{23}{32}$
9	$26\frac{3}{8}$		$3\frac{3}{8}$	$3\frac{11}{36}$
10	$26\frac{6}{8}$		3	$3\frac{39}{40}$
12	$27\frac{1}{8}$		$2\frac{5}{8}$	$2\frac{23}{48}$
14	$27\frac{4}{8}$		$2\frac{2}{8}$	$2\frac{1}{8}$
16	$27\frac{6}{8}$		2	$1\frac{55}{64}$
18	$27\frac{7}{8}$		$1\frac{7}{8}$	$1\frac{47}{72}$
20	28		$1\frac{6}{8}$	$1\frac{9}{80}$
24	$28\frac{2}{8}$		$1\frac{4}{8}$	$1\frac{23}{96}$
28	$28\frac{3}{8}$		$1\frac{3}{8}$	$1\frac{1}{16}$
32	$28\frac{4}{8}$		$1\frac{7}{8}$	$0\frac{119}{128}$

A The number of equal ſpaces at the top of the tube, that contained the ſame parcel of air.

B The height of the mercurial cylinder, that together with the ſpring of the included air, balanced the preſſure of the atmoſphere.

C The preſſure of the atmoſphere.

D The complement of B to C, exhibiting the preſſure ſuſtained by the included air.

E What the preſſure ſhould be, according to the hypotheſis.

* To make the experiment of the debilitated force of expanded air the plainer, we muſt mention ſome particulars, eſpecially with relation to the manner of performing it. 1. We made it on a light pair of ſtairs, and with a box lin'd with paper to receive the mercury that might be ſpilt; and in a glaſs tube about ſix feet long, hermetically ſealed at one end. 2. We alſo provided a ſlender glaſs pipe about the bigneſs of a ſwan's quill, and open at both ends, all along which was paſted a narrow liſt of paper, divided into inches and half quarters. 3. This ſlender pipe being thruſt into the greater tube, almoſt filled with quick-ſilver, the glaſs helped to make it ſwell to the top of the tube; and the quick-ſilver getting in at the lower orifice of the pipe, filled it up till the mercury included in that, was near upon a level with the ſurface of the ſurrounding mercury in the tube. 4. There being little more than an inch of the ſlender pipe left above the ſurface of the ſtagnant mercury, and conſequently unfill'd therewith, the prominent orifice was carefully cloſed with melted ſealing-wax; after which the pipe was let alone for a while, that the air, dilated by the heat of the wax, might, upon refrigeration, be reduc'd to its wonted denſity. And then we obſerved, by help of the liſt of paper, whether we had included more or leſs than an inch of air, and in either caſe, we rectified

the error, by a ſmall hole made with a heated pin in the wax, and afterwards cloſed it up again. 5. Having thus included a juſt inch of air, we lifted up the ſlender pipe by degrees, till the air was dilated to an inch, an inch and a half, two inches, *&c.* and obſerved, in inches and eighths, the length of the mercurial cylinder, which at each degree of the air's expanſion was impelled above the ſurface of the ſtagnant mercury. 6. The obſervations being ended, we preſently made the *Torricellian* experiment with the above-mentioned large tube, ſix feet long, that we might know the height of the mercurial cylinder for that particular day and hour, which we found to be 29 inches and three quarters. 7. Our obſervations, made after this manner, furniſhed us with the preceding table, in which there would not probably have been found the difference here ſet down betwixt the force of the air, when expanded to double its former dimenſions, and what that force ſhould have been preciſely, according to the theory, but that the included inch of air received ſome little acceſſion during the trial; which this difference cauſing us to ſuſpect, we found, by plunging the pipe again into the quick-ſilver, that the included air had gained about half an eighth; which we gueſs'd to have come from ſome little aerial bubbles in the quick-ſilver contained in the pipe.

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* "The open air, in which we breathe," ſays Sir *Iſaac Newton,* "is 8 or 900 times "lighter than water, and by conſequence "8 or 900 times rarer. And ſince the air "is compreſſed by the weight of the incumbent atmoſphere, and the denſity of "the air is proportionable to the compreſſing force, it follows, by computation, "that at the height of about ſeven *Engliſh* "miles from the earth, the air is four times "rarer than at the ſurface of the earth; and "at the height of 14 miles it is 16 times "rarer than at the ſurface of the earth; and "at the height of 21, 28, or 35 miles, it "is reſpectively 64, 256, or 1024 times rarer, or thereabouts; and at the height "of 70, 140, and 210 miles, it is about "1.000000, 1.000000.000000, or 1.000000. "000000. 000000." &c. *Newton. Optic.* p. 341, 342.

Here we find, that the inch of air, when firſt included, ſuſtained no other preſſure than from the incumbent air, and was no more compreſſed than the reſt of the air we breath'd and mov'd in; that this inch of air, when expanded to twice its former dimenſions, was able, with the help of a mercurial cylinder of about 15 inches, to counterpoiſe the weight of the atmoſphere; and that this was impelled up into the pipe by the external air gravitating upon the ſtagnant mercury, which alſo ſuſtained above 28 inches of mercury, when the internal air had its ſpring too far weakened to make any conſiderable reſiſtance; from whence it is plain, that the free air here below is almoſt as ſtrongly compreſſed by the weight of the incumbent atmoſphere, as it would be by the weight of a mercurial cylinder 28 or 30 inches high; and conſequently is not in ſuch a ſtate of laxity as men uſually imagine; but acts like ſome mechanical agent, with a force decreaſing, in a ſtricter proportion to its increaſe of dimenſion, than has been hitherto taken notice of.

And hence at length we ſee, that our author's hypotheſis is unneceſſary to ſolve the phenomena in diſpute; which is no ſmall acquiſition, ſince the two principal things that induced him to reject our hypotheſis, are, nature's abhorrence of a vacuum; and that, tho' the air have ſome weight and ſpring, yet theſe are inſufficient to make out the known phenomena; for which we muſt therefore have recourſe to his *Funiculus*. But he has not diſproved a vacuum, yet we have manifeſted, that the ſpring of the air may perform greater things, than what our explanation of the *Torricellian* experiments, and thoſe of our engine, require.

We come now to the laſt part of our defence, wherein we are to conſider what our author objects to ſome particular experiments.

Particular pneumatical experiments defended.

Againſt our firſt experiment he objects nothing, but that, by applying the finger to the orifice of the valve, when the pump is freed from air, the ſucker will not appear to be thruſt inward by the external air, but as the finger, to be drawn inwards, by the internal. But this phenomenon has been formerly accounted for upon our hypotheſis.

Of our third experiment he ſays, that "it very well agrees with his "principles; for ſince by this depreſſion of the ſucker, the air in the cavity of the cylinder, is ſeparated from the cylinder, and deſcends, together with the ſucker, in that whole depreſſion, new ſurfaces are taken from that deſcending air, and ſtretched out, as in the caſe of deſcending water. Since therefore ſuch ſurfaces are as eaſily ſlipt off, and "extended at the end of the depreſſion as at the beginning, it is no "wonder there ſhould be the ſame difficulty of depreſſing in both caſes." By which he ſeems to intend an oppoſition to a part of the third experiment, which I oppoſed not againſt his opinion; yet he offers nothing at all to invalidate my inference; but inſtead of that, propoſes a defence of his own opinion, which ſuppoſes the truth of his hypotheſis; and is unſatisfactory, even according to that, or elſe diſagrees with what himſelf hath taught us but a little before. For it is evident, that the more the ſucker is depreſſed, the more the cylinder is exhauſted of air. And ſpeaking of the air in the receiver, he affirms, that "it is the "more extended and rarified, the more is drawn out; and therefore "acquires the greater force to contract itſelf." Tho' here he would have us believe, that the little internal air in the cavity of the ſhank of the ſtop-cock, as ſtrongly attracts the ſucker, or reſiſts its depreſſure, when the ſucker is near the top of the cylinder, as when being forced down to the lower part thereof, the ſame portion of remaining air muſt be exceedingly more diſtended.

To the fourth experiment our author objects nothing, but endeavours to explain it his own way, whereto he ſays this circumſtance excellently agrees, that upon the return of the external air into the receiver, the tumid bladder immediately ſhrinks; becauſe the air in the receiver, which drew the ſides of the bladder outward, from the middle of it, is hereby relaxed; which explication, whether it be more natural than ours, let any one judge, who has conſider'd what we have alledg'd againſt the *Funiculus*.

To the breaking a glaſs receiver, not of a globular figure, by exhauſting moſt of the internal air, whereby its diminiſh'd preſſure became unable to reſiſt that of the outward air, our author confidently ſays, "it ſeems "incredible, that the moſt ſoft air ſhould ſo vehemently compreſs ſuch a "glaſs, on all ſides, as to break it." As if it were more credible, that the air within, ſhould be able to act more powerfully upon the glaſs, than that without, which himſelf confeſſes to be a heavy body; and which not only reaches from the ſurface of the earth, to the top of the higheſt mountains, but may, for ought we know to the contrary, be heaped upon the receiver, to the height of ſome hundreds of miles.

After a recital of the ninth experiment, he propoſes his objection to it thus. "But this ſeems far removed from truth, becauſe, if the preſſure "of the air, which deſcends by that tube into the vial, be ſo great as to "break the vial itſelf, it ſhould certainly firſt very much move the water, "in which the tube is immerſed, excite bubbles in it, *&c.* yet it is certain, "that the water, before the vial breaks, doth not move at all." But for all this, I think our explanation true; for we put the water into the vial that was broken, upon a particular deſign; and in the ſecond trial, the water was omitted. But notwithſtanding this water, the ſides of the glaſs being expoſed to the preſſure of the atmoſphere, wholly ſuſtained it, before the exhauſtion of the receiver; ſo that there needed no ſuch blowing in of the air afreſh, as our author imagines, to break the vial; it being ſufficient for that purpoſe, that the preſſure againſt the convex ſuperficies of it, was taken off, by exhauſting the receiver; the preſſure againſt the concave ſuperficies, remaining as great as ever. And therefore we need not altogether deny what he ſays, that "tho' the tube had been cloſed "at the top, the vial would ſtill have broke." For ſince, in ſuch caſes, the air is ſhut up, with the whole preſſure of the atmoſphere upon it, this may almoſt as eaſily break the glaſs, as if it were unſtopt; and accordingly we mention the breaking of a thin glaſs, hermetically ſealed, upon the extraction of the ambient air. But as confidently as our author ſpeaks, ſuch thin vials are ſubject, upon withdrawing the ambient air, to ſtretch a little; whereby the ſpring of the included air, may, in ſome caſes, be ſo far weakened, as not to be able to break them, unleſs aſſiſted by the preſſure of the atmoſphere; and when the vial actually begins to break, the enſuing preſſure of the outward air, upon that within the vial, may help to throw the parts of the glaſs more forcibly aſunder.

The author having recited our conjecture, as to the reaſon why two flat ſmooth marbles ſtick ſo cloſely together, approves my way of examining that conjecture. But I ſay, tho' the marbles were kept together, by the preſſure of the ambient air, yet they did not fall aſunder in our exhauſted receiver, becauſe of ſome ſmall leak in the receiver; yet he tells us, with his uſual confidence, that this very experiment ſufficiently ſhews, that opinion falſe. But poſſibly he would have ſpoken leſs reſolutely, if he had made all the trials, about the adheſion of marbles, that I have. For he ſpeaks, as if all that we aſcribe to the air, in ſuch experiments, were to ſuſtain the lower marble, with the weight perhaps of a few ounces; whereas, if the air be wholly kept from getting between the ſtones, it may ſuſtain a weight equal to that of a pillar of air, as broad as the baſis of the lower marble, and as high as the atmoſphere; or to the weight of a column of quick-ſilver, of the ſame thickneſs, and about 30 inches long. And therefore, ſince when we had exhauſted our receiver, as far as we could, there remained air enough to ſuſtain in the tube, a cylinder of quick-ſilver an inch high; and ſince the broader the contiguous marbles are, the greater weight faſtened to the lowermoſt, may be ſuſtained by the reſiſtance of the air; it is no wonder, that the air remaining in the receiver, ſhould ſupport the lowermoſt marble, whoſe diameter was near two inches, and a weight of four ounces; thoſe two weights being inferior to that of a mercurial cylinder of the ſame diameter, and an inch in length; and tho' they were not, yet perhaps the receiver was leſs emptied when we made the 31ſt experiment, than when we made the 17th. And it was with the ſame pair of marbles that, before an illuſtrious aſſembly, the uppermoſt drew up the lowermoſt, tho' clogg'd with a weight of above 430 ounces.

As for the account our author gives of this phenomenon, few, I believe, will acquieſce in it; for, not to inſiſt upon the objection which himſelf takes notice of, that, according to him, the diſtended air in the receiver, ſhould draw the adhering marbles aſunder, his explanation ſuppoſes, that there cannot naturally be a vacuum; whence he infers, that "the ſtone "could not deſcend, but by leaving ſuch a thin ſubſtance behind it, as "happens in the deſcent of quick-ſilver, or water." He adds, that the adheſion in our caſe proves obſtinate, becauſe ſuch a ſubſtance is far more difficult to be ſeparated from marble, than from quick-ſilver, or any other kind of body; but this aſſertion is precarious. And tho' I have made numerous experiments with ſtones of ſeveral ſizes, yet I could never find that by their coheſion, they would ſuſtain a weight greater than that of a pillar of the atmoſphere, that preſſed againſt the loweſt; which is a conſiderable circumſtance, that much better agrees with our explanation than with his.

Of the ſudden extinction of animals included in our receiver, which I aſcribe to the exceſſive thinneſs of the air therein, he ſays, "it ſeems im"poſſible they ſhould die ſo ſoon, merely thro' want of a thick air;" but gives no other reaſon, than the ſuddenneſs of the effect; which too ſeems grounded upon a miſtake; for the creatures he mentions, were a bee, a fly, and a caterpillar; and thoſe included in a ſmall receiver, which could be ſuddenly exhauſted; and theſe indeed became moveleſs within a minute. And tho' theſe inſects did in ſo ſhort a time grow moveleſs, yet they were not ſo ſoon kill'd, as appears by the narrative. The ſanguineous animals, that did indeed die, were kill'd more ſlowly. And having purpoſely enquired of a diver, how long he could, before he was accuſtomed to dive, remain without breathing, or the uſe of a ſpunge; he told me, that at firſt he could hold out about two or three minutes at a time; which made me think, that divers become able to continue under water ſo long, either by a peculiar conſtitution of body, or a gradual exerciſe. And I am apt to think, that, as it is uſual, he hereby meant a much ſhorter time than, when exactly meaſured, it amounts to. For having made trial upon two live moles, one of them included in a ſmall receiver, was between two and three minutes in killing; whereas the other being detained under water, did not there continue full a minute and a quarter, before it finally ceaſed from giving any ſign of life. Hence it is not impoſſible, that the want of reſpiration ſhould diſpatch an animal in as little time as is mentioned in the experiment. And indeed, our author ſhould either have proved it impoſſible, for the want of air to deſtroy animals ſo ſoon; or have given us ſome better account of the phenomenon.

It were a needleſs taſk to examine any more of our author's objections to particular experiments, ſince they wholly proceed upon the ſuppoſition of his *Funiculus*; which has been ſufficiently proved a chimera; whereas the ſpring, as well as the weight of the air, is not only allowed by himſelf, but demonſtrable by experiments uncontroverted betwixt us.

Mr. *HOBBS*'s
Phyſical Dialogue,
ABOUT THE
Nature of the AIR,
EXAMIN'D,
With relation to the Phyſico-mechanical Experiments of the Spring and Effects of the AIR.

The weight and ſpring of the air aſſerted.

MR. *Hobbs*, in diſputing againſt me, ſeems generally to have miſapprehended my notion of the air. For when I ſay the air has gravity, and an elaſtic power; or, that the air is, in great part, pump'd out of the receiver; it is plain that I take the air, in the obvious ſenſe, for part of the atmoſphere, which we breathe, and wherein we move; nor do I find that any other of my readers underſtand me otherwiſe. But Mr. *Hobbs* thinks he has ſufficiently confuted me, if in ſome caſes he proves, that there is a ſubtile ſubſtance, or æther, in ſome places, which I take not to be filled with air; and that the æther has, or wants ſome properties, which I deny, or aſcribe to the air; but I do not deny that the atmoſphere, or fluid body, which ſurrounds the terraqueous globe, may, beſides the groſſer, and more ſolid corpuſcles, wherewith it abounds, conſiſt of a thinner matter, which, for diſtinction ſake, I alſo agree to call æthereal.

But he does not, that I remember, deny the truth of any of the matters of fact I have delivered; nor attempt to prove, that the explanations I have given of my experiments, are contradictory to the doctrine I advance; but rather rejects our two grand hypotheſes, the weight and ſpring of the air. It will here therefore ſuffice to prove, what he is unwilling to grant.

And firſt, that the air, in my ſenſe of the word, is not deſtitute of weight, we have ſhewn by various experiments; one of them is, that a blown bladder, carefully weigh'd in an exact pair of ſcales, was found manifeſtly heavier when full of air, than when empty of air.

Secondly, It has alſo been obſerved, that an æolipile being well heated, and the little orifice left at the top of the pipe, being ſtopt whilſt it was thus hot; upon opening of this hole, when the æolipile was grown cold again, the external air ruſhing in at the fore-mentioned orifice, cauſed the æolipile to weigh ſo much more than it did juſt before the external air got in, as amounted to near 1000th part of the weight of an equal bulk of water.

Thirdly, In the *Magdeburgic* experiment, the great receiver they were to exhauſt, being weigh'd both before and after the extraction of the air, they found the difference to be one ounce $\frac{3}{10}$, "which," ſays the learned *Schottus*, "is a very clear demonſtration of the gravity of the air."

Fourthly, We have weigh'd the air ſhut up in bodies, in our exhauſted receiver, wherein of two materials, different in nature, a blown bladder, and a glaſs bubble, each equi-ponderant to a more ſolid weight, before the air was pumped out, we found that which included a large quantity manifeſtly to preponderate after the exhauſtion. To theſe we might add other proofs to the ſame purpoſe; but theſe afford ſuch a variety of caſes, that it would be ſuperfluous.

Let us now ſee what Mr. *Hobbs* objects againſt the experiment of the bladder weighed in the exhauſted receiver; for he quarrels not with the reſt. "That the ſcale which contains the bladder, is more depreſſed than "the other, they may be certain by ſight; but that this proceeds from "the natural gravity of the air, they cannot be certain, eſpecially if they "know not the efficient cauſe of gravity." But can we not be ſure, that lead is, *in ſpecie*, heavier than cork, unleſs we know the efficient cauſe of gravity? The reaſon he gives why the bladder outweighs, is this: "That "the bladder, whether blown up with a pair of bellows, or with human "breath, is heavier than when flaccid, I will not deny, becauſe of the "greater quantity of atoms, or of fuliginous corpuſcles; but there is "nothing certain in this experiment. They ought to have put into the "ſcales two veſſels of equal weight, whereof one ſhould be ſhut, and the "other open; for thus air, not blown in, but barely incloſed, had been "weighed; when therefore air ſhall be ſo weighed, we will afterwards "conſider what may be ſaid to the phenomenon." The firſt part of this paſſage does not deny the gravity of that we call air, but only endeavours to ſhew what parts they are that make it heavy. And as to the ſecond, he ſeems to miſtake the preſent caſe; for there is no need that the air in the bladder, before the exhauſtion of the receiver, ſhould be heavier than the outward air. Wherefore when he ſubjoins, that from this experiment we can deduce nothing certain, the affirmation is precarious. And Mr. *Hobbs* might eaſily have perceived, that we did make a trial much of the ſame nature with that he deſires; for we weigh'd the air in a glaſs, hermetically ſealed, wherein it was not blown in, but barely included. And ſince, in his elements of philoſophy, he grants, and gives his reaſon for it, that "if air be blown into a hollow cylinder, or into a bladder, it will increaſe "the weight of either of them a little;" and ſince here he likewiſe confeſſes, that there are mixed with the æther, many aqueous and earthy particles; he confeſſes, that the air is not deſtitute of weight; and it concerns us no more than himſelf, to ſhew how the corpuſcles, upon whoſe account the air is heavy, make it ſo.

This is all which Mr. *Hobbs*, in ſeveral places, thinks fit to object againſt the gravity of the air; leaving the experiment of the æolipile, and ſome others, unanſwer'd; which alone prove the air has a manifeſt weight, even when uncompreſs'd, and in its laxity. Let us now examine whether the air has not alſo a ſpring. This, tho' he calls it (as he likewiſe does the weight of the air) a dream, yet himſelf in effect grants all that is requiſite to prove the ſpring of the air. For delivering that known experiment, wherein the air is compreſſed in a glaſs bottle, by the forcible injection of water, which, when the glaſs is unſtopt, the air again throws out, in recovering its former dimenſions; he ſays, that "the air, with which the "ſpherical glaſs was filled, being moved by earthy corpuſcles, in a ſimple, circular motion; and being compreſſed by the force of the injection, that of it, which is pure, gets out into the open air, and gives "place to the water. It follows, that theſe earthy corpuſcles have leſs "ſpace left, wherein to exerciſe their natural motion; therefore beating "one upon another, they force the water to fly out, when the external air "penetrates it, and ſucceſſively takes up the place of the evacuated air, "till the corpuſcles of the ſame quantity of air being reſtored, regain a "liberty natural to their motion." But to paſs by ſeveral other of his conceſſions to this purpoſe, we can prove the ſpring of the air by many phenomena of our engine, of which he offers no other explanation. *Fig.* 86.

If the *Torricellian* experiment be made in a tube, between two feet and a half, and 3 feet in length; and if, when the mercury reſts at its wonted ſtation, you dextrouſly ſtop the orifice of the tube with your finger, that orifice being raiſed as near the ſurface of the ſtagnant mercury as poſſible, without admitting the external air; if then you quite lift up the tube, thus ſtopt, into the free air, you ſhall feel upon your finger little or no preſſure from the weight of the mercurial cylinder, diſtinct from the weight of the tube; becauſe the gravity of the quick-ſilver is balanced by that of the outward air, which thruſts your finger againſt it; but if you invert the tube, and having let in the air at the orifice, ſtop it again with your finger, and again let the mercurial cylinder reſt upon that finger, you will find your finger ſtrongly preſs'd, and ready to be thruſt away; which new preſſure, ſince it cannot come from the mercury, nor from the weight of the admitted air, to what can we rationally aſcribe it, but to the ſpring of the included air? And the force hereof will be as well manifeſt to the eye, as the finger, if the tube be unſtopt under the ſurface of the ſtagnant mercury; for then that in the glaſs will not reſt, as before, at the uſual ſtation, but be depreſſed far beneath it. And if you make the *Torricellian* experiment in a ſhort open tube, ſtopt above and below with your fingers, upon unſtopping the upper orifice, a new and forcible preſſure will be felt upon the finger that ſtops the lower orifice, made by the gravitation of the external air, which was before kept from reſting upon the mercurial cylinder by the upper finger, the pulp of which, by that gravitating air, was before thruſt into the deſerted cavity of the tube; which demonſtrates both the ſpring of the air, and the gravity of the atmoſphere.

But to the experiment of the ſwelling and ſhrinking of a bladder, hung in our receiver, as the ambient air is withdrawn, and ſuffered to return; Mr. *Hobbs* replies, "that every ſkin is made up of ſmall threads, or fila-" ments, which, by reaſon of their figures, cannot exactly touch in all " points. The bladder therefore being a ſkin, muſt be pervious, not " only to air, but to water alſo; whence there is the ſame compreſſure " within the bladder as without. The endeavour of which (the way of its " motions being every way croſs) tends to the concave ſuperficies of the " bladder; wherefore it muſt of neceſſity ſwell every way, and the vehe" mency of the endeavour increaſing, be at laſt torn." But if this be a ſufficient anſwer to ſuch an experiment, I fear it will be harder than we are yet aware of, to prove any thing by experiments. For firſt, how improbable is it, that ſuch bladders as we uſed, are readily pervious to the air; when eaſy experience ſhews us, that by leiſurely compreſſing ſuch blown bladders betwixt our hands, we ſhall rather break them, than ſqueeze out the air at their pores? So that the reſt of his anſwer being built upon what is ſo repugnant to common experience, will not need a particular confutation; however, we ſhew, that by the exhauſtion of the air, even a glaſs, hermetically ſealed, was broke; and to ſay, that glaſs alſo is pervious to the air, were to affirm what the greateſt part of his book ſuppoſes to be falſe. Beſides, there is not any ſenſible and unqueſtionable phenomenon, to prove that the receiver is full of any ſuch air as he ſpeaks of; for we ſee plainly, that when the air manifeſtly gets into the receiver, the bladder is not thereby made to ſwell, but to ſhrink. Moreover, according to Mr. *Hobbs*, the bladder is pervious to the air; and the air within the receiver is univerſally compreſſed, as well that which is within the bladder, as that which is without it; how then comes the air, that bears againſt the convex ſurface of the bladder, not to reſiſt that which is contiguous to the concave ſuperficies of the ſame; at leaſt, how comes the bladder to be broken by the air, which, according to Mr. *Hobbs*, can get in and out at pleaſure? And laſtly, to ſhew, that to the ſwelling of the bladder, there needs nothing but the ſpring of the included air; and no ſuch vehement agitation of the ambient air, as he ſuppoſes in our engine; it appears by the experiment of M. *Paſchal*, that in the free and ordinary air, a foot-ball, half blown, will gradually ſwell, the nearer it is carried ed to the top of an high mountain, where the incumbent cylinder of the atmoſphere is ſhorter, and its weight the leſs; and will for the contrary reaſon, grow more flaccid, the nearer it approaches to the foot of the mountain.

Mr. Hobbs's *explanation of the phenomena of the air-pump examin'd.*

Mr. *Hobbs* attempts to explain the phenomena of our engine, by ſuppoſing, that "many earthy particles are interſpers'd with the air, which "have a ſimple, circular motion, congenite to their nature; and that there "is a greater quantity of theſe particles in the air, near the earth, than "remote from it." But this aſſumption, to me, ſeems very precarious; for I know no unqueſtionable example or experiment, whereby it can be made out, that any ſmall parcel of matter has ſuch a ſimple, circular motion, as he aſcribes to each of theſe innumerable earthy, and as he adds, aqueous particles. The only argument he here brings to prove, that each atom would have this motion, if all the reſt of the earth were annihilated, does not ſeem clear to me. For it is not always true, that each minute part of a homogeneous body, has, in every reſpect, the ſame qualites with the whole; as the roundneſs which a ſmall drop of water, or quick-ſilver, is obſerved to have upon a dry plain, is not to be met with in a large portion of either of theſe fluids, tho' plac'd upon the ſame plain. And Mr. *Hobbs*, as well as we, makes the terreſtrial atoms in the air to have gravity; a quality that does not properly belong to the whole globe of the earth: nor is it manifeſt why, becauſe the terreſtrial globe moves in a vaſt circle about the ſun, each particular atom of it muſt deſcribe a ſmall circle in the air, about I know not what center. And tho' he aſſerts, that the air, near the earth, abounds with ſuch terreſtrial corpuſcles, 'tis not likely they ſhould have ſuch a regular motion, as he attributes to them; but ſtriking againſt one another, they muſt, in probability, be put into almoſt as various, and confuſed a motion, as *Des Cartes* aſcribes to his terreſtrial particles, ſwimming in the atmoſphere.

Mr. *Hobbs* farther endeavours to prove, that by the exhauſtion of our cylinder, no vacuum is produc'd; and gives a very different account of the experiment itſelf: he ſays, that "while the ſucker is drawn back, the more "ſpace is left within, the leſs is left to the external air; which being "thruſt backwards, by the motion of the ſucker, towards the outer-"moſt parts, moves, in like manner, the contiguous air; and that the "next; and ſo forwards: ſo that of neceſſity, at laſt, the air muſt be "compell'd into the ſpace deſerted by the ſucker, and enter between the "convex ſurface of the ſucker, and the concave of the cylinder. For, "the parts of the air being infinitely ſubtile, muſt inſinuate themſelves "that way by which the ſucker is drawn down; ſince the contact of "thoſe ſurfaces cannot be perfect in all points, becauſe the ſurfaces them-"ſelves cannot be made infinitely ſmooth: and then that force, which is "applied to draw back the ſucker, in ſome meaſure diſtends the cavity of "the cylinder; and if, betwixt the two ſurfaces, one ſingle hard atom "ſhould enter, pure air will enter by the ſame way, tho' with a ſmall "force. And thus air, for the ſame reaſon, inſinuates itſelf through

“ the valve of the cylinder ; and, therefore, the retraction of the ſucker will not prove a vacuum. It follows, alſo, that the air, which is driven up into the ſpace deſerted by the ſucker, becauſe it is forcibly impell'd, has a very ſwift and circular motion, betwixt the top and the bottom of the cylinder ; becauſe there is nothing there to weaken its motion ; and nothing can give motion to itſelf, or diminiſh it.” But, many exceptions may be made to this reaſoning. And firſt, I know not why Mr. *Hobbs* ſhould here confine his diſcourſe to the pump, without taking notice of the glaſs it is deſign'd to evacuate. We will, therefore, conſider how we can account for the exhauſtion of the receiver, as well as of the cylinder, ſince we uſually employ them both together. And he being obliged to explain the exhauſtion of the one as well as the other, it will be convenient to take into conſideration the receiver, becauſe that being of glaſs, and tranſparent, we can better ſee what happens in it, than in the opake cylinder. This premis'd, I do not clearly perceive, by this explanation, how he avoids a vacuum ; for according to his firſt words, the external air is diſplac'd by the motion of the ſucker outward, and this diſplac'd air muſt move that which is next to it ; and that the next, and ſo onward, till at length, the air muſt be compell'd into the ſpace deſerted by the ſucker : ſo that till this returning air get in betwixt the ſucker and the cylinder, how appears it, from this diſcourſe, that the deſerted ſpace was not empty for ſome little time ? Certainly all theſe motions of the air, forward and backward, could not be perform'd in an inſtant ; as may appear by the motion of ſounds and echoes, whoſe velocity is reducible to meaſure. Secondly, tho' he takes his adverſaries to be vacuiſts, yet he here ſuppoſes the plenitude of the world. I wiſh, thirdly, that Mr. *Hobbs* had declar'd, from whence the return of the air's impulſe would begin ; for that may well be required from one, who making the world full, and, for ought appears, fluid, allows us to believe it infinite, if the magiſtrate ſhall enjoin us that belief. Fourthly, I demand, what neceſſity there is for ſo forcible a return of the impulſe, as is requiſite to thruſt in the air at ſo narrow a paſſage as that between the ſucker and cylinder ? For, why may not that impulſe, when diffuſed in the vaſt ambient medium, be ſo communicated, and blended among the different motions of the other parts of it, as not to return again from whence it begun ? As a voice, tho' ſtrong, will not move the air, beyond a certain diſtance, ſmartly enough to be reflected in an echo, to the ſpeaker ; and as a ſtone caſt into a lake, will have the waves, it makes, diverted from returning to the place they began at. Fifthly, I do not likewiſe ſee, that 'tis probable, what Mr. *Hobbs* affirms of ſo thick a cylinder as ours, that it ſhould be diſtended by depreſſing the ſucker. But this I inſiſt not on ; the principal thing, peculiar in Mr. *Hobbs*'s explanation, is, that as much air as is driven away by the ſucker, preſently gets in again, betwixt that and the cylinder. But, by the air thus ſuppos'd to get in, he either means in the uſual ſenſe, and in ours, the common air, ſuch as we live and breathe in ; or, he does not.

If he ſpeaks of ſuch air, I can plainly prove, by ſeveral experiments, that our engine is, in great part, deſtitute of it. For, firſt, if there be a contrivance made, whereby the whole pump may be covered with water, we may, as we have try'd, plainly ſee the air that is drawn out of the receiver, at each reciprocation of the ſucker, paſs in great bubbles out of the valve thro' the water.

Next, it appears by the *Magdeburgic* experiment, that, by reaſon of the receſs of the air, the globe of glaſs, whence it went out, was diminiſh'd in weight, above an ounce. Thirdly, the ſame truth may be prov'd by the experiments formerly mention'd, of the ſwelling of the bladder, and the breaking of an hermetically ſeal'd glaſs, upon the receſs of the ambient air; theſe experiments having been already vindicated from Mr. *Hobbs*'s very improbable ſolutions. Fourthly, the ſame may be prov'd by the breaking of weak, or ill-figur'd receivers, inwards; of which, on our hypotheſis, the reaſon is clear; but not on his. And, fifthly, what I contend for, may be ſufficiently proved from this one phenomenon; that tho', if the receiver being full of common air, the key be turn'd under water, the water will not at all aſcend to the open orifice; yet the like being done, after the exhauſtion of the receiver, we have had ſeveral gallons of water violently impell'd into the cavity of the glaſs: which could not happen, if it were full of air, both in regard there can be no probable cauſe aſſign'd why the water ſhould be thus ſpurted up; and becauſe the receiver being already full of air, either two bodies muſt be contain'd in one place, and ſo we muſt allow penetration of dimenſions; or elſe common air, to which glaſs is impervious, muſt paſs thro' the water; which, we conclude, it does not, becauſe no ſuch bubbles are made in the external water, as would appear, if common air paſs'd thro' it. Nay, ſo little of this common air was, ſometimes, left in the globe uſed at *Magdeburg*, that when the water was ſuffer'd to ruſh in, it reduced the air into leſs than the thouſandth part of the capacity of the globe; and even if our receiver be unſtopped, not under water, but in the open air, the ambient air will, violently, preſs in, with a great noiſe, durable enough to argue, that the glaſs was far from being full of ſuch air before.

And thus we may argue againſt Mr. *Hobbs*, if he would have the engine, when we call it exhauſted, fill'd with common air; as his words ſeem to intimate. But becauſe, by ſome other paſſages of this dialogue, he may be favourably thought to mean, that the pure air is that which gets in by the ſides of the ſucker, into the pump, and ſo into the receiver, let us conſider his explanation in this ſenſe alſo. I deſire it may be obſerv'd, that if Mr. *Hobbs* takes the air in this ſecond ſenſe, he does not oppoſe what I have deliver'd; the air, I pretend to be pump'd out of the receiver, being the common air, which conſiſts, in great part, of groſſer corpuſcles than the æthereal ſubſtance. Yet, even this explanation will be liable to the two firſt inconveniencies, lately objected againſt the other, in favour of the vacuiſts; and to ſeveral objections beſides. I obſerve again, that tho' the pump be kept all the while under water, yet the exhauſtion of the cylinder, and receiver, will proceed as well as in the open air. I demand, then, how the pure air gets in by the ſides of the ſucker, immers'd in water? I preſume, for want of a more plauſible anſwer, Mr. *Hobbs* will here ſay, that the air paſſes thro' the body of the water, to fill up the deſerted ſpace, that muſt, otherwiſe, be void. But then I appeal to any rational man, whether I am obliged to believe ſo unlikely a thing, upon a bare affirmation; for he does not ſo much as pretend, by any phenomenon, to countenance this aſſertion: and there are phenomena that make againſt it. Many experiments ſhew us, that when air paſſes thro' water, it makes bubbles there, which, in our caſe, do not appear. Beſides, why ſhould not the outward air rather impel the water, as we ſee it frequently does, than be ſuppos'd to dive ſo ſtrangely and imperceptibly thro' it? When, alſo, the throughly exhauſted receiver is unſtopp'd, under water, he, who obſerves how the water ruſhes in with a ſtream, as big as the paſſage admits, will hardly imagine, that, at the ſame time, as much air as water can paſs thro' the ſame orifice unperceiv'd. But, it may be ſaid, in Mr. *Hobbs*'s behalf, that either his explanation, or a vacuum, muſt be admitted. To which I reply, firſt, that he has not evinced there can be no vacuum. Next, that we have made it probable, that, by his explanation, he does not avoid the neceſſity of a vacuum. And, thirdly, that a pleniſt, having recourſe to Mr. *Hobbs*'s precarious diving of the air, may, more probably, decline the neceſſity of yielding a vacuum, by ſaying, that the æther is, by the impulſe of the depreſs'd ſucker, and the reſiſtance of the ambient bodies, ſqueez'd thro' the pores of the glaſs, or cylinder, into the cavity of the veſſel, as faſt as room is there made for it. And, I confeſs, I wonder that Mr. *Hobbs* ſhould be ſo averſe to this way of ſolving the objection, ſince he ſuppoſes the parts of the air to be infinitely ſubtile; which, if they are, no pores can be too narrow to admit them. But, to preſs this no farther, I muſt here take notice, that whether the cavity of the receiver be reſolved to be empty, or full of Mr. *Hobbs*'s æthereal body, or the *Carteſian* celeſtial matter; the violent ruſhing in of the water, when the veſſel is unſtopt under that liquor; with ſeveral other phenomena, which cannot be aſcrib'd to the ſubtile matter within; ſufficiently argue, that there is, in the external air, a far greater power of preſſing inwards, than there is within of reſiſting; and conſequently, ſuch a weight, or ſpring in that air, as we plead for.

Mr. *Hobbs*, too, will have the air, impell'd by the ſucker, to move very ſwiftly betwixt the top and bottom of it; as alſo, when it gets into the cavity of the receiver; yet, when a light bladder is ſuſpended in the cavity of the receiver, it betrays no ſuch motion: nay, the flame of a taper was not blown out, nor ſtirr'd by this ſuppoſed wind; and ſmoke, produced in the exhauſted receiver, was not, by this vehement motion of the air, blown about the receiver. But, if the common external air be admitted at the ſtop-cock, that, indeed, will ruſh in with noiſe and violence, and whirl about the bladder, which hung quietly before.

In explaining the *Torricellian* experiment, he ſpends many words to prove, that the place deſerted by the ſuſpended mercury is full of air. But this expoſition ſuppoſes a plenum: and if he takes the air in the common ſenſe of the word, 'tis manifeſtly repugnant to ſeveral phenomena; as that, if the experiment be carefully made, we may, by inclining the tube, impel the mercury from its wonted ſtation to the top; which will not happen, in caſe the air were, before inclination, let into the deſerted ſpace; that if, when the mercury is ſettled at its uſual ſtation, the tube be lifted up out of the ſtagnant quick-ſilver, the outward air will drive up the heavy mercurial cylinder, oftentimes, with force enough to beat out the ſeal'd end; and, laſtly, the quick-ſilver reſting at its ſtandard height, if you carefully ſtop the lower orifice, under the ſurface of the ſtagnant quick-ſilver, and then lifting up the tube into the air, keep it well ſtopt, and firſt depreſs one end, and then the other; the quick-ſilver will fall againſt the depreſs'd end of the tube, with a ſurprizing force and ſwiftneſs: whereas, if unſtopping the tube, whilſt the ſame quantity of mercury remains in it, you let the outward air into the cavity, unpoſſeſs'd by the mercury; and then, again, ſtop the orifice with your finger, and proceed as before, you ſhall perceive the motion of the included fluid, to be much ſlower, and leſs violent than formerly, by reaſon of the reſiſtance of the admitted air; which, alſo, manifeſtly diſcloſes itſelf, by the conflict and bubbles produced betwixt the air and quick-ſilver, in haſtily paſſing by one another, to the oppoſite ends of the tube. But, Mr. *Hobbs*, not pretending that any attraction intervenes in the caſe; I ſee not how he can poſſibly make out, to omit other phenomena, the gradual deſcent of the mercury, in the tube, beneath its wonted ſtation, upon the exhauſtion of the receiver; and the re-aſcent of the ſame, in the ſame tube, as we let in more or leſs of the outward air, without admitting as much of ſpring or preſſure in the air, as I contend for. The weight of the terreſtrial particles, by which he endeavours to account for the quick-ſilver's falling lower at the top, than at the bottom of a hill, will by no means ſerve his turn; it being utterly improbable that the air, contained in ſo little a veſſel as one of our receivers, can, by its weight, counter-balance ſo ponderous a cylinder of quick-ſilver: whence we may be allowed to argue, that the air ſuſtains it by ſuch a preſſure, or ſpring, as we plead for, whether that proceed from the texture of the aerial particles, from their motion, or from both.

The laſt of Mr. *Hobbs*'s principal explanations, is of the experiment wherein above 100 pound weight, being hung at the depreſs'd ſucker, the ſucker was, notwithſtanding, impell'd up again, by the air, to the top of the cylinder. This phenomenon Mr. *Hobbs* accounts for thus. "The air being beaten back by the retraction of the ſucker, and finding no " void place, wherein to diſpoſe of itſelf, beſides that which it may make, " by driving out other bodies, is, by perpetual truſion, at length, forced " into the cylinder, with ſo great ſwiftneſs, between the concave ſurface " of the cylinder, and the convex ſurface of the ſucker, as may anſwer

“ the power neceſſary to draw back the ſucker. Now the air within re-
“ tains the ſwiftneſs wherewith it enters; and then, every way, diſtends
“ the ſides of the braſs cylinder, which is elaſtic. Therefore the air in the
“ cylinder being vehemently moved, thruſts againſt all parts of the con-
“ cave ſurface of the cylinder, tho' in vain, till the ſucker is drawn back;
“ but as ſoon as the ſucker, being looſe, ceaſes to impel the air, that air
“ which was before driven in, will, by reaſon of its endeavour againſt
“ every point of the internal ſuperficies of the cylinder, and of the elaſtic
“ force of the air, inſinuate between the ſame ſurfaces, with a velocity
“ equal to that by which it was impelled; that is, with a velocity which
“ anſwers the ſtrength of the impulſe; when the ſwiftneſs wherewith the
“ ſame air goes out of the cylinder, finding no where to diſpoſe of itſelf,
“ will again impel the ſucker to the top of the cylinder; for the ſame
“ reaſon that the ſucker, a little before, made an impulſe upon the air.”
But this whole conceit, of the air's running in and out with a ſtrange velocity, between the ſucker and the cylinder, is precarious; nor does he propoſe one phenomenon to countenance it. Farther, we found, in an engine ſo contrived, that the pump lay covered with water, when the ſucker was drawn back, the atmoſphere would ſtrongly preſs the water againſt it; and if the handle were let go, ſwiftly repel up the ſucker into the deſerted cavity of the cylinder; which being a caſe parallel to that under conſideration, let any unbiaſſed perſon judge how likely it is that the air could perform all theſe excurſions, without exciting bubbles, notwithſtanding the water's conſtant interpoſition betwixt it and the cylinder. And there is as little probability in what our author teaches of the air's *conatus*. I might here repeat, what we formerly mentioned, of the breaking of our receivers inwards, not outwards; and add, that I ſee no reaſon why the *conatus* of the included air, if granted, ſhould be fruſtrated, when the ſucker is depreſſed; but if the air within have ſo ſtrong an endeavour outwards, as to ſtretch the thick ſides of the braſs cylinder, as Mr. *Hobbs* would have it, I demand, why the air does not throw out the wooden peg or valve, which we have often, to our trouble, ſeen thrown out with great force and noiſe, when the depreſſed ſucker being thruſt up again, whilſt there was air in the cylinder, we forgot to leave the valve open; tho', in this caſe, the air that drove out the peg, was far enough from ſtretching the cylinder? I further demand, how it comes to paſs, that if, having ſtopt the hole of the cylinder with your finger, inſtead of the peg, you ſwiftly depreſs the ſucker, you ſhall be ſo far from feeling a preſſure outwards, againſt the pulp of the finger, from any thing contained in the cavity of the cylinder, that your finger will be ſtrongly preſſed in by the ambient air? But as to the laſt part of the paſſage under conſideration, I do not underſtand, why the air that was impelled in ſo ſwiftly betwixt the cylinder and the ſucker, ſhould not reſiſt the ſwift egreſs which Mr. *Hobbs* aſcribes to the included air, by the ſame paſſage; nor why this impell'd air, that has ſo ſtrong an endeavour outward, ſhould never depreſs the ſucker, as well as the ſame air, diffuſing its motion thro' the

the vaſt ambient medium can enable the external air to thruſt up the ſucker again, eſpecially ſince during ſuch a depreſſion of the ſucker, made by the rebound of the air, forcibly impell'd in from the cloſe bottom of the cylinder, the air from without may all this while, according to Mr. *Hobbs*'s principles, get in between the ſaid ſucker and the cylinder. But I further ſay, that the lifting up of the ſucker, either is not neceſſary to prevent a vacuum, or that, in ſome caſes, it will be hard to ſhew how a vacuum can, by Mr. *Hobbs*, be avoided. For when the depreſſed ſucker is ready to be thruſt up again, if you hang a much greater weight at it than 100, it will not be lifted up at all. And whereas this progreſs and regreſs of the impulſe of the external air cannot reaſonably be ſuppoſed to be very laſting, you may, by a competent weight, detain the ſucker depreſſed, till the ambient air is as quiet as uſual; yet if you then take off the overplus of weight, or perhaps a little more, in caſe the pump has been very ſtanch, the ſucker, and the great weight hung on, will nevertheleſs be carried up; which it is no way likely it could be by the impulſe of the outward air, that had time to decay and be confounded. And as for the inward air, we have proved that it has no ſuch endeavour outwards as Mr. *Hobbs* pretends; otherwiſe, why ſhould not that throw out the ſucker, rather than cauſe it to be impelled inward; it being no way likely, that in caſe ſome air ſhould get out of the cavity of the cylinder, it could ſo move the outward air, as that the reflex of that impulſe ſhould make that free outward air bear more ſtrongly againſt the outſide of the ſucker, than the inſide of the ſame ſucker is preſſed againſt by the included air, whoſe impetus is incomparably leſs diffuſed? In ſhort, whatever is reſolved to be in the cavity of the cylinder when the ſucker is depreſſed, yet ſince it is manifeſt, that it is, at leaſt in great part, deſtitute of common air; and ſince the ſucker, with the weight, may, if the inſtrument leak not, be impelled up, when, in all probability, thoſe forced undulations of the air, that may be ſuppoſed to have been made by the ſucker, have ceaſed; the *Carteſians*, Mr. *Hobbs*, and others, who will not have recourſe to the unintelligible attraction of ſome rarified ſubſtance within, muſt aſcribe ſo ſtrange a phenomenon to the preſſure of the air without. *

The reaſon why water aſcends in ſmall glaſs tubes, Mr. *Hobbs* tells us is, that "the ſmall particles interſperſed in the air, near the water, by "their motion beat upon the ſurface of the water, ſo that the fluid "muſt of neceſſity aſcend into the pipe; and the more ſenſibly into "one exceeding ſlender." But it is manifeſt, that I did not, in relating this phenomenon, take upon me to aſſign the true reaſon of it. However,

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* That experiment of Mr. *Haukſbee*'s, whereby it appears, that 140 pound weight was requiſite to ſeparate two hemiſpheres, 3 half inches in diameter, ſqueez'd together, by an additional atmoſphere of air, proves the preſſure of that fluid, beyond all contradiction, againſt the *Funicular* hypotheſis, that of ſuction, *&c.* eſpecially conſidering, that when the air was exhauſted out of theſe hemiſpheres, and an atmoſphere injected upon their external ſurfaces, the weight of 280 pound was neceſſary to cauſe their ſeparation. See *Haukſbee's Experiments*, p. 88 — 93.

I am no way ſatisfied with Mr. *Hobbs*'s explanation; for to ſay nothing of the motion he aſcribes to the particles diſperſed thro' the air, he leaves the difficulty unſolved; ſince there being common air, as well within the cavity of the ſlender pipes, as without, he offers no reaſon why the preſſure of the air within, ſhould not reſiſt the preſſure of the ſame kind of air without, as we ſee it does in greater pipes. And poſſibly he would have paſt by this particular, if he had not overlook'd the advertiſement I gave, that it would concern thoſe who ſhould undertake to ſhew the cauſes of this phenomenon, to conſider alſo of a reaſon why, if the experiment be tryed with quick-ſilver inſtead of water, the ſurface of that fluid will, inſtead of being higher, be lower within the pipe, than without it; whereas if Mr. *Hobbs*'s explication be ſufficient, why ſhould not the contrary happen in quick-ſilver as well as in water? The next paſſage to be conſidered is this: "If any one, after the frequently repeated impulſe and retraction "of the ſucker, endeavour to draw out the ſtopple of the upper orifice of the receiver, he ſhall find it gravitate very much, as if a "weight of many pounds hung upon it. Whence comes this? from "a ſtrong circular endeavour of the air within the receiver, made by "the violent ingreſs of the air, between the convex ſurface of the ſucker, "and the concave of the cylinder, procured by the repeated impulſe and "revulſion of the ſucker, improperly called the exſuction of the air; "for by reaſon of the fulneſs of nature, the ſtopple cannot be drawn "out; but whilſt that circulates very ſwiftly, it comes out very hardly; "that is, it ſeems to be very heavy; for as ſoon as freſh air is by degrees "let into the receiver, it likewiſe by degrees loſes this ſeeming gravity." To this I anſwer: firſt, that if there be ſuch a vehement circular endeavour of the air in the receiver, by which motion he teaches, that the air ruſhes out with violence enough to make the atmoſphere lift up, in our cylinder, above 100 pound weight; I ſee not why it ſhould not rather throw out the ſtopple, under conſideration, than hinder its extraction; and why, when the external air is re-admitted at the ſtop-cock into the exhauſted receiver, and thereupon there ſenſibly follows for a little while, a rotation of the included air, the ſtopple, that juſt before ſeemed ſo much to reſiſt the being drawn out, ſhould ceaſe to make any ſuch reſiſtance. Nor do I ſee how the plenitude of nature ſhould, as is here intimated, hinder the extraction of the ſtopple; for according to the pleniſts, the world and the receiver, muſt be at all times, equally full. And if the contiguous air muſt, for Mr. *Hobbs*'s reaſon, neceſſarily be extracted with the ſtopple in one caſe, I ſee not why the like ſhould not happen in another. Now it appears by our experiments, that there is no ſuch very ſtrong circular endeavour in the exhauſted receiver as he pretends; but indeed an endeavour of the ambient atmoſphere, to preſs the parts of the glaſs inwards, and cover thoſe that are contiguous to it. For, as I have noted already, a light bladder ſuſpended in the receiver, manifeſted no ſuch motion as he here ſuppoſes; neither were a pair of ſcales ſuſpended within the ſame; nor a long magnetical needle, that reſted upon the point of another needle, at all whirled about by this imaginary motion of the air. Beſides, if you leiſurely looſen the braſs ſtopple, ſo that it may be very near, but not contiguous to the ſides of the ſocket, you ſhall manifeſtly perceive a ſtrong current of air to flow into the receiver at that paſſage; and more than once, when, inſtead of that piece of braſs, we ſtopt the hole in the cover with our cement, we obſerved ſometimes whilſt we were pumping, ſometimes after we had done pumping, that the outward air, by degrees, depreſſed the ſuperficies of the cement expoſed to it, and made it concave; and now and then would break thro' it, thruſting it inward with great violence and noiſe.

Mr. *Hobbs* afterwards attempts to rectify another of our explanations, in theſe words: "We have ſeen alſo, that water let down into the receiver, after ſome returns of the motion of the ſucker, has bubbled ſo, as if it had boiled over a fire; which happens by reaſon of the ſwiftneſs of the circulating air, unleſs perhaps you find the water hot too whilſt it bubbles. For if we were ſure it was hot, we muſt find out ſome other cauſe of the phenomenon. But we are certain it is not ſenſibly hot; how therefore can the greater, or leſs motion of the atmoſphere, promote ſuch a motion as this?" But I confeſs, I ſee not how the circular motion of the air within the receiver, could in a vial with a long neck, produce ſuch effects as we mention; eſpecially, I ſee not how ſuch a wind, paſſing along the ſurface of the water, could raiſe ſo many very large bubbles, which ſeemed generally to riſe from the lower parts of the water, thro' the long neck of the vial, and ſpout it, as it were, into the receiver.

As for what he ſays about the gravity of the atmoſphere, it is plain enough that my conjecture aſcribes the phenomenon to the taking off the preſſure of the air within the receiver; tho' I ſee not why the removal of the weight of the atmoſphere, if it could be effected, out of the engine, ſhould not have a like operation. And that the greater or leſs preſſure of the air concurs to this phenomenon, appears from common experience; for water, by being heated, is expanded, and generates bubbles; and our experiments have made it appear, that there are aerial particles in water, and other liquors, which tend to expand themſelves; and actually do ſo in numerous bubbles, when the preſſure of the incumbent air is conſiderably leſſened. In the preſent phenomenon, that preſſure being, by the exhauſtion of the receiver, taken off, the aerial particles, and agitated vapours abounding in the hot water, are allowed to expand themſelves; and to make ſuch numerous and great bubbles, as thereby to carry off a large part of the water out of the vial. So that I wonder what makes Mr. *Hobbs* ſpeak, as if there were no ſenſible heat in the water, under conſideration; ſince it is expreſly ſaid, that it was put in hot; and if it were put in cold, could, by no pumping, be brought to the leaſt ſhew of efferveſcence. And I have already ſhewn by experiments, that there is, in our exhauſted receiver, no ſuch peculiar motion of the air, as he aſcribes

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the phenomenon to. Nay, when there is manifeſtly a whirling motion of the air in the glaſs, upon the admiſſion of the external air, the production of numerous bubbles in the water preſently ceaſes. And therefore Mr. *Hobbs* might have let alone my conjecture, unleſs he could either have diſproved it better, or ſubſtituted a more probable one in its ſtead.

Mr. *Hobbs* having recited our experiment of killing animals in the exhauſted receiver in 2 or 3 minutes, aſks, "if theſe animals were ſo quickly kill'd for want of air, how do divers live under water? for ſome of them, accuſtom'd to it from their childhood, will ſuſtain the want of air for a whole hour? Certainly, that moſt vehement motion, by which included bladders are diſtended and broken, kills theſe animals." But I ſtill think we want ſome clearer diſcovery about this matter, notwithſtanding what has been delivered concerning it by Mr. *Hobbs*; for his argument againſt my conjecture is anſwer'd by himſelf, ſince he plainly intimates, that divers who can live without air for a whole hour, are accuſtomed to it from their childhood. Wherefore, unleſs the animals that died in our engine, had been for a long time train'd up by degrees to live without air, it will not follow, that the want of it would not ſoon diſpatch them; as ordinary men may be drown'd in a few minutes. And having purpoſely let down ſome mice, and ſmall birds, into a deep glaſs filled with water, and kept them from emerging, by a weight ty'd to their legs or tails; tho' ſome liv'd longer than others, yet I obſerv'd them to be kill'd faſt enough, to keep my conjecture from being incredible; eſpecially the laſt we made trial of, tho' a large and luſty mouſe, appeared to be quite dead within ſomewhat leſs than a minute. And we particularly took notice, that before drowning, ſeveral bubbles, which ſeemed to be the reſpired air, came out of their mouths, and aſcended thro' the water. We have already diſproved the ſuppoſition whereon Mr. *Hobbs*'s opinion, as to the cauſe of the death of animals in the receiver, is built; for no ſuch vehement motion as he pretends, is there to be found. Nor does it at all clear the difficulty, that he would have this motion the ſame whereby included bladders are diſtended and broken. For it is very hard to conceive how the tenacity of the air, or its beating from all parts upon the convex ſurface of an almoſt empty bladder (for in ſuch alſo the experiment will ſucceed) ſhould make it burſt outwards. And we have already proved, that the diſtenſion and breaking of bladders in our receiver, proceeds not from any ſuch motion of the neighbouring air, as is here preſumed, but from a quite different cauſe.

Mr. *Hobbs* has a long diſcourſe concerning the extinction of fire in our receiver, upon exhauſting the air; and he endeavours to reduce what happens to kindled coals placed in our engine, to what happens in certain mines, wherein, when ſome thick damps aſcend, both charcoal and candles are ſoon extinguiſhed. He alſo attempts to ſhew, that by the reciprocation of the ſucker, the air, impelled firſt into the cylinder, and then into the receiver, is put into ſuch a motion, as gives it a certain middle conſiſtence, betwixt that of pure air, and that of water. But I ſhall not need to examine this ſecond part of his diſcourſe, becauſe I deny the firſt; and being able to diſprove the thickneſs of the air in the exhauſted receiver, I need not ſpend time about what he teaches of the manner. His hiſtory of the damp to be met with in mines, is more largely ſet down in his elements of philoſophy; where he ſeems to mention it as a ſtory of doubtful credit, which it is likely he would not have done, if he had then ſeen it; and if he has not ſince obſerved the thing himſelf, there may eaſily be a miſtake in ſome of the circumſtances; as for inſtance, the number of minutes wherein the thick air choaks the fire; and it is upon that circumſtance, the validity of what he deduces from the obſervation, chiefly depends. But however the matter ſtands with theſe ſubterraneal damps, we have already proved, by ſeveral of the experiments of our engine, that in the exhauſted receiver there is no ſuch motion of the air as is here ſuppoſed. And it may be ſufficiently proved, that whatever remains in the receiver, is not ſuch a ſubſtance as Mr. *Hobbs* would have it; for he here tells us, it is of a conſiſtence betwixt air and water; and in his elements ſays, it is not much lighter than water. But by the *Magdeburgic* experiment, it is evident that the receiver, by being exhauſted of common air, is ſo far from growing heavier, that it loſt above an ounce of its former weight. And to this agrees what happens in æolipiles, that grow lighter when the air is expelled. Beſides, if the receiver be, in the preſent caſe, filled with a ſubſtance, whoſe conſiſtence is ſo much nearer that of water, than is our common air, as Mr. *Hobbs* would have it, why does not a pendulum move ſenſibly ſlower in it, when in water the vibrations are ſo exceedingly ſlackened? And the breaking of an hermetically ſealed bubble, in our receiver, outwards, when the air was much exhauſted, and not before, with ſeveral other experiments, that might be eaſily applied to this purpoſe, ſufficiently prove, that it is not a thicker, and far heavier air, but a more yielding, and lighter, that remains, after pumping, in the cavity of our receiver. But as for the thing itſelf, when I related it, I thought it might admit a farther enquiry; and indeed there may be many ways of extinguiſhing fire; ſo that, as it is not, in all caſes, eaſy to aſſign the true cauſe of its extinction, it is unſafe to conclude, with Mr. *Hobbs*, that becauſe a candle, or a live coal, may be extinguiſhed by a thick damp, therefore the effect muſt proceed from the like cauſe in our receiver, where there is no ſign of any damp, or unuſual thickneſs of the air, but of the contrary.

Mr. *Hobbs* next pretends to ſhew, that neither the ſpring, nor weight of the air, have any thing to do with the two flat poliſhed marbles, not falling aſunder in our receiver; "becauſe," ſays he, "the weight of the "atmoſphere can do nothing there;" he adds, "that a ſtronger, or "more evident argument could not be brought againſt a vacuum, than "this experiment. For if two coherent marbles ſhould, either of them, "be thruſt forwards that way that their ſurfaces lie contiguous, they "would eaſily be ſevered, the neighbouring air ſucceſſively flowing into "the deſerted place. But ſo to pull them aſunder, that at one time they "ſhould loſe their whole contact, is impoſſible, the world being full; "for then either motion muſt be made from one term to another in an "inſtant, or two bodies at the ſame time muſt be in the ſame place; to "ſay either of which is abſurd." But how this ſhould be ſo cogent and manifeſt an argument againſt the vacuiſts, I do not well diſcern; for what it proves, if it prove any thing, ſeems to be, that in caſe the cohering marbles could be ſo ſevered, as to loſe at once their whole contact, the world might be concluded not to be full; but I ſee not how it thence follows, that therefore there can be no vacuum. For my part, I would demand, whether the ſtrong adheſion of the marbles be neceſſary or not to the plenitude of the world? If it be, how chance a ſufficient weight hung to the lower marble can immediately draw them aſunder? And if it be not, why does not Mr. *Hobbs* aſſign ſome other cauſe of their ſo ſtrong adheſion, if it depend neither upon the ſpring nor weight of the air? As for the non-ſeparation of the two marbles in our receiver, I have ſaid, that the cauſe may probably be the preſſure of the air remaining in the receiver; for it is no way unlikely, that the remaining air ſhould be able to ſuſtain a weight of 4 or 5 ounces hanging at the lower marble, ſince the free air was able to ſupport between 4 and 500 ounces hanging at the ſame.

But Mr. *Hobbs* tells us, that the cauſe I aſſign of the coheſion of our marbles, is liable to great inconveniences; for they confeſs," ſays he, "that all ponderation is an endeavour, every way, by right lines, to "the center of the earth; ſo that it is made not by the figure of a cy"linder or column, but by a pyramid, whoſe top is the center of the "earth, and whoſe baſis is part of the ſurface of the atmoſphere." As if it were material whether a body, whoſe baſis is ſcarce two inches diameter, and length ſome thouſands of miles, be conſidered as a cylinder or a pyramid. Certainly *Stevinus*, and other learned hydroſtatical writers, would ſcarce have made this an objection; ſince they ſcruple not to poſtulate, that all perpendiculars, not very diſtant, be looked upon as parallel; tho' they allow ſuch perpendiculars would meet in the center of the earth. What he adds, "that therefore the endeavour of all the "points which ponderate, will be propagated to the ſurface of the up"per marble, before it can be propagated further to the earth," to prove that the whole endeavour of the pyramid, that reſts upon the upper marble, is terminated there; and that there is no endeavour of the atmoſphere, againſt the under ſuperficies of the lowermoſt marble, ſeems grounded partly upon a conceit of his about the nature of gravity; according to which, I ſee not why any body, placed between the ſides of that pyramid or cone, whereof the upper ſuperficies of the higher marble is the baſis, ſhould deſcend upon the account of gravity; and partly from a miſtake of my opinion; for I no where ſpeak as if I thought

I thought the lower marble was ſuſtained by little globules, or other minute bodies, protruding one another directly towards the center of the earth, and rebounding from a perfectly ſmooth ſurface. Nor, need I ſay, that the lower ſtone is ſuſtained by the preſſure of the ſame pillar of the atmoſphere, as reſts on the upper; ſince other parts of the atmoſphere, ſome on the one hand, and ſome on the other, preſſing obliquely upon the uneven ſurface of the earth, may have their preſſure upward, terminated againſt the lower ſurface of the undermoſt marble. Thus an æolipile being, by heat, well freed from air; if the little orifice, at the extremity of the neck, by which the air gets in and out, be ſoon carefully ſtopped with wax, and afterwards ſuffered to cool, there will not be, in the cavity of the æolipile, a reſiſtance near equal to the preſſure of the outward air. And therefore, if you perforate the wax, that air will be violently impelled in, at the unſtopped orifice, whether the neck be held parallel, or perpendicular to the horizon; or, in any other ſituation, in reſpect of the center of the earth. And the like will happen, if the æolipile be unſtopp'd under water.

And, I lately met with the relation of an experiment, which abundantly makes out the power of the ambient atmoſphere, to preſs bodies againſt each other, when it cannot get between their internal ſurfaces. The ingenious author of the *Magdeburgic* experiment, writes to the induſtrious *Scottus*, that having cauſed two copper plates to be made, almoſt in the form of ſcales a little above half an ell in diameter, and exactly fit; if when laid upon one another, "the air be drawn "out, they are kept ſo compreſſed, and united, by the gravity of the "external air, that ſix ſtrong men cannot pull them aſunder; but if, at "length, they are disjoined, they make a noiſe equal to the report of "a muſquet; yet as ſoon as ever there is the leaſt entrance given to "the air, they fall aſunder of their own accord." And, if a glaſs vial have a pipe open at both ends, ſo fitted to it, that no air can get in or out, betwixt the neck and it; and if the vial be ſo far filled with water, that the lower end of the pipe be well immerſed therein; if you ſuck at the upper-end of the pipe, the water will aſcend to a great height; which argues its being forced upwards, by the oblique preſſure of the air in the vial: for it is only in the pipe, and not in the vial, that there is any air in the ſame perpendicular with the water that is impelled.

As to the aſcent of liquors by ſuction, Mr. *Hobbs* ſays, "whoever "ſucks water into his mouth by a pipe, firſt ſucks up the air between; "whereby he removes the external diſtended air, which can have no "place, but by removing the next; and ſo, by continual pulſion, the "water is at length, driven into the pipe, and ſucceeds the air which "is ſucked out." And this account of the phenomenon is alſo, given by the learned *Gaſſendus*, and other atomiſts; and ſeems generally acquieſced in by the modern philoſophers. But, though I deny not, that many phenomena of nature, may be, probably, explained by this propagation, and return of motion; yet there are ſome others, here below, which I ſee not how the *Carteſians*, the atomiſts, or Mr. *Hobbs*, can ſolve, without admitting the ſpring of the air; and which, perhaps, may, by the ſpring of the air, be explain'd, without recurring to ſuch a propagation, and return of impulſe. I took a glaſs veſſel, conſiſting of two parts; the one, a vial, capable of containing about a pound of water; and the other, a pipe, open at both ends; the lower of which reach'd within two inches of the bottom of the vial: this pipe was faſtened into the neck of the vial, with melted glaſs, of a good thickneſs; and into the veſſel, by the open pipe, I pour'd water enough to ſwim a pretty way above the lower-end of the pipe; and then often inclined the veſſel to give a free intercourſe betwixt the air within the vial, and that without it: whence, if the external air were compreſſed by the affuſion of the water, it might free itſelf, as it readily did by aſcending in bubbles along the inclined pipe, till the outward and inward air were reduced to an equality of preſſure. Now, if all ſuction were produced by the preſſure of the air, thruſt away by the dilated cheſt of him that ſucks, and ſo thruſting the fluid into the pipe, it ſeems evident, in our caſe, that the water would not aſcend by ſuction; ſince by the contrivance of the veſſel, the air thruſt away by him that ſucks, cannot, at all, come to preſs upon the water. Yet, whether the pipe were inclined, or erect, the water aſcended, upon ſuction, to the top of the pipe, and ran over into my mouth, and that with tolerable eaſe: fot though the ſpring of the air, pent up in the vial, were able, upon the decreaſe of the preſſure of the outward air occaſioned by my ſucking, to impel the water ſtrongly into the pipe; yet, when a moderate quantity of water had been impelled, the included air, gaining, thereby, more room to expand its ſpring, was ſo much weakened, that the water aſcended far leſs eaſily than in ordinary ſuction. And, to ſhew that the aſcent of the water, in the pipe, did not proceed from any ſuch tendency, in the water itſelf, to aſcend, for prevention of a vacuum; I carefully took out the water, by degrees, till the lower-end of the pipe lay but very little under the ſurface of the water; tho' in the cavity of the pipe, the water was conſiderably higher: then ſuffering the veſſel to reſt, and ſucking at the upper-end of the pipe, the water was impelled up, yet without reaching near the top, till the ſurface of it was fallen a little below the bottom of the pipe. But then, though I continued ſucking, no more water aſcended into the pipe; but the air paſſing thro' it, towards my mouth, in its paſſage toſs'd up the water that was already in the pipe, and turned it into bubbles, which broke with noiſe one after another. And thus the aſcending air long kept the water, in the pipe, from falling back to that in the vial; but when I removed my mouth, the ſpring of the air, remaining in the cavity of the vial, being weakened by the receſs of the air, I had ſucked out, it was not able to reſiſt the preſſure of the outward air; and accordingly, the water in the pipe, was not only depreſſed into the vial, but the external air forced its way, in many bubbles, and not without ſome noiſe, thro' the water contiguous to the bottom of the pipe, till the preſſure of the included air, and that of the atmoſphere, were reduced to an equality.

REMARKS UPON Mr. *HOBBS*'s Problems, ABOUT A *VACUUM*.

I Here only propoſe to conſider, whether Mr. *Hobbs* has cogently proved his aſſertion, that there is no ſuch thing as a vacuum; and whether he has rightly explain'd thoſe phenomena of nature, which he undertakes to ſolve, and eſpecially thoſe produc'd by means of our engine.

Arguments againſt a vacuum conſider'd.

And firſt, as I account for the adheſion of two flat poliſh'd marbles by the preſſure of the air, Mr. *Hobbs* thinks this phenomenon ſo fully proves the plenitude of the world, that tho' he tells us, he has many ſtrong arguments to make it out, yet he mentions but this one, as abundantly ſufficient; for, ſays he, ſince the vacuiſts allow interſperſed vacuities, not only in the air that ſurrounds the joined marbles, but in the reſt of the ambient air, there is no reaſon why there ſhould be any difficulty in ſeparating the marbles; or at leaſt, any greater difficulty than in moving them in that air after their ſeparation. But they will eaſily anſwer, that notwithſtanding the vacuities they admit in the ambient air, a manifeſt reaſon may be given, on their hypotheſis, of a difficulty in the divulſion of the marbles; for the vacuities they admit being but interſperſed, and very ſmall; and the corpuſcles of the atmoſphere being, according to them, endow'd with gravity, there reſt ſo many upon the upper ſurface of the marble, that this ſtone cannot be, at once, perpendicularly drawn up from the lower, contiguous to it, without a force able to ſurmount the weight of the aerial corpuſcles that preſs upon it. And this weight has already ſo condenſed the neighbouring parts of the ambient air, that he who would perpendicularly raiſe the upper marble from the lower, needs a conſiderable force to make the revulſion, and compel the contiguous parts of the incumbent air, to enter the pores, or intervals intercepted between them. For, the force of him, who endeavours to raiſe the upper marble, whilſt the lower ſurface of it is fenc'd from the preſſure of the atmoſphere, by the contact of the lower, which ſuffers no air to come in between them, is not aſſiſted by the weight, or preſſure of the atmoſphere; which, when the marbles are once ſeparated, preſſing as ſtrongly againſt the under ſurface of the upper marble, as the incumbent atmoſpherical column does againſt the upper ſurface of the ſame marble; the hand which endeavours to raiſe it, in the free air, has no other reſiſtance, than that ſmall one of the marble's own weight to ſurmount.

And as for the reaſon which Mr. *Hobbs*, and, as he thinks, all others give of the difficulty of this divulſion, " that the whole ſpace between " the two ſeparated marbles, cannot be inſtantaneouſly filled by the air, " let the ſeparation be ever ſo ſudden;" the pleniſts may give a more plauſible account of this experiment, than he has here done; and, therefore abſtracting from the two oppoſite hypotheſes, I may ſay, that the genuine cauſe of the phenomenon ſeems to be that which I have already aſſign'd; and that the difficulty of raiſing the upper ſtone, attending the air's not being able to come in all at once, to poſſeſs the ſpace left between the ſurfaces of the two marbles, upon their ſeparation, proceeds from hence; that, till this ſpace be fill'd with the atmoſpherical air, the hand, which would raiſe the upper marble, cannot be fully aſſiſted by the preſſure of the air, againſt the lower ſurface of that marble: for, ſince I declare not for the hypotheſis of the pleniſts, as it is maintained by Mr. *Hobbs*, I am not bound to allow, what the common explanation, adopted by my adverſary, ſuppoſes, that either nature abhors a vacuum; or that there could be no divulſion of the marbles, unleſs at the ſame time, the air were admitted into the ſpace that divulſion makes for it. And a vacuiſt may ſay, provided the ſtrength employed, to draw up the upper marble, be able to ſurmount the weight of the aerial corpuſcles, accumulated upon it; the divulſion would enſue, though no air, or other body, ſhould be permitted to fill the room made for it, by the divulſion; and that the air's ruſhing into this ſpace, does not, neceſſarily, accompany, but, in order of nature, and time, follow, upon a ſeparation of the marbles; the air that ſurrounded their contiguous ſurfaces, being, by the weight of the collateral air above, impelled into the room newly made by divulſion. But, having purpoſely, in our pneumatic receiver, accommodated two flat and poliſhed marbles, ſo that the lower being fix'd, the upper might be laid upon it, and drawn up again, as there ſhould be occaſion; I found, that if, when the receiver was well exhauſted, the upper marble was, by a certain contrivance, laid flat upon the lower, they would not then cohere, as before, but. with great eaſe, be ſeparated; tho' it did not, by any phenomenon, appear, that any air could ruſh in, to poſſeſs the place given it by the receſs of the upper marble; whoſe

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very eaſy avulſion, ſeemed owing to the preſſure of that little air remaining in the receiver, being too faint to make any conſiderable reſiſtance to the ſeparation of the upper marble; whence the hand, that drew it up, had very little more to overcome, than the ſingle weight of the ſtone.

But the cauſe Mr. *Hobbs* aſſigns for this phenomenon, is improbable; and a better has been aſſign'd already. For, firſt, he requires, to the divulſion of the marbles, a force great enough to ſurmount the hardneſs of the ſtone. But, this is *gratis dictum*; and, it ſeems very unlikely, that ſo ſmall a weight, as will ſuffice to ſeparate two coherent marbles, of about an inch, for inſtance, in diameter, ſhould be able to ſurmount the hardneſs of ſuch ſolid ſtones, as we uſually employ in this experiment. And, tho' it be generally, judg'd more eaſy to break a broader piece of marble *cæteris paribus*, than a much narrower; yet, neither I, nor, I believe, Mr. *Hobbs*, ever obſerv'd any difference in the reſiſtance of marbles to ſeparation, ariſing from the greater or leſs thickneſs of the ſtones. Yet, I find, by conſtant experience, that, *cæteris paribus*, the broadneſs of the coherent marbles, exceedingly increaſes the difficulty of disjoining them.

But Mr. *Hobbs*, upon the ſuppoſition of the world's plenitude, illuſtrates our phenomenon, by drawing aſunder the oppoſite parts of a piece of wax: a very improper inſtance, ſurely! For, the parts to be divided in the wax, are of a ſoft, and yielding conſiſtence, and according to him, of a ductile nature; and not, as the parts of the coherent marbles, very ſolid and hard. The parts of the wax, alſo, do not ſtick together, barely, by a ſuperficial contact of two ſmooth planes, as do the marbles we are ſpeaking of; but have their parts intangled with one another; and, therefore they are are far from a diſpoſition to ſlide aſunder, like the marbles. Beſides, it is manifeſt, that the air has opportunity to ſucceed in the places ſucceſſively deſerted by the receding parts of the attenuated wax: but it is neither manifeſt, nor well proved by Mr. *Hobbs*, that the air, after the ſame manner, ſucceeeds between the two marbles; which, as I lately noted, are not forced aſunder, after ſuch a manner; but, as himſelf ſpeaks, ſever'd, in all their points, at the ſame inſtant.

In the ſecond place, a better ſolution of the phenomenon has been already given from the preſſure of the atmoſphere, upon all the ſuperficial parts of the upper marble, except thoſe that touch the plane of the lower. And to ſhew, that when two coherent marbles are ſuſtained, horizontally, in the air, the cauſe why they are not to be forc'd aſunder, if they have two or three inches in diameter, without the help of a conſiderable weight, is the preſſure of the ambient air; I cauſed two ſuch coherent marbles to be ſuſpended in a large receiver, with a weight at the lower, that might help to keep them ſteddy; but was very inconſiderable to that which their coheſion could have ſurmounted: then cauſing the air to be pumped, by degrees, out of the receiver, the marbles long ſtuck cloſe together; becauſe during that time, the air could not be ſo far exhauſted, but that there remained enough to ſuſtain the ſmall weight which endeavour'd their divulſion. But, when the air was further evacuated, at length, the ſpring of the little expanded air that remained, being grown too weak to ſuſtain the lower marble, and its ſmall clog, they dropt off. And to confirm my explanation of our phenomenon, I ſhall add, that as this trial, which I had ſeveral times occaſion to repeat, ſhews the coheſion of our two contiguous marbles would ceaſe, upon withdrawing the preſſure of the atmoſphere; ſo by another experiment it appears, that ſuch a preſſure ſufficed to cauſe that coheſion; for having found, that when the receiver was well exhauſted, two marbles, tho' conſiderably broad, being laid upon one another, after the requiſite manner, their adheſion became ſo weak, that the upper would be eaſily drawn up from the other, we laid them again one upon the other; and then letting the external air flow into the receiver, we found that the marbles now adhered ſo well together, that we could not raiſe the upper without the lower.

But farther, Mr. *Hobbs*, in arguing againſt our engine, unjuſtly ſuppoſes the whole deſign of it to be the proof of a vacuum; for or againſt which I have never yet declared.*

Beſides, he ſeems not to have rightly underſtood, or at leaſt not to have ſufficiently regarded, in what chiefly conſiſts the advantage which the vacuiſts may make of the air-pump againſt him. In ſeveral places he is very ſollicitous to prove, that the cavity of our pneumatical receiver is not altogether empty; but the vacuiſts may tell him, that ſince he aſſerts the abſolute plenitude of the world, he muſt reject, not only great vacuities, but alſo thoſe very ſmall and interſperſed ones, which they ſuppoſe are intercepted between the ſolid corpuſcles of other bodies, and particularly

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* The moſt cautious and reſerv'd philoſopher need now make no ſcruple to aſſert a vacuum. Sir *Iſaac Newton* has furniſh'd us with ſeveral arguments which are deciſive of this point. Thoſe indeed who make the eſſence of matter to conſiſt in extenſion, may well deny a vacuum; but the nature of gravity, which is proportionable to the matter, or ſolid content of bodies, fully demonſtrates, much the largeſt part of the univerſe to be empty ſpace. The motions of the comets alſo demonſtrate a vacuum, for theſe move in all manner of planes and directions, thro' the vaſt celeſtial ſpaces, without any ſenſible reſiſtance. The vibrations of pendulums confirm the ſame; for if they be made to move in a ſpace free from air, they likewiſe meet with no reſiſtance; and therefore there is no ſenſible matter remaining in that ſpace. If there were no vacuum, a projectile in the air, or in a ſpace deſtitute of air, would move as ſlowly as in quick-ſilver; for the reſiſtance of fluid mediums is nearly in proportion to their denſity; and it is a great miſtake, to think that the reſiſtance of projectiles will decreaſe *in infinitum*, by an infinite diviſion of the parts of the fluid; for it is plain that reſiſtance cannot be conſiderably leſſened by dividing the parts of the fluid. For why ſhould not the ſame quantity of matter have the ſame power of reſiſtance, when divided into many ſubtile parts, as when divided into a few that are larger? A ſphere, moving in a compreſſed fluid at reſt, and of the ſame denſity with itſelf, would loſe half its motion before it had gone twice the length of its diameter.

In another place the ſame great philoſopher argues thus: "The reſiſtance of water ariſes principally, and almoſt entirely, "from the *Vis inertiæ* of its matter; and "by conſequence, if the heavens were as "denſe as water, they would not have "much leſs reſiſtance than water; if as "denſe as quick-ſilver, they would not "have much leſs reſiſtance than quick-"ſilver;

larly of the air; whence it would not confute them to prove, that in our receiver, when moſt thoroughly exhauſted, there is not one great and abſolute vacuity; or, as they ſpeak, a *vacuum coacervatum*; ſince ſmaller and diſſeminated vacuities would ſerve their turn. And therefore they may think their pretenſions highly favour'd, as by ſeveral particular effects, ſo by this general phenomenon of our engine, that the common, or atmoſpherical air, which, before the pump was work'd, poſſeſſed the whole cavity of our receiver, is, by the contrivance of the pump, made in a great meaſure to paſs out of the cavity into the open air, without being able, at leaſt for a while, to get in again; yet it does not appear, by any thing Mr. *Hobbs* has alledg'd, that any other body ſucceeds, adequately to fill the ſpaces deſerted by ſuch a multitude of aerial corpuſcles.

Glaſs impervious to air.

And tho' he endeavours to prove our receiver to be always full of air, yet that the common air cannot enter thro' the pores of glaſs, appears by the following experiment. We took a bubble of thin white glaſs, about the bigneſs of a nutmeg, with a very ſlender ſtem, 4 or 5 inches long, and of the thickneſs of a crow's quill; and holding the end of this ſtem in the flame of a lamp, blown with a pair of bellows, it was readily ſeal'd up; and preſently the ſpherical part of the glaſs being held by the ſtem, was kept turning in the flame, till it became red hot, and ready to melt; then being a little removed from the heat, as the included air began to loſe of its agitation and ſpring, the external air manifeſtly and conſiderably preſſed in one of the ſides of the bubble. But the glaſs being again,

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" ſilver; if abſolutely denſe, or full of matter, without any vacuum, let the matter be never ſo ſubtile and fluid, they would have a greater reſiſtance than quick-ſilver. A ſolid globe, in ſuch a medium, would loſe above half its motion, in moving three times the length of its diameter; and a globe not ſolid, ſuch as are the planets, would be retarded ſooner. And therefore to make way for the regular, and laſting motions of the planets and comets, it is neceſſary to empty the heavens of all matter, except perhaps ſome very thin vapours, ſteams, or effluvia, ariſing from the atmoſphere of the earth, planets, and comets. A denſe fluid can be of no uſe for explaining the phenomena of nature, the motions of the planets and comets being better explained without it. It ſerves only to diſturb and retard the motions of thoſe great bodies, and make the frame of nature languiſh; and in the pores of bodies, it ſerves only to ſtop the vibrating motion of their parts, wherein their heat and activity conſiſts. And as it is of no uſe, and hinders the operations of nature, and makes her languiſh; ſo there is no evidence for its exiſtence, and therefore it ought to be rejected."

" If all ſpace was equally full, the ſpecific gravity of the fluid, wherewith the region of the air is filled, would, by reaſon of the vaſt denſity of its matter, be as heavy in ſpecie as quick-ſilver, gold, or any other the moſt denſe body; whence neither gold, nor any other matter, would deſcend in that air; for no bodies deſcend in fluids, that are not ſpecifically lighter than they."

It is farther demonſtrable, that all bodies have more pores than ſolid parts; and that ſome have infinitely more pores than others; whence ariſes another proof of a vacuum. See *Newton. Optic.* p. 310, 311, *&c. Princip.* p. 316, 317. 328. 342, 343. 368.

before the cold crack'd it, held, as before, in the flame; the rarified air diſtended and plump'd up the bubble; which, being the ſecond time remov'd, was a ſecond time compreſs'd; and being, the third time, brought back to the flame, ſwell'd as before, and remov'd, it was again compreſſed; till at length, having ſatisfied ourſelves, that the included air was capable of being condens'd, or dilated, without the ingreſs or egreſs of air, properly ſo called, we held the bubble ſo long in the flame, ſtrenghtened by nimble blaſts, that not only it had its ſides plump'd up, but a hole violently broken in it, by the over-rarified air, tho' till then, it was no way crack'd.

Hence, it eaſily appears, how impervious our thick pneumatic receivers are to common air; ſince a thin glaſs bubble, when its pores were opened, or relaxed by flame, would not give paſſage to the ſpringy particles of the air, tho' violently agitated: for, if theſe particles could have got out of the pores, they never would have broke the bubble; nor, probably, would the compreſſion, that afterwards enſued, of the bubble, by the ambient air, be check'd near ſo ſoon, if thoſe ſpringy corpuſcles had not remained within to make reſiſtance.

The Nature of rarifaction and condenſation.

Yet, what I principally deſign'd, in this experiment, was to ſhew, and prove at once, by an inſtance not liable to the ordinary exceptions, the true nature of rarifaction, and condenſation, at leaſt, of the air: for, 'tis here plain, that when the bubble, after the glaſs had been firſt thruſt in, towards the center, was expanded again by heat, the included air poſſeſſed more room than before; yet it could perfectly fill no more room than before; each aerial particle taking up, both before and after the heating of the bubble, a portion of ſpace adequate to its own bulk: ſo that in the cavity of the expanded bubble, we muſt either admit vacuities interſpers'd between the corpuſcles of the air; or allow, that ſome fine particles of the flame, or other ſubtile matter, came in, to fill up thoſe interſtices; which matter muſt have enter'd the cavity of the glaſs at its pores. And, afterwards, when the red-hot bubble was remov'd from the flame, it is evident, that ſince the groſſer particles of the air could not get thro' the glaſs, which they were unable to do, even when vehemently agitated by an ambient flame, the compreſſion of the bubble, and the condenſation of air, neceſſarily conſequent upon it, cou'd not, ſuppoſing the plenitude of the world, be perform'd without ſqueezing out ſome of the ſubtile matter, contained in the cavity of the bubble, whence it could not iſſue but at the pores of the glaſs.

The preſſure of the air manifeſted to the eye.

Mr. *Hobbs* is pleaſed to compare our pneumatic engine to a pot-gun, and attributes the phenomena, exhibited in the exhauſted receiver, to the expanſive endeavour of the air outwards: I ſhall, therefore, ſhew, that there is, in our exhauſted receiver, no ſuch ſtrong endeavour outwards, as he ſuppoſes; but that the weight of the atmoſpherical air, when 'tis not reſiſted by the counter-preſſure of any internal air, is able to perform what a weight of many pounds would not ſuffice for. Glaſs not being a yielding body, cannot, by the alteration of its figure, from an external uniform

form preſſure, ſhew when ſuch an one is exerciſed upon it; and therefore inſtead of a receiver of glaſs, we provided one of pewter, and applied it to the engine after the uſual manner. And tho' the inverted veſſel, by reaſon of its ſtiffneſs, thickneſs, and the convexity of its ſuperficies, were ſtrong enough to have ſupported a great weight, without changing its figure; yet as ſoon as by an exſuction or two, the remaining part of the included air was brought to ſuch a degree of expanſion, that its weakened ſpring was able to afford little aſſiſtance to the tenacity and firmneſs of the metal, the weight of the pillar of the incumbent atmoſphere, preſently depreſſed the upper part of the veſſel; at once leſſening its capacity, and changing its figure; ſo that inſtead of the convex ſurface, it gain'd a concave one. The experiment ſucceeded alſo with a common pewter porringer. And ſometimes I found alſo, that the veſſel would be thruſt in, not at the top, but on the ſide, if that were the only part made too thin to reſiſt the external preſſure.

Whether air penetrates quick-ſilver in the Torricellian *experiment.*

Mr. *Hobbs* afterwards proceeds to the *Torricellian* experiment, which he will needs have performed by means of air in the tube; and attempts to prove, that air may paſs thro' quick-ſilver, becauſe a blown bladder, forcibly detained under it, will of itſelf emerge, when the detaining force is removed.

This I confeſs is ſurprizing. It concern'd Mr. *Hobbs* to prove, that as much air as was diſplaced by the deſcending mercury, did, at the orifice of the tube, immerſed in ſtagnant mercury, inviſibly aſcend to the upper part of the pipe; and he tells us, that a bladder full of air being depreſſed in quick-ſilver, will, when the hand that depreſſed it is removed, be ſqueezed up by the very weight of the mercury; whence it follows, that air may penetrate quick-ſilver. But who ever deny'd that air, ſurrounded with quick-ſilver, may thereby be ſqueez'd upwards? And ſince even very ſmall bubbles of air may be ſeen to move in their paſſage thro' mercury, how will this example help Mr. *Hobbs?* For it is by mere accident that the air included in the bladder comes to be buoy'd up, becauſe the bladder itſelf is ſo; and if it were filled with water inſtead of air, or with ſtone inſtead of water, it would nevertheleſs emerge, as himſelf confeſſes, if it were iron, or any matter, except gold; becauſe all other bodies are lighter in ſpecie than quick-ſilver. But ſince the emerſion of the bladder is manifeſt enough to the ſight, how does this prove, that the air gets into the *Torricellian* tube inviſibly, ſince our eyes diſcover no ſuch motion of the air, which muſt not only paſs unſeen thro' the ſuſtained quick-ſilver, but likewiſe imperceptibly dive, in ſpite of its comparative lightneſs, beneath the ſurface of the ſtagnant mercury, to get in at the orifice of the erected tube.

But to clear up this matter; having made the *Torricellian* experiment in a ſtrait tube, after the ordinary manner, we took a piece of fine bladder, and raiſing the pipe a little in the ſtagnant mercury, but not ſo high as the ſurface thereof, we dextrouſly convey'd it into the quick-ſilver, ſo as to be apply'd by the finger to the immers'd orifice of the pipe, without letting the air get into the cavity of it; then the bladder was, cautiouſly, ty'd very tight to the lower end of the pipe, whoſe orifice it cover'd before, and the pipe, being now ſlowly lifted out of the ſtagnant mercury, the ſuſtain'd quick-ſilver appear'd to preſs, but very lightly upon the bladder; being ſo near an exact equilibrium with the atmoſpherical air, that if the tube were but a very little inclin'd, whereby the perpendicular gravitation of the quick-ſilver came to be ſomewhat leſſen'd, the bladder would immediately be driven into the orifice of the tube, and to the eye, plac'd without, appear to have acquir'd a concave ſuperficies, inſtead of the convex it had before. And, when the tube was again erected, the bladder would no longer appear ſuck'd in, but be again ſomewhat protuberant. And if, when the mercury in the tube was made to deſcend, a little below its ſtation, into the ſtagnant quick-ſilver, the piece of bladder were, at the juncture, nimbly and dexterouſly apply'd, as before, to the immers'd orifice, and faſten'd to the ſides of the pipe; upon lifting the inſtrument out of the ſtagnant mercury, the cylinder of quick-ſilver being now ſomewhat ſhort of its due height, was no longer able fully to counterpoiſe the weight of the atmoſpherical air; which, conſequently, tho' the glaſs were held erect, would preſs up the bladder into the orifice of the tube, and cauſe a cavity, ſenſible both to the eye and touch.

This experiment fully ſhews, that the preſſure of the external air is able to ſuſtain a cylinder of twenty-nine or thirty inches of mercury, and, upon a ſmall diminution of the gravity of that ponderous fluid, to preſs it up higher into the tube. But a farther uſe may be made of it againſt Mr. *Hobbs*. Fot, when the tube is again erected, the mercury will ſubſide as low as at firſt, and leave as great a ſpace, as formerly was left deſerted at the top; into which, how the air ſhould get to fill it, will not appear eaſy to them, who know, that a bladder will rather be burſt by air, than afford it paſſage. And if it ſhould be pretended, either that ſome air from without had got thro' the bladder, or that the air preſum'd to have been, juſt before, included between the bladder and the mercury, made its way from the lower part of the inſtrument to the upper; we reply, 'tis no way likely that it ſhould paſs all along the cylinder unperceiv'd by us; ſince when there are really any aerial bubbles, tho' ſmaller than pins heads, they are eaſily diſcernible. And, in our caſe, there is no ſuch reſiſtance of the air to the aſcent of the ſtagnant mercury, as Mr. *Hobbs* pretends in the *Torricellian* experiment, made after the uſual way.

But further, we took a cylindrical pipe of glaſs cloſed at the upper end, and ſo long, that being dexterouſly bent at ſome inches diſtance from the bottom, the ſhorter leg was made parallel to the longer. In this glaſs, we found an expedient to make the *Torricellian* experiment; the quick-ſilver in the ſhorter leg ſerving inſtead of the ſtagnant portion in the uſual baroſcope; and that in the longer leg, reaching above the mercury in the ſhorter about eight or nine and twenty inches. Then, by another artifice, the ſhorter leg, into which the mercury did not riſe within an inch of the top, was ſo order'd, that it could in a trice be hermetically ſeal'd up;

an inch of common air being left in it, without diſordering the mercury: and having, in this manner, ſhut up ſuch a quantity of uncompreſs'd air, we warily held a pair of heated tongs near the outſide of the glaſs, whereby, the air being agitated, was enabled to expand itſelf to double its former dimenſions; and conſequently had its ſpring ſo ſtrengthen'd, that it was able to raiſe all the quick-ſilver in the longer leg, and ſuſtain a mercurial cylinder above nine and twenty inches high; when, were it not for the heat, it would have loſt half the force of its elaſticity.

Now Mr. *Hobbs* will find it very difficult to ſhew, what keeps the mercury ſuſpended in the longer leg of ſuch a barometer, when the ſhorter leg is unſtopp'd, at which it may run out; ſince this inſtrument is portable. And when the ſhorter leg is ſeal'd, it will be very hard for Mr. *Hobbs* to ſhew there the odd motions of the air, to which he aſcribes the *Torricellian* experiment. For if you warily incline the inſtrument, the quick-ſilver will riſe to the top of the longer leg, and immediately ſubſide, when the inſtrument is again erected; and yet no air appears to paſs thro' the quick-ſilver interpos'd between the ends of the longer, and the ſhorter leg. But that which I would chiefly take notice of in this experiment, is, that upon the external application of a hot body to the ſhorter leg, when ſeal'd up, the included air was expanded from one inch to two; and ſo rais'd the whole cylinder of mercury in the longer leg; and whilſt the heat continu'd undiminiſh'd, kept it from ſubſiding again. For if the air were able to get, unſeen, thro' the body of the quick-ſilver, why had it not been much more able, when rarify'd by heat, to paſs thro' the quick-ſilver, than for want of doing ſo, to raiſe and ſuſtain ſo great a weight of mercury?

How water comes to aſcend in glaſſes after ſome of their air is drawn out.

The laſt thing attempted by Mr. *Hobbs*, in his problems, is to account for the riſe of water into a vial plung'd therein, with the mouth downwards, after ſome air has been ſuck'd out of it: where he demands, upon ſuppoſing a *Vacuum*, and, conſequently, interſpers'd vacuities in the air of the vial, "how it happens, that the water would not aſcend before the ſuction was made?" To this the vacuiſts will eaſily anſwer, by acknowledging, that there were, indeed, interſpers'd vacuities in the air contain'd in the vial before the ſuction; but adding that there was no reaſon why the water ſhould aſcend to fill them, becauſe, being a heavy body, it cannot riſe of itſelf, but muſt be rais'd by ſome prevalent weight or preſſure, which was then wanting. Beſides, there being interſpers'd vacuities, as well in the reſt of the air that was very near the water, as in that contain'd in the vial, there is no reaſon why the water ſhould aſcend to fill the vacuities of one portion of air, rather than that of another. But when once, by ſuction, many of the aerial corpuſcles were made to paſs out of the vial, the ſpring of the remaining air being weaken'd, whilſt the preſſure of the ambient air, which depends upon its conſtant gravity, is undiminiſh'd, the ſpring of the internal becomes unable to reſiſt the weight of the external air; which is therefore able to impel the interpos'd water, with ſome violence, into the cavity of the glaſs, till the air, remaining in that cavity, being reduced almoſt to its uſual denſity, is able, by its ſpring, and the weight of the water got up into the vial, to hinder any more from aſcending. For, as to what Mr. *Hobbs* affirms, that "the perſon, who ſucks "the vial, draws nothing into his ſtomach, lungs or mouth;" how he will reconcile this with what he elſewhere delivers about ſuction, I leave him to conſider. I cannot, however, but wonder at his confidence, who can poſitively aſſert a thing ſo repugnant to the common ſentiments of men of all opinions, without offering any proof for it. But, I ſuppoſe, they who are, by trial, acquainted with ſuction, and have felt the air come in at their mouth, will prefer their own experience to his authority. And as to what he adds, that the perſon who ſucks agitates the air, and turns it within the vial into a kind of circulating wind, that endeavours every where to get out; I wiſh he had ſhewn us, by what means a man in ſucking makes this odd commotion in the air, eſpecially in ſuch vials as I employ about this experiment, the orifice whereof is ſometimes leſs than a pin's head.

But that real air may be extracted, by ſuction, out of a glaſs, appears by an experiment made with a receiver exhauſted by our pump; and, conſequently by ſuction. For, when we had counterpois'd it in very nice ſcales, and, afterwards, by turning a ſtop-cock, let in the outward air, there ruſh'd in as much to fill the ſpace deſerted by the evacuated air, as weigh'd ſome ſcruples, tho' the receiver were not of the largeſt ſize.

Mr. *Hobbs* pretends, that as ſoon as the neck of the vial is unſtopt under water, the air that whirl'd about before, makes a ſally out, and forces in as much water. But if the orifice be any thing large, you will, inſtead of feeling an endeavour to thruſt away your finger that ſtopt it, find the pulp of it ſo thruſt inward, as to appear to be ſuck'd in. And this may be the reaſon why the lip of him who ſucks, is often ſtrongly faſten'd to the orifice of the vial's neck; which Mr. *Hobbs* aſcribes to a moſt exquiſite contact, but without clearly telling us how that extraordinary contact is effected. And, when your finger is remov'd, inſtead of perceiving any air go out of the vial thro' the water; which, if any ſuch thing happen'd, would eaſily be diſcover'd by the bubbles; you ſhall ſee the water briſkly ſpring up in a ſlender ſtream to the top of the vial; which it could not do, if the cavity were already full of air. And, to prove when the air really paſſes in and out of the vial immers'd under water, that 'tis very eaſy to perceive its motions; if you dip the neck of the vial in water, and then apply to the globular part of it, either your warm hands, or any other competent heat; the internal air being rarify'd, a portion of it, anſwerable to the degree of heat apply'd, will manifeſtly paſs thro' the water in ſucceſſive bubbles, whilſt yet no water gets into the vial to fill the place deſerted by that air. And if, when you have fill'd the neck, and part of the belly of the vial with water, you immerſe the orifice into ſome that is ſtagnant, and apply your warm hands to the ſpherical part as before; the water in the vial will be driven out, before any bubbles paſs out of the vial into the ſurrounding water; which ſhews, that the air is not ſo forward to dive under water, as Mr. *Hobbs* ſuppoſes.

But to clear up this matter ſtill farther, we took a glaſs bubble with a ſlender cylindrical ſtem, and by applying a convenient heat to the outſide of the ball, we expell'd ſo much of the air, that, when the end of the pipe was dipt in water, and the inward air had time to recover its former coolneſs, the water aſcended to the top of the pipe. This done, we gently and warily rarify'd the air in the cavity of the bubble, till, by its expanſion, it had driven out almoſt all the water, which had got up into the ſtem; ſo that it might attain, as near as poſſible, to that degree of heat and meaſure of expanſion it had when the water began to riſe in it. And we left two or three drops of water, unexpell'd, at the bottom of the pipe; to be ſure, that none of the included air was, by this ſecond rarifaction, driven out at the orifice of it: as the depreſſion of the water ſo low aſſured us, on the other ſide, that the included air wanted nothing conſiderable of the expanſion it had when the water began to aſcend into the pipe. Whilſt the air was in this rarify'd ſtate, we preſently remov'd the little inſtrument, out of the ſtagnant water, into ſtagnant quick-ſilver, which, in a ſhort time, began to riſe in the pipe. Now, if the aſcent of the liquor were the effect of nature's abhorrence of a *Vacuum*, or of ſome internal principle of motion, or of the compreſſion or propagated truſion of the outward air, by that which had been expell'd; why ſhould not the mercury have aſcended to the top of the pipe, as the water did before? But, in fact, it did not aſcend near half ſo far; and if the pipe had been long enough, as well as 'twas ſlender enough, I queſtion, whether the mercury would have aſcended, in proportion to the length of the ſtem, half ſo high as it did.

Now, of this experiment, which we try'd more than once, I ſee not how any good account can be given without our hypotheſis, according to which 'tis clear; for the aſcent of liquors, being an effect of the prevalency of the external air's preſſure againſt the reſiſtance it meets with in the cavity of the inſtrument, and the quick-ſilver being bulk for bulk many times heavier than water; the ſame ſurplus of preſſure that was able to impel up water to the top of the pipe, ought not to be able to impel up the quick-ſilver to any thing near that height. And if it be here objected, as it very plauſibly may, that the rais'd cylinder of mercury was much longer than it ought to have been, with regard to a cylinder of water, the proportion in gravity between thoſe two fluids being conſider'd; I anſwer, that when the cylinder of water reach'd to the pipe, the air poſſeſſed no more than the cavity of the ſpherical part of the inſtrument; being very little aſſiſted to dilate itſelf by ſo light a cylinder as that of water: but when the quick-ſilver came to be impell'd into the inſtrument, by the weight of the external air; that ponderous body did not ſtop its aſcent, as ſoon as it came to be equiponderant to the expell'd cylinder of water; becauſe to attain that height it reach'd but a little way into the pipe, and left all the reſt of the cavity to be fill'd with part of that air, which formerly was all ſhut up in the bubble; by which means, the air included in the whole inſtrument muſt needs be in a ſtate of expanſion, and thereby have its ſpring weaken'd, and, conſequently, diſabled to reſiſt the preſſure of the external air, as much as the ſame included air did before, when it was leſs rarified: on which account, the undiminiſhed weight or preſſure of the external air was able to raiſe the quick-ſilver gradually higher, till it had obtained that height, at which the preſſure, compounded of the weight of the mercurial cylinder, and the ſpring of the internal air, now leſs rarified than before, was equivalent to the preſſure of the atmoſphere.

And, to confirm this experiment, by a kind of inverſion of it, we, by heat, drove a little air out of the bubble, and dipt the open end of the pipe into quick-ſilver, which, by the this means, aſcended, till it had filled about a fourth part of the pipe when held erect. Then, carefully, removing it, without letting fall any quick-ſilver, or letting in any air, we held the orifice of the pipe a little under the ſurface of a glaſs full of water; and applying a moderate heat to the outſide of the ball, we warily expelled the quick-ſilver, yet leaving a little, to be ſure, that no air was driven out with it; then ſuffering the included air to cool, the external made the water, not only aſcend to the very top of the pipe, and thence ſpread itſelf a little into the cavity of the ball, but carried up before it, the quick-ſilver that had remained unexpelled at the bottom of the ſtem. And, if, in making the experiment, we firſt rais'd, as we ſometimetimes did, a greater quantity of quick-ſilver, and afterwards drove it out; the quantity of water that would be impelled into the cavity of the pipe, and ball, was accordingly increaſed.

In this experiment, it is manifeſt, that ſomething is driven out of the cavity of the glaſs, before the water or quick-ſilver begins to aſcend in it. And here, alſo, we ſee not, that the air can paſs thro' the pores of quick-ſilver or water, but that it drives them on before it, without eaſily mixing with them. And there appears no circular wind, as Mr. *Hobbs* fancies in the ſucked vial, nor any tendency outwards of the included air, upon the account of ſuch a wind: but that inſtead of theſe, the aſcent of the liquors into the cavity of the pipe, depends upon the external air preſſing them up, appears from hence, that the ſame weight of the atmoſphere, impell'd into the pipe ſo much more of the lighter fluid, water, than of the heavier, mercury.

And that there is no need of the ſallying of air out of a vial to make the atmoſpherical air preſs againſt a body that cloſes the orifice of it, when the preſſure of the internal air is much weakened, I have ſhewn, by ſucking out, by the help of an inſtrument, a conſiderable portion of the air contained in a glaſs: for having then, inſtead of unſtopping the orifice under water, ſuddenly apply'd a flat body to it, the external air preſs'd that body ſo forcibly againſt it, as to keep it faſten'd and ſuſpended, tho' it were clogged with a weight of many ounces. Or if there be ſuch a circular wind, as Mr. *Hobbs* pretends, produced by ſuction, in the cavity of the vial, it muſt needs be ſtrangely laſting. For I have ſeen more than once, that when, by an inſtrument, much air has been ſucked out of a vial, which was afterwards carefully cloſed, and thus kept for ſome months; yet, when 'twas open'd under water, a convenient quantity of liquor would be briſkly impell'd up into the neck and belly of the vial.

And, having with the ſun-beams produced ſmoke in one of thoſe well-ſtopp'd vials; this circular wind did not at all appear to blow it about, but ſuffer'd it to riſe, as it would have done, if the included air had been very calm.

I ſhall add but one experiment more, which will not be liable to ſome of the objections, invalid as they are, that Mr. *Hobbs* has alledg'd in his account of the ſuck'd vial; and which will ſhew, that the weight of the atmoſphere is a very conſiderable thing; and alſo, that whilſt Mr. *Hobbs* does not admit a more ſubtile matter than common air to paſs thro' the pores of cloſe and ſolid bodies; the air he has recourſe to, will ſometimes come too late to prevent a *Vacuum*. Having caus'd an æolipile very light, conſidering its bulk, to be made by a famous artiſt, I had occaſion to put it ſo often into the fire, for ſeveral trials, that the copper ſcal'd off by degrees and left the veſſel much thinner than when it firſt came out of the artificer's hands; and now, after a long interval, I had occaſion to employ it, as formerly, to weigh the air it would contain. Tō make this experiment the more exactly, the air was, by a ſtrong fire, carefully driven away; when clapping a piece of ſealing-wax to the pin-hole, at which it had been forced out, we prevented a communication betwixt the cavity of the inſtrument, and the external air; and ſuppoſing the æolipile to be very well exhauſted, we laid it by, that when it ſhould be grown cold, we might, by opening the orifice again, let in the outward air, and obſerve what increaſe of weight it would make. But the inſtrument had been ſo far exhauſted, that what air remained, being unable by its ſpring to aſſiſt the æolipile to ſupport the weight of the ſurrounding air; this external fluid did, by its weight, ſo ſtrongly compreſs it, and thruſt it ſo conſiderably inwards, and, in more than one place, ſo change its figure, that when I ſhewed it to the Gentlemen aſſembled at *Greſham* College, they were pleaſed to command it of me, to be kept in their repoſitory.

A N
INQUIRY
INTO THE
Cauſe of Attraction
BY
SUCTION.

The Nature of Attraction.

SUCTION being generally look'd upon as a kind of attraction, it will be requiſite to premiſe ſomething about the latter, in order to clear the nature of the former. The cauſe, nature, and notion of attraction, are generally either left untouch'd, or happen to be but very obſcurely delivered.

How general and ancient ſoever the common opinion may be, that attraction is a kind of motion quite different from pulſion, I confeſs it ſeems to me a ſpecies of pulſion, at leaſt among inanimate bodies. I have not yet obſerved any thing which ſhews attraction cannot be reduced to pulſion; for they ſeem but extrinſical denominations of the ſame local motion, in which, if a moved body precede the movent, or tend to get at a greater diſtance from it, we call it pulſion; and if, upon account of the motion, the ſame body follow the movent, or approach to it, attraction. But this difference may conſiſt only in an accidental reſpect, which does not phyſically alter the nature of the motion, but is founded upon the reſpect which the line, wherein the motion is made, happens to have to the ſituation of the movent. And that which ſeems to have been the chief cauſe of miſtaking attraction, for a motion oppoſite to pulſion, is, that men look upon both the moving, and moved bodies, in a popular and ſuperficial manner; and conſider in the movent, rather the ſituation of the conſpicuous, and more bulky part of the agent, than the ſituation of that part of it which immediately impreſſes the motion upon the mobile.

Whoever attentively regards this, may eaſily obſerve, that ſome part of the body, or of the inſtrument, which, by reaſon of their conjunction in this operation, is to be look'd on as but making one with it, is really placed behind ſome part of the body to be drawn; and therefore cannot move onwards itſelf, without thruſting the body forward. Thus when a man draws a chain after him, tho' his body precede the chain, yet his finger, or ſome other part of the hand, wherewith he draws it, has ſome part or other which reaches behind the fore-part of the firſt link; and the hinder part of this link comes behind the firſt part of the ſecond link; and ſo each link has one of its parts placed behind ſome part of the next link after it, till you come to the laſt link of all. And ſo as the finger that is in the firſt link, cannot move forwards but it muſt thruſt on that link, by this ſeries of truſions the whole chain is thruſt forwards; and if any other body be drawn by that chain, you may perceive ſome part of the laſt link comes behind ſome part of that body, or of ſome intervening body, which, by its coheſion with it, ought, in our preſent caſe, to be conſidered as part of it. And thus attraction ſeems to be but a ſpecies of pulſion, and uſually belongs to that kind of it, which, for diſtinction ſake, is called truſion; whereby we underſtand that kind of pulſion, wherein the movent goes along with the moved body, without quitting it, whilſt the progreſs laſts; as happens in propelling a wheel-barrow, without letting go the hold of it.

But it may be ſaid, there are attractions where it cannot be pretended that any part of the attrahent comes behind the attracted body; as in magnetical and electrical attractions; and in that which is made of water, when drawn up into ſprings and pumps. Now the *Carteſians*, and other modern philoſophers, have recourſe either to ſcrew'd particles, and other magnetical emiſſions, to ſolve phenomena of this kind; and according to ſuch hypotheſes, we may ſay, that many of theſe magnetical and electrical effluvia, come behind ſome parts of the attracted bodies; or at leaſt of the little ſolid particles, that are, as it were, the walls of their pores; or procure ſome diſcuſſion of the air, that may make it thruſt the moveable towards the load-ſtone, amber, *&c.* But if there were none of theſe, nor any other ſubtile agents, to cauſe this motion, by a real, tho' unperceived pulſion, I ſhould make a diſtinction betwixt other attractions, and theſe which I ſhould then call attractions by inviſibles. However, in raiſing water into the barrel of a ſyringe, there is no true attraction made of the liquor. For by the aſcending rammer, as a part of which I here conſider the obtuſe end, plug, or ſucker, there is no attraction made of the contiguous and ſubjacent water, but only room made for it to riſe into, without being expoſed to the preſſure of the ſuperior air. For if we ſuppoſe the whole rammer to be annihilated, and conſequently incapable of exerciſing any attraction; yet, provided the ſuperior air were kept off from the water, by any other way, as well as it was by the rammer, the liquor would as well aſcend into the cavity of the barrel; ſince the ſurface of the terraqueous globe, being continually preſs'd on by the incumbent atmoſphere, the water muſt, by that preſſure, be impell'd into any cavity, here below, where there is no air to reſiſt it; as, by ſuppoſition, there is not, in the barrel of our ſyringe, when the rammer was annihilated. Thus, if the external air, and conſequently, its preſſure, be, by the air-pump, withdrawn from about the ſyringe, you may raiſe the ſucker as much as you pleaſe; but none of the ſubjacent water will follow. In ſhort, let us ſuppoſe, that a man, ſtanding in an inner-room, does, by his utmoſt reſiſtance, keep ſhut a door, that is neither lock'd, nor latch'd, againſt another, who, with equal force, endeavours to thruſt it open; in this caſe, if one ſhould forcibly pull away the firſt man, it could not be ſaid, that he, by his receſs from the door he endeavoua'd to preſs outward, did truly and properly draw in his antagoniſt, tho', upon that receſs, the coming in of his antagoniſt, would preſently enſue; ſo it cannot, properly, be ſaid, that by the aſcent of the rammer, which diſplaces the ſuperior air, either the rammer itſelf, or the expell'd air, properly attracts the ſubjacent water, tho' the ingreſs of that liquor, into the barrel, thereupon neceſſarily enſues. And that, as the compariſon ſuppoſes, there is a preſſure of the ſuperior air againſt the upper-part of the ſucker, may be eaſily perceiv'd, if, having well ſtopp'd the lower orifice of the ſyringe with your finger, you forcibly draw up the ſucker to the top of the barrel; for, if then you let go the rammer, you will find it impell'd downwards, by the incumbent air, with a conſiderable force.

Having thus premis'd ſomething, in general, about the nature of attraction, as far as it is neceſſary for my preſent deſign, we may proceed to the conſideration of that kind of it, employ'd to raiſe liquors, which is, by a diſtinct name, called ſuction.

The cauſe of ſuction inquired into.

About the cauſe of this there is great contention, between the modern philoſophers, and the *Peripatetics*. The former aſcribe the aſcent of liquors, upon ſuction, to nature's abhorrence of a vacuum: for, ſay they, when a man dips one end of a ſtraw, or reed, into ſtagnant water, and ſucks at the other, the air contained in the cavity of the reed, paſſes into that of his cheſt; and, conſequently, the reed would be left empty, if no other body ſucceeded in the place it deſerts: but there are only two bodies that can ſucceed, the air, and the water; now the air cannot do it, becauſe of the interpoſition of the water, that denies it acceſs to the immerſed orifice of the reed; and, therefore, it muſt be the water itſelf, which, accordingly, aſcends to prevent a vacuum, abhorr'd by nature.

But many of the modern philoſophers look upon this *Fuga vacui* as an imaginary cauſe of ſuction. The atomiſts, who willingly admit vacuities, properly ſo called, both within our world, and out of it, cannot think that nature dreads a vacuum, and declines her uſual courſe to prevent it. And the *Carteſians*, tho' they, as well as the *Peripatetics*, deny a vacuum, yet, ſince they affirm, not only that there is none in nature, but that there can be none, they will not grant nature to be ſo indiſcreet, as to ſtrain her ſelf to prevent the making of a thing that is impoſſible to be made.

The *Peripatetic* opinion, about the cauſe of ſuction, though commonly defended by the ſchools, as well modern as ancient, ſuppoſes in nature ſuch an abhorrence of a vacuum, as neither has been well prov'd, nor agrees with the late phenomenon of ſuction: ſince, according to their hypotheſis, water, and other liquors, ſhould aſcend, upon ſuction, to any height, to prevent a vacuum, which yet is not agreeable to experience. For, I have carefully tried, that by working a pump, far more ſtanch than thoſe that are uſually made, and, indeed, as well clos'd as we could poſſibly bring it to be, we could not, by all our endeavours, raiſe water by ſuction to above 33 ½ feet. The *Torricellian* experiment ſhews, that the weight of the air is able to ſuſtain, and ſome of our experiments ſhew, 'tis able to raiſe a mercurial cylinder, equal in weight, to as high a cylinder of water, as we were able to raiſe by pumping. For, mercury being near 14 times as heavy as water, of the ſame bulk, if the weight of the air be equivalent to that of a mercurial cylinder of 29, or 30 inches, it muſt be able to counterpoiſe a cylinder of water, near 14 times as long; that is, from 34, to 36 feet. And very diſagreeable to the common hypotheſis, but conſonant to ours, is the experiment that I have, more than once, made, by taking a glaſs pipe, about three feet long, dipping one end of it in water, and ſucking at the other, whereby the water will be, ſuddenly, made to flow briſkly into one's mouth; when if, inſtead of water, you dip the lower-end into quick-ſilver, though you ſuck as ſtrongly as ever you can, provided, in this caſe, as in the former, you hold the pipe upright, you will never be able to raiſe the quick-ſilver near ſo high as your mouth: ſo that, if the water aſcended, upon ſuction, to the top of the ſame pipe, becauſe there would, otherwiſe, have been a vacuum left in the cavity of it; why ſhould we not conclude, that when we have ſuck'd up the quick-ſilver as ſtrongly as we can, ſo much of the upper-part of the tube, as is deſerted by the air, and yet not fill'd by the mercury, admits, in part at leaſt, a vacuum, as to air; of which, conſequently, nature cannot reaſonably be ſuppoſed to have ſo great and unlimited an abhorrence, as the *Peripatetics* preſume. Yet, I will not determine whether there be any more than many little vacuities, or ſpaces deſtitute of air, in the cavity of the pipe, unfill'd by the mercury, ſo that the whole cavity is not one entire empty ſpace; it being ſufficient for my purpoſe, that the experiment affords a good argument againſt the *Peripatetics*; and warrants us to ſeek for ſome other cauſe, than a *Fuga vacui*, why a much ſtronger ſuction, than that which made the water aſcend with eaſe into the mouth, will not alſo raiſe quick-ſilver to near the ſame height.

Thoſe modern philoſophers, who admit not a *Fuga vacui* to be the cauſe of the aſcent of liquors in ſuction, generally agree in referring it to the action of the thorax: for when a man endeavours to ſuck up a liquor, he, by means of the muſcles, enlarges the cavity of his cheſt; which he cannot do, but, at the ſame time, he muſt thruſt away thoſe parts of the ambient air, that were contiguous to his cheſt; when the diſplaced air, according to ſome, compreſſes the contiguous air, and that, the next to it, and ſo onwards, till the preſſure, ſucceſſively paſſing from one part of the air to the other, arrive at the ſurface of the liquor; ſo that all other places being, as to ſenſe, full, the impell'd air cannot find place, but by thruſting the water into the room made for it in the pipe, by the receſs of the air, that paſs'd into the lungs of the perſon. And they differ not much from this explanation, who, without taking in the compreſſion of the ambient air, made by the thorax, refer the phenomenon to the propagated motion, or impulſe, impreſs'd on the air, diſplac'd by the thorax, in its dilatation, and yet unable to move in a world perfectly fill'd, as they ſuppoſe ours to be, unleſs the liquor be impell'd as much into the cavity of the pipe, as faſt as 'tis deſerted by the air, ſaid to be ſuck'd up. But, tho' I readily confeſs this explanation to be ingenious, and ſuch as I wonder not they ſhould acquieſce in, who are only acquainted with the obvious phenomena of ſuction; and tho' I am not ſure, that in the moſt familiar caſes, the cauſes aſſign'd by them may not contribute to the effect, yet I cannot acquieſce in this theory: for, I think the cauſe of ſuction aſſign'd, is, in many caſes, not neceſſary; in others, not ſufficient. And, firſt, as to the condenſation of the air by the dilatation of the cheſt; when I conſider the extent of the ambient air, and how ſmall a compreſſion, no greater an expanſion than that of the thorax is likely to make; I can ſcarce think ſo ſlight a condenſation of the free air, ſhould have ſo conſiderable an effect on the ſurface of the liquor, to be rais'd, as the hypotheſis requires: and, that this impulſe of the air, by the dilated thorax, though it accompanies the aſcent of the water, procured by ſuction, is not of abſolute neceſſity thereto, will, I preſume, be eaſily granted: even a propagated pulſion, abſtracted from any condenſation of the air, is not ſo neceſſary, but that the effect may be produc'd without it. Now, ſuppoſe ſo much air as is diſplaced by the thorax, annihilated, yet the aſcent of the liquor would ſtill enſue; for, when a man begins to ſuck, there is an equilibrium between the preſſure which the air contain'd in the pipe has, by virtue of its ſpring, upon that part of the ſurface of the water, that is ſurrounded by the ſides of the pipe, and the preſſure which the atmoſphere has, by virtue of its weight, upon all the reſt of the ſurface of the ſtagnant water: ſo that when, by the dilatation of the thorax, the air, within the pipe, comes to be rarified, and, conſequently, to loſe of its ſpring, the weight of the external air continuing, in the mean time, the ſame; it muſt neceſſarily happen, that the ſpring of the internal air will be too weak to balance, any longer, the gravitation of the external; and, conſequently, that part of the ſurface of the ſtagnant water, which is included in the pipe, being leſs preſs'd upon than all the other parts of the ſame ſurface, muſt neceſſarily give way, and, therefore, be impell'd up into the pipe, where the air, having had its ſpring weaken'd, is no longer able to reſiſt it, as before. Thus, conceiving that, within a chamber, three men thruſt all together, with their utmoſt force, againſt a door, to keep it ſhut, at the ſame time that three others, of equal ſtrength, endeavour to thruſt it open; though, whilſt their oppoſite endeavours are equal, the door will continue ſhut, yet if one of the three men, within the room, ſhould ceaſe to act, there would need no new force, in the three men, to make them prevail, and thruſt open the door, againſt the reſiſtance of the two.

The long neck of a glaſs bubble being ſeal'd up, and, almoſt, all the air, by heat, driven out of the whole cavity thereof, the glaſs was laid aſide for ſome hours; afterwards the ſeal'd apex of the neck was broken off under water; I demand, now, of a *Peripatetic*, whether the liquor ought to be ſuck'd, or drawn into the cavity of the glaſs; and why? If he ſays, as, queſtionleſs, he will, that the water wou'd be attracted to hinder a vacuum, he, thereby, acknowledges, that, till the glaſs was unſtopp'd, under water, there remain'd ſome empty ſpace in it: for, 'till the ſeal'd end was broken off, the water cou'd not get in; and therefore, if the *Fuga vacui* had any thing to do in the aſcent, the liquor muſt riſe, not to prevent an empty ſpace, but to fill one that was made before. Nor does our experiment much more favour the other philoſophers, I diſſent from: for there is here no dilatation made of the ſides of the glaſs, as in ordinary ſuction there is made of the thorax; but only ſo much air driven out of the cavity of the bubble, into whoſe room, ſince neither common air, nor water, is permitted to ſucceed, it appears not how the propagated, and returning impulſe, or the circle of motion, as to common air and water, takes place. Again, I demand, what becomes of the air, that has been, by heat, driven out, and is, by the hermetical ſeal, kept out of the cavity of the bubble? If it be ſaid, that it diffuſes itſelf into the ambient air, and mixes with it; this is to grant, what I contend for, that ſo little air, as is uſually diſplac'd in ſuction, cannot make any conſiderable compreſſion of the free ambient air: for, what can one cubic inch of air, which is, ſometimes, more than one of our glaſſes contains, do, towards condenſing a whole chamber full, when the expell'd corpuſcles are evenly diſtributed among thoſe of the ambient? And how comes this inconſiderable condenſation to have ſo great an effect in every part of the room, as to be able there to impel into the glaſs, as much water, in extent, as the whole air that was driven out of the cavity of it? But, if it be ſaid, that the expell'd air only condens'd the contiguous air; 'tis no way probable that the expell'd particles of the air ſhould not, by the different motions of the ambient air, be quickly made to mix with it; but ſhould rather wait till the veſſels, whence 'twas driven out, were unſtopp'd again. But though this could, probably, be pretended, it cannot, truly, be aſſerted: for if you carry the ſeal'd glaſs quite out of the room, and unſtop it at ſome other place, tho' two or three miles diſtant, the aſcent of the water will, as I found by trial, nevertheleſs, enſue; in which caſe, I preſume, it cannot be ſaid, that the air expell'd out of the glaſs, and which condens'd the contiguous air, attended the bubble in all its motions; and was ready at hand, to impel in the water, as ſoon as the ſeal'd apex of the vial was broken off.

In our own hypotheſis of ſuction, we ſuppoſe firſt, without diſputing either the exiſtence or the nature of elementary air, that the common air we breathe in, and which I call atmoſpherical, abounds with corpuſcles not deſtitute of weight, and endow'd with elaſticity, whereby the lower parts, compreſſed by the weight of the upper, inceſſantly endeavour to expand themſelves; by which expanſion, and in proportion to it, the ſpring of the air is weaken'd, the more they are permitted to ſtretch themſelves.

Next we ſuppoſe, that the terraqueous globe, being ſurrounded with this gravitating and ſpringy air, has its ſurface, and the bodies plac'd on it, preſſed by as much of the atmoſphere, as either perpendicularly reſts on them, or can otherwiſe come to bear upon them. This preſſure is, by the *Torricellian*, and other experiments, found to be equivalent to a perpendicular cylinder of about 29 or 30 inches of quick-ſilver.

Laſtly, we ſuppoſe, that air being contained in a pipe, or other hollow body, having but one orifice open to the free air, if this orifice be hermetically ſeal'd, or otherwiſe clos'd, the included air, whilſt it continues without any farther expanſion, will have an elaſticity equivalent to the weight of as much of the outward air as did before preſs againſt it. For if the weight of the atmoſphere had been able to compreſs it further, it would have done it; and then the cloſing of the orifice, at which the internal and external air communicated, as it fenced the included air from the preſſure of the incumbent, ſo it hinder'd the ſame included air from expanding itſelf; whence, as it was ſhut up with the preſſure of the atmoſphere upon it, that is, in a ſtate of as great compreſſion as the weight of the atmoſphere could bring it to; ſo being ſhut up, and thereby kept from weakening that preſſure by expanſion, it muſt retain a ſpringineſs proportionable to the preſſure it was before expoſed to. But if, as was ſaid in the firſt ſuppoſition, the included air ſhould come to be dilated or expanded, its ſpring, like that of other elaſtic bodies, would be weaken'd, anſwerably to that expanſion.

To me then it ſeems in general, that liquors are, upon ſuction, raiſed into the cavities of pipes, and other hollow bodies, when there is a leſs preſſure on the ſurface of the liquor in the cavity, than on the ſurface of the external liquor that ſurrounds it; whether that preſſure of thoſe parts of the external liquor, which are from time to time impelled up into the orifice of the pipe, proceed from the weight of the atmoſphere, the propagated compreſſure or impulſe of ſome parts of the air, the ſpring of the air, or ſome other cauſe; as the preſſure of bodies quite diſtinct from air.

Upon a general view of this hypotheſis, it ſeems very agreeable to mechanical principles. For if there be, on the different parts of the ſurface of a fluid body unequal preſſures, it is plain, as well by the nature of the thing, as by what has been demonſtrated by *Archimedes*, that the greater force will prevail againſt the leſs; and that ſuch a part of the water's ſurface muſt give way, as is the leaſt preſſed.

To proceed to ſome experiments made in favour of this hyyotheſis: We took a glaſs pipe bended like a ſiphon, but ſo, that the ſhorter leg was parallel to the longer, and hermetically ſeal'd at the end; into this ſiphon we conveyed water, ſo that the crooked part being held downwards, the liquor reach'd to the ſame height in both legs; about an inch and an half of uncompreſſed air being ſhut up in the ſhorter. This little inſtrument, about 15 inches long, being thus prepared, it is plain, that according to the hypotheſis I diſſent from, there is no reaſon why the water ſhould aſcend upon ſuction. For tho' we ſhould admit that the external air were conſiderably compreſſed, or received a notable impulſe when the cheſt is enlarged, yet in our caſe, that compreſſion or protruſion will not reach the ſurface of the water in the ſhorter leg; becauſe it is there fenced from the action of the external air by the ſides of the glaſs, and the hermetical ſeal at the top; but if a perſon ſuck'd ſtrongly at the open orifice in the longer leg, the water in the ſhorter would be depreſſed, and that in the longer aſcend, at one ſuck, about an inch and a half; of which the reaſon is clear on our hypotheſis. For the ſpring of the included air, together with the weight of the water in the ſhorter leg, and the preſſure of the atmoſpherical air, aſſiſted by the weight of the liquor in the longer leg, balanced one another before the ſuction began; but when afterwards, upon ſuction, the air in the longer leg came to be dilated, and thereby weakened, it was render'd unable to reſiſt the undiminiſh'd preſſure of the air included in the ſhorter leg; which conſequently expanding itſelf, by virtue of its elaſticity, depreſſed the contiguous water, and made it proportionably riſe in the oppoſite leg, till its ſpring being by the expanſion gradually more weakened, it balanced the gravitation or preſſure of the atmoſphere. And this is the reaſon why, when the perſon who ſuck'd had raiſed the water in the longer leg leſs than 3 inches higher, by repeated endeavours, and that without once ſuffering the water to fall back again, he was not able to elevate the water in the longer ſo much as 3 inches above its firſt ſtation. And if in the ſhorter leg, there was only an inch and a quarter of ſpace left for the air; by ſeveral acts of ſuction, ſkilfully repeated, he could not raiſe the liquor in the longer leg above two inches; becauſe by that time, the air included in the ſhorter leg had, by expanding itſelf further, proportionably weakened its ſpring, till at length it became as much rarified as the air in the cavity of the longer leg; and conſequently was able to thruſt away the water with no more force than the air in the longer leg was able to reſiſt. And hence it appeared, that the rarifaction uſually made of air by ſuction, is not near ſo great as one would expect; probably, becauſe by the dilatation of the lungs, the air being ſtill ſhut up, is but moderately rarified; and that in the longer leg can by them be brought to no greater degree of rarity than the air within the cheſt. For whereas the included air in our inſtrument was not expanded, at one ſuck, to above double its former dimenſions, and by ſeveral ſucceſſive ſucks was expanded, but from one inch and a half to leſs than 4 inches and a half, if the ſuction could have been conveniently made with a great ſtanch ſyringe, the rarifaction of the air would probably have been far greater; ſince, in our pneumatic engine, air may without heat, and by a kind of ſuction, be brought to poſſeſs many hundreds of times the ſpace it took up before. From this rarifaction of the air in both the legs of our inſtrument, proceeds another phenomenon readily explicable by our hypotheſis. For if, when the water was impelled up as high as the ſuction could raiſe it, the inſtrument were taken from the perſon's mouth, the elevated water would, with violence, return to its wonted ſtation. For the air, in both legs of the inſtrument, having by ſuction loſt much of its ſpring, and conſequently of its power of preſſing ; when once the orifice of the longer leg was left open,the atmoſpherical air came again to gravitate upon the water in that leg ; and the air included in the other, having its ſpring weakened by the preceding expanſion, was not able to hinder the external air from violently repelling the elevated water, till the included air was thruſt into the ſpace it poſſeſſed before the ſuction ; in which ſpace it had denſity and elaſticity enough to reſiſt the preſſure that the external air exerciſed againſt it, thro' the interpoſed water.

But our hypotheſis about the cauſe of ſuction, needs only the aſſiſtance of our pneumatic engine. For by trials, purpoſely deviſed and carefully made therewith, we found, that a good ſyringe being ſo conveyed into the receiver, that the open orifice of the pipe, or lower part, remained under water ; if the engine were exhauſted, and the handle of the ſyringe drawn up, the water did not follow it, which yet it would do if the external air were let in again. The reaſon of which is plain ; for the air that ſhould have preſſed upon the ſurface of the ſtagnant water having been pumped out, there was nothing to impel up the water into the deſerted cavity of the ſyringe, till the receiver was filled with air.

I ſhall next offer ſome eaſy experiments to make out theſe three propoſitions.

1. That a liquor may be raiſed by ſuction, when the preſſure of the air, neither as it has weight nor elaſticity, is the cauſe of it.
2. That the weight of the atmoſphere is ſufficient to raiſe up liquors in ſuction.
3. That in ſome caſes there will be no ſuction, tho' there is a dilatation of the thorax ; and no danger of a *vacuum* if the liquor ſhould aſcend.

Suction may raiſe a fluid without the preſſure or elaſticity of the air.

1. And firſt to ſhew how much the riſing of liquors, in ſuction, depends upon the weight or preſſure of the impelling body ; and how little neceſſity there is, where ſuch a preſſure happens, that in the place deſerted by the liquor ſuck'd, there ſhould ſucceed air, or ſome other viſible body ; I deviſed the following experiments. We took a glaſs pipe, fit for the *Torricellian* experiment, but much longer than was neceſſary for that uſe ; this being hermetically ſealed at one end, the other was ſo bent as to be reflected upwards, and make, as it were, the ſhorter leg of a ſiphon parallel to the longer ; ſo that the tube was now ſhaped like an inverted ſiphon, with legs of a very unequal length. This we filled with mercury in an inclining poſture, and then erecting it, the mercury ſubſided in the longer leg, as in the *Torricellian* experiment, and attained to between two feet and a quarte, and two feet and a half, above the ſurface of the mercury in the ſhorter

leg, which in this inſtrument anſwers to the ſtagnant mercury in an ordinary barometer. Out of the ſhorter leg of this tube, we warily took as much mercury as was thought convenient; and this we did ſo as to hinder any air from getting into the deſerted cavity of the longer leg; by which means the mercurial cylinder retained the ſame height above the ſtagnant mercury in the ſhorter. The upper and cloſed part of this portable barometer muſt have been free from common air, becauſe, if gently inclined, the quick-ſilver would aſcend to the top of the tube; which it could not do, if the place formerly deſerted by it were poſſeſſed by the air. The inſtrument being thus fitted, I cauſed a perſon to ſuck at the ſhorter leg of it; whereupon there preſently enſued an aſcent of 4 or 5 inches of mercury in that leg, and a proportionable ſubſidence in the longer; yet in this caſe the riſe of the mercury cannot proceed from the preſſure of the air. For the weight of the atmoſphere is fenced off, by that which cloſes the upper end of the longer tube; and the ſpring of the air has here nothing to do, ſince the ſpace deſerted by the mercury is not poſſeſſed by the included air; and the pulſion or condenſation of the air, ſuppoſed by ſeveral modern philoſophers to be made by the dilatation of the cheſt, and to preſs upon the ſurface of the liquors that are to be ſuck'd up, cannot here be pretended; becauſe the ſurface of the liquor in the longer leg is every way fenced from the preſſure of the ambient air. It remains therefore, that the cauſe which raiſed the quick-ſilver in the ſhorter leg, upon the ſuction, was the weight of the collateral quick-ſilver, ſuperior in the longer leg; which being equivalent to the weight of the atmoſphere, there is a plain reaſon why the ſtagnant mercury in the ſhorter leg ſhould be raiſed ſome inches by ſuction, as mercury ſtagnant in an open veſſel will be raiſed by the weight of the atmoſphere, when the ſuction is made in the open air; for in both caſes there is a pipe that reaches to the ſtagnant mercury, and a competent weight to impel it into that pipe, when the air in the cavity of it has its ſpring weakened by the dilatation that accompanies ſuction.

The weight of the atmoſphere may alone raiſe liquors in ſuction.

2. That the weight of the air is ſufficient to raiſe liquors in ſuction, may appear by arguments drawn from the *Torricellian* experiment; and much more clearly from ſome we have made with our air-pump. And with the like view, having provided an inſtrument in imitation of the portable barometer, lately mention'd, but whoſe legs were not ſo unequally long; and having in it made the *Torricellian* experiment, after the manner deſcribed, we ordered the matter ſo, that there remained in the ſhorter leg the length of ſeveral inches unfill'd with ſtagnant mercury. Then I cauſed a perſon to raiſe the quick-ſilver, by ſuction, ſo to the orifice of the ſhorter leg, that the orifice being ſeaſonably and dextrouſly cloſed, the mercury continu'd to fill that leg as long as we thought fit; and then having put a mark at the ſurface of the mercury in the longer leg, we unſtopt the orifice of the ſhorter; whereupon the mercury that before filled it, was depreſſed, till in the longer leg it was raiſed 5 inches or more above the mark, and continu'd at that height. This mercury, rais'd by ſuction, was depreſs'd, becauſe its

own weight could not here make it fall; ſince a mercurial cylinder of five inches was far from being able to raiſe ſo tall a cylinder of mercury, as made a counterpoize in the longer leg; and therefore, the depreſſion we ſpeak of, is to be referr'd to the gravitation of the atmoſpherical air upon the ſurface of the mercury in the ſhorter leg. And I ſee no cauſe to doubt, that if we could have procur'd an inſtrument, into whoſe ſhorter leg, a mercurial cylinder many inches higher might have been ſuck'd up, it would, by this contrivance, have appear'd, that the preſſure of the atmoſphere could eaſily impel up a far taller cylinder of mercury than it did in our experiment. For, if the gravity of an incumbent pillar of the atmoſphere be able to compreſs a parcel of included air as much as a mercurial cylinder equivalent, in weight, to between thirty and thirty-five feet of water is able to condenſe it, the ſame atmoſpherical cylinder may well be able, by its weight, to raiſe and counterbalance twenty-eight or twenty-nine inches of quick-ſilver, or an equivalent pillar of water in tubes, where the reſiſtance of theſe two fluids, to be rais'd and ſuſtain'd by the air, depends only upon their own unaſſiſted gravity.

To confirm our doctrine of the gravitation of the atmoſphere, upon the ſurface of the liquors expos'd to it, I ſhall ſubjoin an experiment, devis'd to ſhew, that the incumbent air, in its uſual ſtate, would compreſs other air in the like natural ſtate, as much as a cylinder of twenty-eight or twenty-nine inches of mercury could condenſe it. But in order hereto, I ſuppoſe it known, that about twenty-nine or thirty inches of quick-ſilver will compreſs air ſhut up, in its uſual ſtate, in the ſhorter leg of our portable barometer into half the ſpace it poſſeſs'd before.

We provided a portable barometer, wherein the mercury in the longer leg was kept ſuſpended by the counterpoize of the air that gravitated on the ſurface of the mercury in the ſhorter, which, we had ſo order'd, that it reach'd not, by about two inches, to the top of the ſhorter leg. Then making a mark at the place where the ſtagnant mercury reſted, 'twas manifeſt, according to our hypotheſis, that the air in the upper part of the ſhorter leg was in its natural ſtate, or of the ſame degree of denſity with the outward air, with which it freely communicated at the open orifice of the ſhorter leg; ſo that this ſtagnant air was equally preſs'd upon by the weight of the collateral ſuperior cylinder of mercury in the longer leg, and the equivalent weight of a directly incumbent pillar of the atmoſphere. Then the upper part of the ſhorter leg that had been before, purpoſely, drawn out to almoſt a capillary ſmallneſs, was hermetically ſeal'd; which, tho' the inſtrument remain'd erect, was ſo ſuddenly done, by reaſon of the ſlenderneſs of the pipe, that the included air did not appear to be ſenſibly heated: after this, we open'd the lower end of the longer leg without ſhaking the veſſel; by which means, the atmoſpherical air, gaining acceſs to the mercury included in the longer leg, did, by its gravitation upon it, ſo compreſs the air included in the ſhorter leg, that, according to the eſtimate we made, with the help of a ruler, it was crowded into near half the ſpace it took up before; and conſequently ſuffer'd a compreſſure like

that,

that, which a mercurial cylinder of about twenty-nine inches would have given it.

This experiment was made a ſecond time, with like ſucceſs.

That the aſcent of liquors, by ſuction, depends upon preſſure.

3. And to make it yet further appear, how much the aſcent of liquors, by ſuction, depends upon preſſure, rather than upon nature's imaginary abhorrence of a *Vacuum*, or the propagated pulſion of the air; I will ſubjoin an inſtance, wherein that preſum'd abhorrence cannot be pretended. The experiment was thus made.

A glaſs ſiphon, like thoſe lately deſcrib'd, with one leg far longer than the other, was hermetically ſeal'd at the ſhorter leg, and then, by degrees, we put in, at the orifice of the longer leg, as much quick-ſilver, as by its weight ſufficed to compreſs the air in the ſhorter leg, into about half the ſpace it poſſeſs'd before; ſo that, according to the *Peripatetic* doctrine, the air muſt be in a ſtate of preternatural condenſation, and that to a far greater degree, than 'tis uſually brought by cold intenſe enough to freeze water. Then, meaſuring the height of the quick-ſilver in the longer tube above the ſuperficies of that in the ſhorter, we found it not to exceed thirty inches. Now if liquors roſe, in ſuction, for fear of a *Vacuum*, there is no reaſon why this quick-ſilver, in the longer part of the ſiphon, ſhould not eaſily aſcend upon ſuction, at leaſt till the air in the ſhorter leg had regain'd its former dimenſions; ſince it cannot, in this caſe, be pretended, that if the mercury ſhould aſcend, there would be any danger of a *Vacuum* in the ſhorter leg of the tube; becauſe the contiguous included air is ready at hand to ſucceed, as faſt as the mercury ſubſides in the ſhorter leg of the ſiphon. Nor can it be alledg'd, that, to fill the place deſerted by the quick-ſilver, the included air muſt ſuffer a preternatural rarifaction; ſince 'tis plain, that, on the contrary, as long as the air continues in the ſtate whereto 'tis reduced by the weight of the quick-ſilver, it is kept in a violent ſtate of compreſſion, becauſe in the ſhorter leg it was in its natural ſtate, when the mercury pour'd into the longer leg did, by its weight, thruſt it into about half the ſpace it took up before. Yet having caus'd ſeveral perſons to ſuck, ſeveral times, as ſtrongly as they could, they were not able, ſo much as for a minute, to raiſe the mercury in the longer, and make it ſubſide in the ſhorter far more than an inch. And to ſhew that the experiment was not favourably made for me; the height of the mercurial cylinder in the longer leg, above the ſurface of that in the ſhorter, was at the time of ſuction, an inch or two ſhort of thirty; and the compreſs'd air in the ſhorter leg was ſo far from having been expanded, by the exſuction, beyond its natural and firſt dimenſions, that it did not, when the contiguous mercury ſtood as low as we could make it ſubſide, regain ſo much as one half of the ſpace it had loſt by the precedent compreſſure; and, conſequently, was in a preternatural ſtate of condenſation, when it had been freed from that ſtate as far as ſuction could do it. Whence it ſeems evident, that it was not *ob fugam vacui*, that the quick-ſilver did, upon ſuction, aſcend one inch; for upon the ſame ſcore it ought to have aſcended 2, or perhaps more inches; ſince there was no danger, that by ſuch an aſcent, any *vacuum* ſhould be produced, or left in the ſhorter leg of the ſiphon. But according to our hypotheſis, a clear cauſe of the phenomenon is aſſignable; for before the ſuction began, there was an equilibrium between the weight of the ſuperior quick-ſilver in the longer leg, and the ſpring of the compreſſed air, included in the ſhorter; but when the perſon began to ſuck, his cheſt being widened, ſome part of the air included in the upper part of the longer leg, paſſed into it, and what remained, had, by that expanſion, its preſſure ſo weakened, that the air in the ſhorter leg, finding no longer the former reſiſtance, was able, by its own ſpring, to expand itſelf, and conſequently to depreſs the contiguous mercury, in the ſame ſhorter leg, and raiſe it as much in the longer.

But here it may be objected, that if the compreſſed air in the ſhorter leg, had a ſpring equivalent to the weight of the mercury in the longer leg, why is not the mercury ſuck'd up in this inſtrument, as well as in the free air? ſince, according to me, the preſſure of the included air upon the ſubjacent mercury, muſt be equivalent to the weight of the atmoſphere? yet experience ſhews, that the weight of the atmoſphere will, upon ſuction, raiſe quick-ſilver to the height of ſeveral inches.

To clear this difficulty, and ſhew that it is not inſuperable, let us conſider, that I make indeed the ſpring of the compreſſed air to be equivalent to the weight of the compreſſing mercury; and I have a manifeſt reaſon to do it; becauſe if the ſpring of the air were not equivalent to that weight, the mercury muſt neceſſarily compreſs the air further; which it is granted, in fact, not to do. But then, in our caſe, there ought to be a great difference between the operation of the ſpring of the included air, and the weight of the atmoſphere, after ſuction has been once begun. For the weight of the atmoſphere, that impels up mercury, and other fluids, when ſuction is made in the open air, continues ſtill the ſame; but the force or preſſure of the included air, is equal to the counter-preſſure of the mercury, no longer than the firſt moment of ſuction; after which, the force of the impriſon'd air ſtill gradually decreaſes; ſince this compreſſed air, being more and more expanded, muſt needs have its ſpring proportionably weakened; ſo that it is no wonder that the mercury was not ſuck'd up any more than we have ſaid; for there was nothing to make it aſcend to a greater height, than that at which the weakened ſpring of the expanded air was brought to balance the undiminiſh'd, and indeed ſomewhat increaſed weight of the mercurial cylinder, in the longer leg; and the preſſure of the cylinder of air, in the ſame leg, leſſened by the action of him who ſuck'd. For when the orifice of this leg ſtood open, the mercury was preſſed upon by a cylinder of the atmoſpherical air, equivalent to about 30 inches of quick-ſilver; but by the mouth and action of him who ſuck'd, the tube was freed from the external air; and by the dilatation of his thorax, the neighbouring air, that had a free paſſage thro' his wind-pipe to it, was proportionably expanded, and had its ſpring and preſſure weakened; by which means the compreſſed air in the ſhorter leg of the ſiphon, was enabled to impel up the mercury, till the equilibrium was attained. And I muſt here take notice, that as the quick-ſilver was raiſed by ſuction but a little way, ſo the cylinder raiſed was a very long one; whereas, when the mercury is ſuck'd up in the free air, it is ſeldom raiſed to half that length; tho', as I noted before, the impellent cauſe, which is the weight of the atmoſphere, continued ſtill the ſame; but in our ſiphon, when the mercury was ſuck'd up only an inch, the compreſſed air, poſſeſſing double the ſpace it did before, had, by this expanſion, already loſt a very conſiderable part of its former ſpring and preſſure.

Among the more familiar phenomena of the air-pump, none leaves ſo great a ſcruple in the minds of ſome ſort of men, as that, when one's finger is laid cloſe upon the orifice of the little pipe, by which the air paſſes from the receiver into the exhauſted cylinder, the pulp of the finger is made to enter conſiderably into the cavity of the pipe; which doth not happen without a moderate ſenſe of pain in the lower part of the finger; for moſt of thoſe who are ſtrangers to hydroſtatics, perſuade themſelves that they feel this painful protuberance of the pulp of the finger, to be effected not by preſſure, but diſtinctly by attraction.

To this we anſwer, that common air being a body not deſtitute of weight, the phenomenon is clearly explicable by the preſſure of it; for when the finger is firſt laid upon the orifice of the pipe, no pain nor ſwelling is produced; becauſe the air which is in the pipe, preſſes as well againſt that part of the finger which covers the orifice, as the ambient air doth againſt the other parts of the ſame finger. But when, by pumping, the air in the pipe is made to paſs out of that into the exhauſted cylinder, there is nothing left in the pipe, whoſe preſſure can any thing near balance the undiminiſh'd preſſure of the external air on the other parts of the finger; and conſequently, that air thruſts the moſt yielding and fleſhy part of the finger into the place where its preſſure is unreſiſted; that is, into the cavity of the pipe, where this forcible intruſion cauſeth pain.

To illuſtrate this, we took a glaſs pipe, of a convenient length, open at both ends, whoſe cavity was near an inch in diameter. To one end of this pipe, we cauſed to be firmly ty'd, a piece of very fine bladder, that had been oil'd, to make it both very limber, and unapt to admit water; and care was taken, that the piece of bladder ty'd on, ſhould be large enough, not only to cover the orifice, but to hang looſe, ſomewhat beneath it. This done, we put the cover'd end of the pipe into a tall glaſs body; and the pipe being held ſo, that the end of it reach'd almoſt to the bottom of the glaſs, we cauſed water to be poured, both into this veſſel, and into the pipe, at its upper orifice, which was left open, that the water might aſcend equally, both without and within ſide of it. And when the glaſs body was full of water, and the ſame liquor level to it, or a little higher within the pipe, the bladder at the lower orifice was kept plump; becauſe the water, within the pipe, did, by its weight, preſs as forcibly downwards, as the external water, in the large glaſs, endeavoured to preſs it inwards and upwards. Then we caus'd part of the water in the pipe, to be taken out of it, by a piece of ſpunge, or by ſuction with a ſmaller pipe; upon which, the water remaining in the pipe, being no longer able, thro' want of weight, to preſs againſt the inſide of the bladder, near ſo forcibly as it did before; the external water, whoſe weight was not leſſen'd, preſs'd the ſides and bottom of the bladder, whereto it was contiguous, into the cavity of the pipe, and thruſt it up therein ſo ſtrongly, that the diſtended bladder made a kind of hemiſphere within the pipe. Here, then, we have a protuberance, like that abovementioned of the finger, effected by pulſion, not attraction; and, in a caſe where there can be no juſt pretence for having recourſe to nature's abhorrence of a vacuum; ſince the upper orifice of the pipe being left wide open, the air might, freely, paſs in and out.

The like ſwelling of the bladder, we could procure, without taking out any of the internal liquor, by plunging the pipe deeper into the water; for then the external liquor having by its increaſe of depth, a greater preſſure on the outſide of the bladder, than the internal liquor had on the the inſide of it, the bladder muſt yield to the ſtronger preſſure, and conſequently be impell'd up.

If the bladder, lying looſe at the lower-end of the pipe, the upper-end were carefully cloſed, that the air might not get out; and if the pipe, thus cloſed, were thruſt, almoſt, to the bottom of the water, the bladder would not be protuberant inwards, as formerly; becauſe the included air, by virtue of its ſpring, reſiſted, from within, the preſſure of the external water againſt the outſide of the bladder. But the upper orifice of the pipe being unſtopp'd, the air, before compreſs'd, having liberty to expand itſelf, and its elaſticity being weaken'd thereby; the external water would, ſuddenly, with noiſe, drive up the bladder into the cavity of the pipe, and there keep it very protuberant.

To obviate an objection, that might be brought, thro' want of skill in hydroſtatics, I caus'd ſuch a pipe, as the former, to be ſo bent, near the lower-end, that the orifice of it ſtood quite on one ſide, in a right angle. This lower-orifice being fitted with a bladder, and the pipe, with its contained liquor, being thruſt under water, after the former manner; the lateral preſſure of the water forced the bladder into the ſhort horizontal leg, and made it protuberant there, as it had done when the pipe was ſtreight.

Laſtly, that the experiment might not appear confined to one liquor; inſtead of water, we put into the ſtreight pipe, as much red-wine as was requiſite to keep the bladder bulging, when near the bottom of the water; and then ſaw the ſuperficies of the red liquor, in the pipe, was much higher than that of the external water: and, if the depth of both liquors, were proportionably leſſen'd, the difference of height betwixt the two ſurfaces, would, indeed, as it ought, decreaſe; but ſtill the ſurface of the wine would be the higher of the two; becauſe, being lighter in ſpecie, than common water, the equilibrium between the preſſures of the two liquors, upon the bladder, would not be maintained, unleſs a greater height of wine ſupply'd its want of ſpecific gravity. And, if the pipe were thruſt deeper into the water, the bladder would be made protuberant inwards, as when it contained water. 'Tis, therefore, evident, that theſe phenomena, without recourſe to attraction, may be explained barely from the equilibrium of liquors.

The End of the Second Volume.

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